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VOL. VIII.—PART I, 1946.

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Vol. VIII, No. 1, 1946

PROCEEDINGS OF THE FIRST MEETING OF THE RESEARCH SECTION OF THE ROYAL STATISTICAL SOCIETY, SESSION 1945–46. HELD ON TUESDAY, DECEMBER 4TH, 1945, DR. J. WISHART IN THE CHAIR.

THE CHAIRMAN, in opening the meeting, said that this was the inaugural meeting of a new Section. The former Industrial and Agricultural Research Section was founded in 1933, and held its last meeting in May 1939. On the outbreak of war it was clear that those who would be likely to contribute to its proceedings would be heavily engaged in war-time activities, and with some regret the proceedings were suspended. At the same time the *Supplement* to the Society's *Journal*, which had been the organ of the Industrial and Agricultural Research Section for the publication of its proceedings, was also suspended.

It had now been decided to form two Sections. The Research Section, of which this was the first meeting, would present papers on the theory of statistics and statistical methods, or on the development of new applications, and, in addition, an Industrial Applications Section had been formed which it was intended should operate on a regional basis in a number of groups, and would continue over a wider geographical field the series of discussions which had been taking place in London during the war. The publication of the *Supplement* would also be resumed. This would normally contain the papers and discussions at the Research Section meetings, of which it was hoped to have four in each session, but it would also be a medium for publishing general papers on statistics not read at the meetings, and it would contain selected papers and discussions coming from the Industrial Applications Section. It was fitting that at this inaugural meeting the subject chosen should be one which arose among groups of workers dealing with war problems on both sides of the Atlantic, and should be presented by a member of one of these groups.

The following paper was then read:

SEQUENTIAL TESTS IN INDUSTRIAL STATISTICS

By G. A. BARNARD, M.A.

SUMMARY

AFTER an introductory and an historical note, an elementary problem of simple qualitative inspection of a box of components is treated by using a " lattice diagram representation." This leads to the consideration of sequential tests for such cases. Procedures for determining " Target-Handicap " forms of inspection, and their operating and sample size properties are given.

This leads to a consideration of general linear sequential tests, which are those test procedures which can be formulated in terms of a " score." Such procedures are shown to be similar to classical games of chance, and to physical diffusion processes. The diffusion analogy leads to a differential equation which gives the approximate characteristics of any such linear test.

In many cases, Wald's " Probability Ratio Sequential Test " takes the form of a linear test. The conditions for this are determined. The P.R.S. test is seen to be " best possible linear test," in the sense of minimizing average sample size. The effects of deviations from normality, and general distributions are considered.

Reference is made to Wald's work on tests which involve parameters other than those being estimated, and then consideration is restricted to tests for the mean of normal populations where the variance is unknown. Methods of reducing such tests to simple binomial tests are indicated.

A number of procedures for use with 2×2 comparative trials, and double dichotomies, are given, and their properties discussed.

Returning to general inspection problems, the paper indicates that these are not always to be

identified with problems involving merely tests of statistical hypotheses. The notions of Consumer's Lot, Producer's Batch, the Lot Quality Curve, the Process Curve, are explained, and their importance indicated. A distinction is made between Acceptance Inspection schemes and Rectifying Inspection schemes, and the notions of Operating Characteristic Curve, Operating Characteristic Matrix, and the Sample Size distribution function are explained.

The lattice diagram is used to bring out relationships between notions involved in general inspection, and some other uses are also indicated.

Finally, some reflections on the relevance of the matters discussed to matters of current debate among statisticians are given.

CONTENTS

Introduction

THE distinction between sequential tests and what may be called classical tests is illustrated by various games of skill.

In football, and in chess championship matches, the duration of play is fixed in advance. In football, the game goes on for an hour and a half; in a chess championship the match goes on for twenty-one games. In tennis, on the other hand, the duration of play is not fixed in advance. A certain minimum number of points must be played, but after that the game ends whenever one player has gained at least two points over his opponent. And in bridge, the game goes to the pair who first score 100 points below the line.

Football and chess championships correspond to classical test procedures; tennis and bridge correspond to sequential test procedures. In classical procedures the number of observations to be made is fixed in advance of the experiment; in sequential procedures the results of the observations themselves are allowed to influence the number of observations made.

Sequential procedures are not always practicable. If we are experimenting with the growth of trees, for example, it may take years for a tree to grow to maturity; in such a case it would be absurd, in our present system of society, to grow one tree first, and see what happened, and then to plant another, and so on. On the other hand, if we are examining boxes of mass-produced components, we may in any case take components from the boxes one by one, or two by two. With the trees, a sequential procedure would be inapplicable; with the components, provided we can take them in random order, a sequential procedure would be not merely applicable, but in fact highly desirable.

When they are applicable, sequential procedures are desirable because they enable us to reduce the average amount of experimental labour involved in a test of given precision, very often by as much as 50 per cent.

Sequential procedures save time in two ways. First, they make us stop work just as soon as we have enough evidence on the points in question. And, second, they enable us to use judgment in arranging the experiment, and to use any plausible guesses we can make about the true results to minimize the work involved; these guesses may be wrong, but if they are, they affect only the amount of work we do, not the validity of our results.

Historical note

Wald's paper [1] contains a historical note on the origins of sequential ideas, and so my remarks need be only supplementary. As Wald points out, many authors have used sequential ideas in particular cases, without apparently realizing the general implications. His work, done in April 1943, seems to have been the first general attack, while mine was started in June of that year. Some of our results were published in reports whose circulation was restricted during the war. Beginning independently, we afterwards tried to keep in step with the American work, but we succeeded only partly, as was natural. In the following account many of the results overlap with Wald's; but the approach and methods used are different, in most cases.

Many people will find themselves in the position of M. Jourdain—they have, in a sense, been applying sequential methods all the time without knowing it. Engineering inspectors, untutored in the theory of statistical hypotheses, often judge batches of components by taking first a small

sample, and then another if the first is not decisive. And scientists often have their doubts about some theory gradually removed by a steady accumulation of evidence. Sequential theory only makes precise these common-sense procedures.

In particular, Dr. Case seems to have been using a sequential method for estimating a probability p for some time past, and a mathematical theory of his method has been worked out by Haldane.[2]

It should also be pointed out that many of the problems of mathematical probability which are involved in sequential procedures are of enormously long standing. The "Problem of Points," considered by Fermat and Pascal, is closely related to our work, and later historical references are so numerous that they are perhaps best covered by a reference to Isaac Todhunter's book,[3] which forms a good Baedeker for the early literature. Since Todhunter, too, many problems in physics have led to work which can be taken over into the theory of sequential tests. Mr. Tweedie's work [4] on "inverse variates" seems to have arisen in such a connection. And Khintchine's book [5] should also be mentioned.

I. A simple inspection problem—the inspection diagram

Suppose we inspect a box of components which we can classify simply into "effectives" and "defectives." We are interested in the fraction defective, p—the proportion of defective components to the total number. We assume that the number of components in the box is large

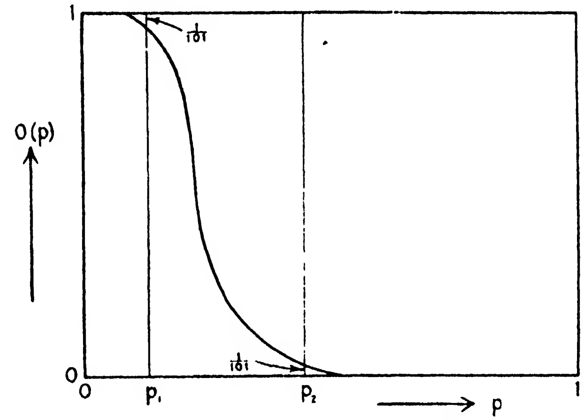

FIG. 1.—O.C. curve (not to scale).

compared with the number of components we actually examine, and that our sampling procedure is random, so that the probability that each component examined will be defective is p, and the probability that it will be effective is $q = 1 - p$.

We also assume that we are operating an "Acceptance Inspection Plan"—that is, an inspection plan in which we confine our activities to acceptance or rejection of the batch, and we do not take into account any possibility of improving the batch quality by replacing defective items by effective items. With this assumption, the properties of our plan in relation to the single batch we are now considering are specified by the Operating Characteristic Curve (O.C. curve), which gives the probability $O(p)$ of accepting the batch, as a function of p.

Any sensible inspection plan will always accept perfectly good batches ($p = 0$) and always reject perfectly bad batches ($p = 1$). In general our O.C. curve will be S-shaped (Fig. 1), with $O(p)$ decreasing as p increases from 0 to 1. And as a matter of practical experience it is found that most O.C. curves are sufficiently well represented by a straight line, if drawn on logarithmic probability paper using the logarithmic scale for p. Consequently, if we determine values p_1, p_2, such that

$$O(p_1) = 100/101 \quad \text{and} \quad O(p_2) = 1/101$$

we can obtain the rest of the O.C. curve, between these two points, by plotting on log probability paper and joining by a straight line. Although the approximation breaks down when p lies

outside the interval $p_1 < p < p_2$, we can, for practical purposes, regard the O.C. curve as being determined by these two values, p_1, p_2, called respectively the Producer's 100 to 1 Safe Point and the Producer's 100 to 1 Risk Point.

If the Risk and Safe points are fixed, the O.C. curve is practically fixed, and the problem of designing a good acceptance inspection scheme reduces to that of finding one scheme, out of all those "equivalent" schemes having the same Risk and Safe points, which will minimize inspection costs. And in this section we assume that inspection costs are measured solely by the number of items to be inspected, so that minimizing costs means minimizing the expected number of observations we make.

The inspection diagram

If we can inspect our components one by one, we can represent our results by the "random walk" diagram, or the "inspection diagram," as I shall call it (Fig. 2). Taking axes in the plane

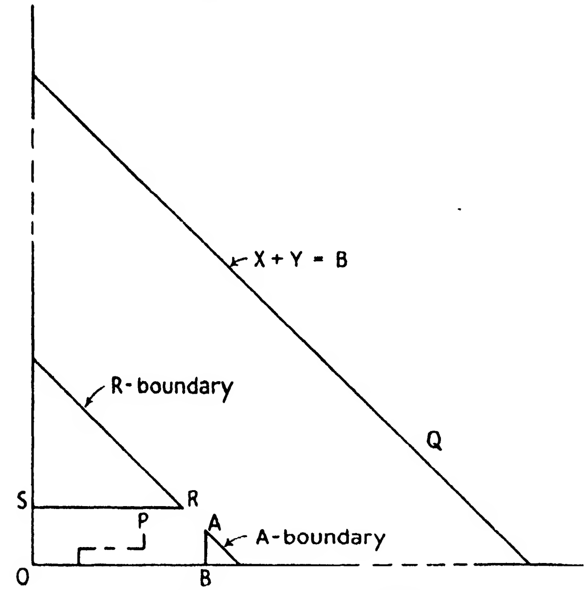

FIG. 2.—Inspection Diagram.

and starting at the origin, we move unit distance to the right each time we find an effective item and unit distance upwards each time we find a defective item. Then the inspection process is represented by a zig-zag line OP. The co-ordinates (x, y) of P give the summary result of the inspection up to this stage—we have found x effectives and y defectives in the total number $(x + y)$ inspected so far. As inspection proceeds, P moves outwards from the origin.

The adoption of any particular inspection plan will correspond to drawing two lines on the inspection diagram—the Acceptance Boundary and the Rejection Boundary. As soon as P reaches a point on the Acceptance Boundary, the batch is accepted, while as soon as P reaches the Rejection Boundary, the batch is rejected. The decision on the batch depends on which of these two possibilities occurs first. And the determination of the inspection plan reduces to the determination of the equations

$$y = A(x), \quad y = R(x) \qquad \ldots \ldots \ldots (1)$$

to the acceptance and rejection boundaries. The A and R boundaries, taken together, are called the inspection boundary.

Fundamental properties of the inspection diagram

One of the most useful features of the inspection diagram is that it enables us easily to distinguish between possibilities which are mutually exclusive, and possibilities which are not

mutually exclusive. Thus the calculation of the various probabilities associated with an inspection scheme is simplified.

In particular, the points on the inspection boundary represent mutually exclusive possibilities, while those points on the line

$$x + y = n$$

which lie between the A and R boundaries represent mutually exclusive and exhaustive possibilities at the nth stage of inspection, when the sample size is n.

A second feature of the inspection diagram is based on the fact that our assumption that the order of sampling is random is equivalent to the assumption that all paths from the origin O to any point P are equally likely—provided, of course, that the paths always move upwards or to the right, in accordance with the rules, and provided also that they are " permissible " paths, which do not cross the inspection boundary. Consequently the total probability, $Tpr(X)$, of reaching any point X within the inspection boundary is given by

$$Tpr(X) = N'(OX) \, . \, P(X) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where $N'(OX)$ = number of distinct permissible paths from O to X
and $P(X)$ = probability of reaching X from O by any one path.

Suppose, for example, that we have a large batch of fraction defective p, and we operate a " Single Sample Scheme," involving a single sample of 10 items, and we reject if there are 3 or more defectives in the sample. Then our inspection boundary will be as shown in Fig. 2, and the equations (1) will be

$$y = A(x) = 10 - x \qquad (8 \leqslant x \leqslant 10)$$
$$y = R(x) = 10 - x \qquad (0 \leqslant x \leqslant 7)$$

and

$$O(p) = N(O,(10,0)) \, . \, q^{10} + N(O,(9,1)) \, . \, q^9 p + N(O,(8,2)) \, . \, q^8 p^2$$

where $N(OX)$ represents the total number of paths (permissible or not) from O to X. In our case, for example,

$$N(O,(8,2)) = N'(O,(8,2)) = \frac{(8+2)!}{8\,!\,2\,!}$$

and we get the usual Bernouilli formula for $O(p)$.

A third feature of the inspection diagram emerges when we examine the " Single Sample Scheme " depicted on Fig. 2. Because, from the figure, it is apparent that as soon as P crosses the line RS we can be sure the batch will be rejected; and as soon as P crosses the line AB we can be sure it will be accepted. So we can replace the rejection boundary by the line RS, and the acceptance boundary by the line AB, without altering the O.C. curve. And it pays to do this because the sample sizes associated with points on RS and AB are smaller than the sample sizes associated with the points on the original inspection boundary.

Similar considerations will apply to any proposed inspection boundary. It can always be " contracted," without affecting the O.C. curve, so that finally the functions $A(x)$, $R(x)$ are each non-decreasing functions of x, and this procedure will always reduce the mean sample size; unless, of course, the boundary is already in the " contracted " form. We can therefore enunciate the principle:

Any economical inspection scheme will have A and R boundaries such that $A(x)$ and $R(x)$ are non-decreasing functions of x . . . . (3)

But the limitation implied by this principle does not enable us to determine an inspection scheme, when the Risk and Safe Points are given. There will be many " contracted " schemes, all having the same Risk and Safe Points. And, although in the example just given we could calculate $O(p)$ by performing the reverse operation to " contraction," and so apply the Bernouilli formula, this will not in general be possible.

In general, the calculation of $O(p)$ for a contracted scheme is very difficult. If we neglect the presence of the R boundary, and if $A_r = (x_r, r)$ is the first point on the A boundary for which $y = r$, then the formula *

$$N'(OA_r) = \sum_{j=0}^{r-1} (-)^{r-j-1} \binom{x_j}{r-j} \, . \, N'(OA_j) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

* Found simultaneously by myself and Mr. James Godwin.

enables us to calculate the acceptance properties of such a scheme. But when x_r and y_r and r are large, the formula becomes unmanageable. Other methods exist, as we shall see later, but none of them is as simple as Wald's method, to which we now turn.

Wald's Binomial Test

With the assumptions stated at the beginning of this section, if $Tpr(A,p)$, $Tpr(R,p)$ denote, respectively, the total probabilities of acceptance and rejection for the boundaries A and B,

$$Tpr(A,p) = \sum_A \{N'(OP) \cdot q^x p^y\} \quad \ldots \ldots \ldots \quad (5)$$

where $P = (x,y)$ runs over the A boundary, and, similarly,

$$Tpr(R,p) = \sum_R \{N'(OP) \cdot q^x p^y\} \quad \ldots \ldots \ldots \quad (5')$$

where P runs over the R boundary.

Wald's method consists, not in determining $Tpr(A,p)$, $Tpr(R,p)$, for given A and R boundaries, but rather in determining the A and R boundaries so as to obtain prescribed values for $Tpr(A,p)$ and $Tpr(R,p)$. In fact, if p_1 and p_2 are given, and we determine the A boundary so that all along it

$$(q^x{}_1 p^y{}_1 / q^x{}_2 p^y{}_2) = (1-\alpha)/\beta = \text{const.} \quad \ldots \ldots \ldots \quad (6a)$$

and the R boundary so that all along it

$$(q^x{}_1 p^y{}_1 / q^x{}_2 p^y{}_2) = \alpha/(1-\beta) = \text{const.} \quad \ldots \ldots \ldots \quad (6b)$$

then we shall have

$$\begin{aligned} Tpr(A,p_1) &= \sum_A \{N'(OP) \cdot q^x{}_2 p^y{}_2 (1-\alpha)/\beta\} \\ &= Tpr(A,p_2) \cdot (1-\alpha)/\beta \end{aligned}$$

and similarly

$$Tpr(R,p_1) = Tpr(R,p_2) \cdot \alpha/(1-\beta)$$

Assuming for the moment (we prove it later) that the procedure is bound to terminate sooner or later either in acceptance or rejection, we have

$$O(p_1) = Tpr(A,p_1) = 1 - Tpr(R,p_1)$$

and

$$O(p_2) = Tpr(A,p_2) = 1 - Tpr(R,p_2)$$

and then, eliminating the Tpr's from these equations, we get

$$O(p_1) = 1 - \alpha \text{ and } O(p_2) = \beta \quad \ldots \ldots \ldots \quad (7)$$

Now, returning to the equations (6), they can be put in the form

$$x - by = H \quad \ldots \ldots \ldots \quad (8a)$$

$$x - by = -H' \quad \ldots \ldots \ldots \quad (8b)$$

where

$$b = -\log(p_1/p_2)/\log(q_1/q_2) \quad \ldots \ldots \ldots \quad (9a)$$

$$H = \log((1-\alpha)/\beta)/\log(q_1/q_2) \text{ and } H' = -\log(\alpha/(1-\beta))/\log(q_1/q_2) \quad . \quad (9b)$$

which shows that the A and R boundaries are parallel straight lines, satisfying the condition (3) since β is positive.

Here we should note a slight difficulty. The points on our inspection boundary must have integer co-ordinates; and it will not in general be possible to satisfy (8) with integer values. Because of this, we have to replace "$=$" in (6) by "$\geqslant$," and, following through, we find we have to replace "$=$" by "$\geqslant$" and α by $(\alpha/1-\beta)$ in the first equation of (7), while in the second equation we replace "$=$" by "$\leqslant$" and β by $(\beta/(1-\alpha))$. When α and β are small the effect of these modifications is very slight, and we shall henceforth neglect them.

By Wald's method, given the Risk and Safe Points p_1 and p_2, we can determine b,H and H' from (9), and so determine an inspection scheme. Conversely, given b,H and H', we can determine the values of $O(p)$ for pairs of values, p_1,p_2, by solving (9) for α and β. In both cases the approximations involved are slight when α and β are reasonably small.*

* Wald says (1) that he overlooked the possibility of working back from (8) to (7) at first. The converse procedure was found independently by George W. Brown and Milton Friedman in America, and in this country by the present author.

Simplification of the Wald procedure

The calculation involved in setting up an inspection scheme of Wald's type is not at all difficult, especially since common logarithms can be used in (9). But the derivation of actual inspection rules from the lattice diagram may happen to be somewhat tedious, and some simplification seems desirable.

Looking at equations (8), we see that if we introduce a "score" S, putting

$$S = x - by$$

we can compute S at each stage of the inspection by adding 1 each time an effective item is found, and subtracting b each time a defective is found. The procedure can then be described by saying that the score starts at O, and is kept at each stage. As soon as it rises to H, the procedure stops and the batch is accepted; while as soon as S falls to $-H'$, the procedure stops and the batch is rejected.

If, in particular, p_2 and p_1 are the Producer's 100-1 Risk and Safe Points, we have $\alpha = \beta = 1/101$, and then we have $H = H'$. To avoid negative numbers, we then start the score with a "handicap" H. We add 1 point for each effective found, and subtract b points for each defective. The process continues until either S reaches $2H$ (batch accepted), or S falls to O (batch rejected). b is called the "penalty."

On this basis we can draw up charts from which the values of H and b for given values of p_1 and p_2 can be read directly. The charts actually made are based on 100–1 chances. But if, for example, we required 10-1 chances instead of 100-1, we simply have to take $H/2$ instead of H for our "handicap." This follows from the formulæ (9), and is the reason why we choose to give the risks in terms of odds rather than in terms of probabilities. Further, if we wish to estimate the O.C. curve for given H and b, we can do it by reading the charts in an inverse sense, as indicated by an example below. Alternatively, of course, for the O.C. curve we can use logarithmic probability paper, as already indicated. In fact, it is best to find the middle part of the O.C. curve using probability paper, and to find the ends of the O.C. curve using the charts.

The "Ruine des Joueurs"

This simplified version of Wald's scheme can be compared to a game between two players R and A. The "skill" of R is measured by p, and that of A by q; and the stakes are 1 and b respectively. Each player starts the game with H counters, and the game continues until one or other is bankrupt. This assumes, of course, that b is an integer; but this is not a serious restriction.

Now, Christian Huyghens, in 1657, proposed the problem: "A and B each take twelve counters and play with three dice on this condition, that if eleven is thrown, A gives a counter to B, and if fourteen is thrown, B gives a counter to A; and he wins the game who first obtains all the counters. Show that A's chance is to B's as 244,140,625 is to 282,429,536,481."

To solve this problem approximately, using our charts, we find first the ratio of chances at each throw to be 27 : 15. The "percentage defective" with which we are concerned is therefore $(100 \cdot 15)/(15 + 27) = 35{\cdot}7$ per cent. The penalty here is 1. From the charts we find, for $100p_1 = 35{\cdot}7$ per cent., and $b = 1$, that $H = 7{\cdot}5$, about. The actual handicap is 12. So the actual odds are N to 1, where

$$\log N/\log 100 = 12/7{\cdot}5 = 1{\cdot}6$$

from which $N = 1600$, in moderately good agreement with Huyghen's result. Most of the error will arise through errors of interpolation on the charts.

Huyghens' answer is, however, exact, which implies he must have had a method for solution. The first published solution of Huyghens' problem was given by James Bernoulli, in *Ars Conjectandi* (1713), and his method directly generalizes to our case :

Let $u(x)$ be the probability that A will win when he has x counters. Then, at the next trial, A will either win 1 counter from R, with probability q, or he will lose b counters to R, with probability p. Consequently,

$$u(x) = q \,.\, u(x + 1) + p \,.\, u(x - b) \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

This is a difference equation for $u(x)$, which has to be solved subject to the boundary conditions

$$u(O) = u(-1) = \ldots = u(-b+1) = 0 \quad \ldots \ldots \quad (10a)$$

and

$$u(2H) = 1 \quad \ldots \ldots \quad (10b)$$

This can be solved in the usual form

$$u(x) = \Sigma C_t t^x,$$

where the t's are the roots of the auxiliary equation

$$t^b = q t^{b+1} + p$$

and these roots can easily be found geometrically. But the solution is rather clumsy. The best solution I have seen so far is due to Mr. Burman, who finds

$$u(x) = F(x)/F(2H) \quad \ldots \ldots \quad (11)$$

where

$$F(x) = q^{-x}\left\{1 - \binom{x-b-1}{1}(pq^b) + \binom{x-2b-1}{2}(pq^b)^2 - \ldots\right\}$$

for $x > 0$, and $F(x) = 0$ for $x \leqslant 0$.

From (11) we get the exact O.C, curve of a scheme with handicap H and penalty b, by putting $O(p) = u(H)$.

Sample size

If p_1 is the Producer's $N - 1$ Safe Point, then the average sample size for $p = p_1$ is, for N large, approximately $H(N-1)/S(N+1)$, where S is the "mean score," $q - bp$. This approximation is due to Wald.

If we want the exact distribution of sample size, for any value of p, we notice that the general term in the series (5),

$$N'(OP) \,.\, q^x p^y$$

is the probability that the batch will be accepted when the sample size is $(x + y)$. There is a similar term in (5′). So if we regard p and q as independent variables (*i.e.*, if we do not use the relation $p + q = 1$), we find the probability generating function for sample size to be

$$G(t) = Tpr(A,pt,qt) + Tpr(R,pt,qt)$$

and the mean sample size, for example, is then $G'(1)$.

This result will apply in the case of a large batch, whatever the shape of the inspection boundary. Mr. Burman uses it to extend his formula for the O.C. curve to cover the sample size, in the Wald case. (*See* p. 98 of this issue.)

Alternatively, we can extend Bernoulli's method (as Laplace did), and let $u(x,y)$ be the probability that A wins in exactly y more steps, when he has x counters. Then

$$u(x,y) = q \,.\, u(x+1, y-1) + p \,.\, u(x-b, y-1)$$

and the boundary conditions now involve t. An interesting solution of a special case of this equation was found by the Rev. Leslie Ellis.[6]

The fact that the mean sample size for the Wald test, as found by Mr. Burman, is always finite, constitutes a proof that the procedure is bound to terminate eventually. More strictly, it follows from Markoff's inequality that the probability that the procedure will go on for ever is zero. But the probability that the Wald procedure will go on for more than n steps is finite, however large n is. And so it sometimes may be desirable to "close" a Wald scheme, by putting an upper limit on the sample size. The effect of such a "closure" is often not serious; it has been studied in detail by Miss Stockman and Mr. Armitage, and their results are published elsewhere in the present issue of this Journal.

II. *General Sequential Tests*

Diffusion problems

The problem of the game we have just dealt with is similar to a linear diffusion problem, where a particle starts out from an origin O and, at successive unit intervals of time, jumps either to the right a distance 1, or to the left a distance b. The particle is constrained to lie in a line, and we have two absorbent boundaries, at distances H and $-H$ from O.

In diffusion problems we usually consider that the interval between successive jumps is small, and that the distances, 1 and b, moved at each jump, are both small compared with H. These assumptions enable the problem to be reduced to a differential equation, instead of a difference equation.

This leads us to inquire what happens to our equation (10) when we make such limiting assumptions.

Introducing the difference operator Δ, by

$$\Delta u(x) = u(x+1) - u(x)$$

so that $(1+\Delta)^b u(x) = u(x+b)$, we can write equation (10) as

$$\{q\Delta + p((1+\Delta)^{-b} - 1)\}u(x) = 0$$

Now, if $\Delta u(x)$ and $(pb - q)$ are of the first order of small quantities, and we neglect differences higher than the second, then, to the second order, the equation is equivalent to

$$\tfrac{1}{2}(pb^2 + q)\Delta^2 u(x) + (q - bp)\Delta u(x) = 0$$

and $\mu_1 = (q - bp)$ is the "mean jump" in the unit interval of time, while $\mu_2 = (q + pb^2)$ is the second moment of the jump. Retaining the heuristic character of the argument, we now replace Δ by d/dx, and so get the differential equation

$$u''(x) + \lambda u'(x) = 0 \qquad \text{(13)}$$

where $\lambda = 2\mu_1/\mu_2$. This is the "steady state" differential equation for linear diffusion. If we introduce more general "stopping points," K and $-L$, instead of H and $-H$, the boundary conditions become

$$u(K) = 1, \; u(-L) = 0 \qquad \text{(13a)}$$

Solving this equation, we find, for the probability of acceptance

$$u(O) = (1 - e^{\lambda L})/(e^{-\lambda K} - e^{\lambda L}) = f(\lambda) \qquad \text{(14)}$$

We refer the reader who wishes for a rigorous statement of the conditions of validity for this approximation to Khintchine (5), where a rigorous proof is given.

Linear sequential tests

The relationship of the sequential inspection method to the linear diffusion problem is useful in two ways. First, it suggests an extension to two-, three-, or more-dimensional diffusion problems; and these problems are seen to be related to those inspection problems where we are not merely concerned to classify components as "effective" or "defective," but we require to classify them separately according to two or more properties, such as length and diameter of rods. In such cases the plane inspection diagram no longer serves to represent what happens, and we have to have a three- or more-dimensional diagram, with one axis for each of the component properties we are inspecting.

Secondly, the diffusion analogy leads us to generalize in another direction—to consider sampling from distributions other than the binomial ones we have so far dealt with.

In fact, suppose we sample from a population for which the likelihood of the observation $X = x$ is $P(x,\theta)$, where θ is an unknown parameter in whose value we are interested. Assuming suitable conditions about $P(x,\theta)$—such as, for example, that for each value of x, $P(x,\theta)$ is a continuous function of θ, strictly increasing for $\theta < \theta(x)$ and strictly decreasing for $\theta > \theta(x)$—we can construct a test of the hypothesis H_0, which says that $\theta = \theta_0$, as follows : We take as origin some "base point" x_0, such that the first moment $\mu_1(\theta_0)$ of the distribution $P(x,\theta_0)$, about this origin, is positive, while the second moment about this origin is $\mu_2(\theta_0)$. Referred to this origin, we take the cumulative sum of observations Σx_i to be our "score," S. Then, with stopping points K and $-L$, we accept H_0 as soon as S reaches K, and reject H_0 as soon as S reaches $-L$.

Under these conditions, provided that μ_1 and μ_2 are small compared with K and L, we can calculate the significance level α of our test from the formula (14), approximately,

$$\alpha = 1 - f(\lambda_0) \qquad \text{(15)}$$

where

$$\lambda_0 = 2\mu_1(\theta_0)/\mu_2(\theta_0)$$

and we can adjust x_0, K and L so as to obtain any desired value of α.

In this way we test the hypothesis $\theta = \theta_0$ against alternatives $\theta \lesssim \theta_0$.

If, for example, we are concerned with normal populations with unit variance, and

$$P(x,\theta) = \frac{1}{\sqrt{2\pi}} exp - \tfrac{1}{2}(x - \theta)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

if θ_0 is positive we can take the origin to be $x = 0$, and then

$$\lambda_0 = 2\theta_0/1 + \theta^2{}_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and if we now take $K = (1/\lambda_0) \log_e a$, and $L = (1/\lambda_0) \log_e b$, we find

$$\alpha = (1 - a)/(1 - ab) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Linear sequential tests of this kind can be made to cover a wide variety of statistical problems. For example, we could, if necessary or convenient, test the variance σ^2 of some (not necessarily normal) population, basing our test on, say, the range w of samples of three. w then replaces x above, and σ replaces θ_0.

A linear sequential test (L.S. test) is, in general, a test which is based on a score consisting of the cumulative sum of the observations. Such tests will, for reasonably large samples, always be governed by the differential equation (13), and we always have three parameters at our disposal —the " base point," and the two stopping points—for fixing the significance level α.

Wald's probability ratio test

The line of development we have pursued so far is distinct from Wald's. He begins by considering a situation in which we wish to test two hypotheses, H_0 and H_1 against each other. The H_j ($j = 0,1$) specify the likelihood function $P_j(x)$. If the ith observation is $X = x_i$, we put

$$z_i = \log (P_0(x_i)/P_1(x_i))$$

the logarithm of the likelihood ratio, and C_n is then the cumulative sum of the z_i, up to the nth stage. The sequential probability ratio test (S.P.R. test) is then defined by the inequalities

$$(H_1), \ \log B \leqslant C_n \leqslant \log A, \ (H_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

which mean that H_0 is accepted as soon as C_n reaches $\log A$, while H_1 is accepted as soon as C_n reaches $\log B$.

By an argument of which we have already given an outline on page 6, Wald then shows that the probability of accepting H_1 when H_0 is true is α, and the probability of accepting H_0 when H_1 is true is β, approximately, provided that

$$A = (1 - \beta)/\alpha \quad \text{and} \quad B = \beta/(1 - \alpha)$$

If we apply this to the case of two normal populations, each with unit variance, and with H_0 saying that the mean is $+ \theta_0$, while H_1 says that the mean is $- \theta_0$, we find

$$z_i = \log_e (P_0(x_i)/P_1(x_i)) = 2\theta_0 \, . \, x_i$$

so that in this case the P.R.S. test is an L.S. test. It is in fact the same test as that indicated in the previous section, provided we put, approximately, for θ_0 small,

$$\lambda_0 = 2\theta_0/1 + \theta^2{}_0 \approx 2\theta_0$$

and we require of our L.S. test that

$$f(-\lambda_0) = \beta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

which is the condition that our L.S. test, besides giving a probability α of rejecting H_0 when H_0 is true, should also give a probability β of rejecting H_1 when H_1 is true—assuming we accept H_1 when we reject H_0, and vice versa.

Thus with the P.R.S. test, as compared with the L.S. test, we use up two degrees of freedom, in fixing both α and β. But with the general L.S. test we have three degrees of freedom available—the choice of the two stopping points K and L, and also the choice of the " base point," or origin. α and β account for the two stopping points, but what, with the P.R.S. test, has happened to our third degree of freedom, the choice of the base point?

Optimum property of the P.R.S. test

On comparing the L.S. test with the P.R.S. test for normal populations of unit variance, we see that, in the case of the L.S. test we fixed our " base point " at $x = 0$ for no better reason than mere convenience. On the other hand, with the P.R.S. test of H_0 against H_1, where H_0 gave $\theta = \theta_0$ while H_1 gave $\theta = -\theta_0$ we were compelled by the P.R.S. procedure to take our base point at $x = 0$—midway between the two means, where the probability densities on the two hypotheses were equal.

If, with the L.S. test, we took our base point somewhere other than the origin, the means referred to this new origin would be t_0 and $-t_1$, and they would necessarily satisfy

$$t_0 + t_1 = 2\theta_0 \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

and if we now put

$$\lambda_0 = 2t_0/1 + t_0^2, \quad \lambda_1 = 2t_1/1 + t_1^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

then our conditions for α and β become

$$f(\lambda_0) = 1 - \alpha, \quad f(-\lambda_1) = \beta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

The extra condition imposed by the P.R.S. procedure then becomes

$$t_0 = t_1 \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

Now the mean sample size, if H_0 is true, is approximately

$$S_0 = K/t_0$$

while the mean sample size if H_1 is true is approximately

$$S_1 = L/t_1$$

and if we now take θ_0, α and β to be fixed, and K, L, t_0 and t_1, λ_0 and λ_1, to vary subject to (21), (22), and (23), we find, using undetermined multipliers, that the conditions that S_0 should be minimised and that S_1 should be minimized give us the further equation (24).

Thus the P.R.S. test in this case is the " best " of the one-parameter family of L.S. tests satisfying the conditions about α and β, in the sense that, for the P.R.S. test, S_0 and S_1 are both minimized.

In fact, Wald (1) has shown, by a rigorous argument, that no sequential test, linear or not, which satisfied the conditions about α and β, can make S_0 or S_1 appreciably smaller than the P.R.S. test makes them.

Condition for the P.R.S. test to be an L.S. test

This optimum property of the P.R.S. test leads us to inquire, when will a P.R.S. test be an L.S. test? What restriction on the form of the one-parameter family of likelihood functions, $P(x,\theta)$ is implied?

Since the P.R.S. test is based on the cumulative sum of z_i, we must have

$$z_i = \log\left(P(x_i,\theta_0)/P(x_i,\theta_1)\right) = x_i \,.\, \phi(\theta_0,\theta_1) + \psi(\theta_0,\theta_1)$$

and if this is to hold for all x_i, and for all values of θ_0,θ_1, we can fix θ_1, put $\theta_0 = \theta$, and introduce a transformed parameter $\eta = \phi(\theta,\theta_1)$, and then putting $P(x,\theta_1) = g(x)$, we see that $P(x,\theta)$ must be expressible in the form

$$P(x,\theta) = g(x) \,.\, exp(x\eta).\ h(\eta).$$

Putting $h(x) = 1 - p$ and $\eta = \log_e p/q$ gives us the binomial case, while $g(x) = exp(-\tfrac{1}{2}x^2)$ gives us the normal case. And in general, it appears that, for the P.R.S. test to be an L.S. test, for every pair of distributions in the family, the family must be a family of binomial, normal, or " generalized Type III " distributions, taking η as the parameter instead of θ. In particular, it is evident that the P.R.S. test for the variance of a normal population of known mean will be an L.S. test.

Thus, in these cases, the P.R.S. test is an L.S. test, and the optimum property of the P.R.S. test carries over to the L.S. test, in the sense that, if we work out an L.S. test for a hypothesis H_0, using significance level α, then there will exist an H_1, and a β, such that this L.S. test is nearly optimum for H_0, H_1, α and β.

If the P.R.S. test is not a linear test, we shall have to balance the possible gain in economy

with the P.R.S. test, against the possible gain in simplicity of operation with the L.S. test. The situation here is reminiscent of that concerning the range and standard deviation in normal samples, where the greater simplicity of the range sometimes outweighs the loss in statistical efficiency.

Effect of deviations from normality

The argument given above for the differential equation (13), and the more conclusive argument of Khintchine,[5] show that the effect of departures from assumed normality in the case of L.S. tests is not essentially different from their effect with classical tests. In both cases, provided the samples are reasonably large, the assumption of normality is not really serious.

The P.R.S. test does present a new aspect in a way, however, because in making up this test there is no " sampling distribution problem " to be solved, once H_0 and H_1 are specified. And in industrial work we often know our distributions to be non-normal, and yet we use the normal tests, simply because we cannot calculate the true sampling distributions on any other hypotheses. Yet with P.R.S. tests, this " reason of convenience " for the assumption of normality ceases to be valid.

Another " reason of convenience " takes its place, however. Because the argument above shows that, unless we assume the distribution to be normal, or at least of a generalised Type III kind, our P.R.S. test will not be a linear test. So that, in spite of the greater simplicity of setting up the P.R.S. test, it is still advisable to assume normality, if possible, for simplicity of operation.

In this connection another point raises itself. With the P.R.S. test, the zero point for z is always the point where the probability densities, $P_0(x)$, $P_1(x)$ are equal. This point coincides with the point midway between the two means if the distributions are normal, and have the same variance, but it will not do so in general. Suppose, then, that we have a pair of slightly skew distributions to compare, and we determine the " stopping points " K and L by the formulæ (23). Where should we take our base point?

The differential equation (13) suggests we should again take the base point midway between the two means, not where $z = 0$. And Miss Rigg has verified this suggestion, in certain cases, by direct calculation of the probabilities involved.

Other types of sequential test

In the situations so far considered in detail, we have always been concerned with a single unknown parameter. This category does not, of course, exhaust the situations to which sequential methods can be applied.

Wald has considered the general problems of what he calls " composite hypotheses," and the reader is referred to his paper for these considerations. In particular, Wald has developed a sequential analogue of the t-test, to apply to the case of the mean of a normal population of unknown variance. The calculations involved in the operation of this test, however, are by no means as simple as those with L.S. tests, and we are led to ask whether an L.S. test can be devised for the same situation.

In fact, if we are concerned only with one-sided alternatives—that is, if we wish to test whether the mean is zero or not, when we know it will be positive, if not zero, then, instead of testing the mean we can test the median. And this can be done simply by observing the signs of successive observations. On the null hypothesis, the probability that each sign will be " + " should be $\frac{1}{2}$, while if the null hypothesis is not true, the probability of a " + " will be more than $\frac{1}{2}$. In this way, we can reduce the problem to the simple binomial case. And the surprising thing is, that some sampling experiments by Mr. Armitage indicate that, in using an extension of this binomial test instead of Wald's two-sided test, we lose very little in efficiency. Naturally, in extreme cases, Wald's test will be more efficient, but these will not often occur.

If we wish to test against two-sided alternatives, we can either run two tests in parallel, one against positive alternatives and one against negative alternatives, or we can take observations in pairs. In the latter case, the null hypothesis asserts that the probability of $(+,+)$ or $(-,-)$ should be the same as the probability of $(+,-)$ or $(-,+)$, so that we can base a test on the products of consecutive signs.

As tests of the median, rather than the mean (if the two are different), these binomial tests

have the doubtful advantage of being entirely independent of the form of the distributions involved. And all of them, except the last one, tend to be better than the classical t-test, in practical cases.

Comparative trials and double dichotomies

Suppose we have two events, E_1 and E_2. Under certain specific circumstances, E_1 has a probability p_1 of happening; and under certain (possibly different) circumstances, E_2 has a probability p_2 of happening. We may wish to compare p_1 and p_2—to test, for example, whether $p_1 = p_2$, or whether $p_1 = kp_2$. A trial of E_1 and E_2, designed to test such a hypothesis, is what I call a 2×2 comparative trial, or 2×2 trial.

For example, E_1 may represent getting a defective component by one process of production, while E_2 represents getting a defective component by another process of production. The components need not necessarily be the same. Then we may wish to compare the efficiency of one process with that of the other.

In other circumstances, we may have a set of components each of which may or may not have two properties, A and B. For example, a rod-shaped component may be correct for length (A), or incorrect for length (not-A); and it may be at the same time correct for diameter (B), or incorrect for diameter (not-B). In such a case, we may wish to see whether there is association between A and B. An examination of a sample of such objects, to detect such association, if it exists, is what I call a double dichotomy.

Provided that some, but not all, the objects we consider have the property A, we can let p_1 be the probability that an object has B, when we know it has A, and p_2 be the probability that an object has B, when we know it lacks A. Then in this case, our test of association will be a test of whether or not $p_1 = p_2$, and in such a case there may be no need to distinguish between 2×2 trials and double dichotomies.

Wald [1] has proposed a sequential test, primarily for double dichotomies, but also usable with 2×2 trials, which assumes one of the attributes, A say, to be controllable, so that we can determine that a given object will have A, or will not have A, at will. (For example, with our rod-shaped components, we might imagine them all to be already sorted into two boxes, according to length, A and not-A; and we can then choose which box we take from. To test the hypothesis $p_1 = p_2$, we make the observations in pairs, each pair containing one A and one not-A (allocated at random, by tossing a coin if necessary). Then each pair (A, not-A) may give one of four results:

(i) (B, B); (ii) (not-B, not-B); (iii) (B, not-B); (iv) (not-B, B).

Results of the first two kinds are neglected. But (iii) tends to indicate $p_1 > p_2$, while (iv) tends to indicate the opposite. And if the null hypothesis is true, results (iii) and (iv) should occur equally often in the long run. So, neglecting results (i) and (ii), we can reduce the test whether $p_1 = p_2$ to a simple binomial test whether or not the probability of (say) (iii), among all non-neglected results, is $\frac{1}{2}$.

This test can be generalized in several different directions. First, we can test whether $(p_1/q_1)/(p_2/q_2) = r/s$ by testing the binomial probability $r/(r + s)$ instead of $\frac{1}{2}$ among non-neglected trials. Alternatively, we can use triplets, say (A, not-A, not-A), instead of pairs; such a procedure is useful for testing $p_1 = 2p_2$ when both p_1 and p_2 are known to be small. And finally, we may notice that although the results (i) and (ii) may be irrelevant in the double dichotomy case, they are not irrelevant in the 2×2 trial; and so we may try to construct a test which takes results of this kind into account.

To do this, imagine a game between two players A and B, in which a third person takes the part of a " banker." The banker initially credits both A and B with H points. Supposing that both p_1 and p_2 are small compared with $\frac{1}{2}$, we construct the rules of the game as follows:

We take observations in pairs, as before. Then

if the result is (i), the banker adds 1 point to A's and B's credits;
if the result is (ii), the banker subtracts 1 point from A's and B's credits;
if the result is (iii), then B gives a points to A;
if the result is (iv), then A gives a points to B.

The game continues until either A's credit or B's credit, or both credits, are exhausted. In the

first case, we conclude $p_1 < p_2$, in the second, $p_1 > p_2$, while in the third case we may either draw no conclusion, or conclude that, to sufficient accuracy, $p_1 = p_2$.

If $u(x,y)$ is the probability that A will win when his credit is x and B's credit is y, we get the difference equation:

$$u(x,y) = p_1p_2u(x+1,y-1) + q_1q_2u(x-1,y-1) + p_1q_2u(x+a,y-a) + q_1p_2u(x-a,y+a) \quad . \quad . \quad (25)$$

with the boundary conditions

$$u(O,y) = O \text{ for all } y, \quad \text{and} \quad u(x,O) = 1 \text{ for all } x > O \quad . \quad . \quad . \quad (25a)$$

It turns out that the equation (25) is of "elliptic type," while the boundary conditions (25a) are of "hyperbolic type," and the solution is not unique, although our game cannot go on for ever. To overcome this difficulty, we put an upper limit U on the total credit the banker gives at any one time, which adds the condition

$$u(x,U-x) = O \text{ for } O < x < U \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25b)$$

Although the solution is now unique, the equation (25) involves differences of too high an order to be readily soluble. To reduce this difficulty, we modify the procedure slightly. We reduce "a" to 1, and, instead of making the banker add or subtract 1 point for (i) or (ii), we make him "threaten" to do so. Specifically, when the result is (i) or (ii), the banker takes a disc at random from a bag containing $(a-1)$ counters marked "0," and 1 counter marked "1." He then adds or subtracts the number on the disc from A's and B's credits. This leads to the second order equation:

$$(1-((p_1p_2+q_1q_2)/a))u(x,y) = (p_1p_2/a)u(x+1,y+1) + (q_1q_2/a)u(x-1,y-1) + p_1q_2u(x+1,y-1) + q_1p_2u(x-1,y+1)$$

When p_1 and p_2 are small, a given difference in frequency between results (iii) and results (iv) is more "significant" than when p_1 and p_2 are in the neighbourhood of $\frac{1}{2}$. The effect of the reductions in credit is to allow for this. Of course, a should be large compared with 1, otherwise we may make a decision on too little evidence. When p_1 and p_2 are about $\frac{1}{2}$, the game behaves very much like Wald's test. But an examination of particular cases by Mr. Owen and Miss Allinson has shown that, for p_1 and p_2 small, this procedure may be superior to Wald's.

I have not been able to find a solution of the general equation in closed form, but Mr. Allen, of Imperial College, has very kindly undertaken to tackle it by relaxation methods as soon as a computing machine becomes available.

Yet another kind of sequential test for 2×2 trials was the first sequential test that occurred to me. It is specially useful in situations where the probabilities p_1, p_2 are small, and where we have some idea beforehand about the true value of the ratio (p_1/p_2). For example, in development work on an instrument we may isolate at least two causes of failure. A proposed modification may be hoped to remove one of these causes, while it leaves the other unaffected. From past experience we can often say that if the modification is successful, our failures will be reduced by a certain percentage; and we may then try to determine whether our hopes are justified by an appeal to experiment.

The procedure then consists in arranging to try the modified instrument and the unmodified one, under similar conditions, and to continue to try each until a predetermined number r_1 of failures of the modified instrument, and a predetermined number r_2 of failures of the unmodified instrument, are observed. Then if p_1 refers to the modified instrument, and p_2 to the unmodified one, and n_1, n_2 are the number of observations made in the two cases, the probability of the pair (n_1, n_2) is

$$\frac{(n_1-1)!}{(r_1-1)!(n_1-r_1)!} \cdot p_1^{r_1}q_1^{n_1-r_1} \times \frac{(n_2-1)!}{(r_2-1)!(n_2-r_2)!} \cdot p_2^{r_2}q_2^{n_2-r_2}$$

while if $p_1 = p_2 = p$, the probability of getting some pair of results having the same total $n_1 + n_2 = N$ is

$$\frac{(N-1)!}{(r_1+r_2-1)!(N-(r_1+r_2))!} \cdot p^{r_1+r_2}q^{N-(r_1+r_2)}$$

and so the relative probability, on the null hypothesis, of getting the pair (n_1,n_2) out of all results with the same total N, is

$$\frac{(N-(r_1+r_2))!}{(N-1)!\,(r_1-1)!\,(r_2-1)!} \times \frac{(n_1-1)!\,(n_2-1)!}{(n_1-r_1)!\,(n_2-r_2)!}$$

which is independent of p. Hence, if we sum over all more extreme pairs having the same N,r_1 and r_2, we shall get the significance level of our result.

For example, with $r_1 = 1$, $r_2 = 2$, if we find $n_1 = 10$, $n_2 = 4$, we have the significance level

$$\frac{2!}{13!\,0!\,1!}\left\{\frac{9!\,3!}{9!\,2!}+\frac{10!\,2!}{10!\,1!}+\frac{11!\,1!}{11!\,0!}\right\} = 6/65$$

If p_1 and p_2 are small, n_1 and n_2 will be large, and an approximation becomes desirable. Noticing that if X is the number of trials required for 1 failure,

$$Pr(X > x) = q_1^{x} = exp(-k_1x) = 1 - \int_0^{k_1x} e^{-t}dt,$$

where $k_1 = -\log(1-p_1)$, we see that X/k_1 is distributed as χ^2 on two degrees of freedom, and the approximation is improved by taking $Y = X - \frac{1}{2}$.

Now $n_1 - \frac{1}{2}r_1$ is the sum of r_1 variables, independently distributed like k_1Y, and so $(n_1 - \frac{1}{2}r_1)/k_1r_1$ is distributed approximately as χ^2 on $2r_1$ degrees of freedom. Hence

$$R = (n_1 - \tfrac{1}{2}r_1)k_2r_2/(n_2 - \tfrac{1}{2}r_2)k_1r_1$$

is distributed as F on $(2r_1,2r_2)$ degrees of freedom. On the null hypothesis the k's cancel, and so, for n_1, n_2 large we get the rule, to calculate

$$R = r_2(n_1 - \tfrac{1}{2}r_1)/r_1(n_2 - \tfrac{1}{2}r_2)$$

and to enter Fisher's F tables with this value of R, for $(2r_1,2r_2)$ degrees of freedom.

The test is a valid one, whatever the values of r_1 and r_2. But it is best to fix r_1 and r_2 (*a*) according to how much experimentation we are prepared to do, on the average, and (*b*) so as to maximise the chance of getting a significant result when p_1 and p_2 have the values we hope they are going to have.

The mean values of n_1 and n_2 are

$$\bar{n}_1 = r_1/p_1 \quad \text{and} \quad \bar{n}_2 = r_2/p_2$$

so that, guessing p_1 and p_2, we can guess how much experimentation we are likely to do, on the average. If, for example, we expect 2 per cent. failures and 5 per cent. failures, respectively, and we take (r_1,r_2) to be (1,2), then we should expect about 50 trials of the modified design, and 40 of the unmodified design.

It sometimes happens that the cost of trying the modified design and the cost of trying the unmodified design are not equal. Then, if ν represents the ratio of these costs, we shall want to minimize

$$\nu\bar{n}_1 + \bar{n}_2 = (\nu r_1/p_1) + (r_2/p_2)$$

which, given a guess at p_1 and p_2 gives us one relationship between r_1 and r_2.

The aim (*b*) gives us another relationship between r_1 and r_2. When p_1 and p_2 are small, k_1 and k_2 are respectively nearly equal to p_1 and p_2. So that if $(p_1/p_2) = \theta$, then R/θ is distributed as F. Hence, guessing θ, we can guess the distribution of R, and so guess the likely chance of obtaining a significant result. If we now maximize this chance, we have another condition on r_1,r_2, which serves, together with the first condition, to determine r_1 and r_2 uniquely.

The actual estimation of the best values of r_1 and r_2 is most easily done graphically. A full explanation would be lengthy; I hope the method is sufficiently indicated above.

It should be noted that if we guess badly at the values of p_1 and p_2, all that happens is that we let ourselves in for more experimentation than we need have done; the validity of our results is unimpaired.

By entering the F tables with R/θ instead of R, of course, we can test whether $p_1 = \theta p_2$, instead of $p_1 = p_2$, if we so wish. Another generalization is to the case of a $2xn$ trial, where, provided $p_1,p_2 \ldots p_n$ are all small, we can use Bartlett's test.

We may notice, as a curiosity, that the true probability function of R is zero at all irrational points, and non-zero at all positive rational points. Further, the value of this function at the point (ν/s) depends essentially on the position of (r/s) in the Favey series.

III. General Inspection Problems

Qualitative batch inspection

We now return to the problem we began with—that of determining a suitable inspection scheme for batches of components, which we have already seen as the problem of determining a desirable acceptance and rejection boundary on the inspection diagram.

Although we have headed this section " General Inspection Problems," in fact we shall deal specifically only with cases where three restrictions are satisfied :

(*a*) We assume the classification of components into " effectives " and " defectives " is a simple one, so that the two-dimensional inspection diagram is applicable. We also leave aside any statistical questions which may arise from the possibility that the inspection test used is not directly a " user test." For example, with the " tropical testing " of radio components, we do not actually test the components by taking them for two years to the tropics; and since we cannot do this, many statistical problems arise in connection with a substitute test, into which we do not enter here.

(*b*) We assume that the product is inspected in self-contained batches, of a fixed size B, each batch being the subject of a single inspection judgment. This excludes the case of " continuous production."

(*c*) We assume that the components in the batches are thoroughly mixed when the batches come up for inspection.

The process curve

With the assumptions we have just made, each batch will, for our purposes, be uniquely described by the value of the fraction defective, p. And if we imagine the actual fractions defective of batches to be recorded over a period of time, we can form a histogram by plotting fractions defective against frequency of batches having this fraction defective. The curve obtained in this way we call the " Process Curve " over the period in question.

In order to avoid awkward summation signs, we assume the batch size B to be large enough to permit a continuous approximation. We may then represent the P curve by a function $P(p)$, so that $P(p)dp$ is the relative frequency of batches with fraction defective between p and $p + dp$.

We usually do not know the precise shape of the P curve, but we can often discover its general form. This will be determined partly by the properties of the production process—whether it is stable or unstable, whether the machines involved are working close to, or far from, their natural tolerances, and partly by the actual batching procedure employed.

The batching procedure is important. If we bear in mind that the general purpose of inspection is to separate sheep from goats, it should be obvious that a rational batching procedure, which secures that " bad patches " in production are kept isolated from the good patches, will often go half-way towards solving the inspection problem. No matter what sort of plan we use—sequential or non-sequential—any such plan will find it easier to separate really good batches from really bad batches than to sort out the " not so bad " from the " not so good " among a set of mediocre batches. We can sum this up by saying that the " batching clause " of the inspection scheme should be designed to secure that the P curve is " well-separated "—multimodal if possible, and with modes as widely separated as possible.

Apart from their direct value in production shops, one of the principal virtues of running " Quality Control Charts " is that they enable a rational batching procedure to be carried out.

Acceptance sampling and rectifying inspection

We now have to draw a distinction between those inspection plans in which the actual process of inspection makes no difference to the fraction defective in the particular batch being inspected, and those in which the fraction defective may be altered by the process of inspection. We have already called inspection schemes of the first kind, " Acceptance Sampling Schemes." Schemes of the second kind we call " Rectifying Inspection Schemes."

In Section I, where we assumed the batch size N to be effectively infinite, and where our decision was restricted to acceptance or rejection of the batch, we considered an A.S. scheme. On the other hand, the well-known Dodge–Romig Single and Double Sampling Inspection plans are examples of R.I. schemes; because, with these plans, there is a provision for 100 per cent. inspection of doubtful batches, and it is assumed that when 100 per cent. inspection is carried out, the defective items are replaced by effective items, and so the batch fraction is improved.

Broadly speaking, an A.S. plan will be in order if the cost of inspection of items is large compared with the cost of their production. For example, if the test involved is a destructive one, the cost of inspection must be at least equal to the cost of production; and in such a case we must use an A.S. plan. The idea of 100 per cent. inspection in such a case is clearly ridiculous; it may relieve the consumer's feelings, but it will do little else.

The inspection properties of an A.S. plan will be specified by the O.C. curve of the plan—that is, by the function $O(p)$, which gives the probability of acceptance of a batch whose fraction defective lies between p and $p + dp$.

On the other hand, the inspection properties of an R.I. scheme will be specified by a matrix, or a function of two variables, $K(p,p')$, where $K(p,p')dpdp'$ represents the probability that, if a batch is presented to the inspectors with fraction defective lying between p' and $p' + dp'$, it will be passed out by them after having had its fraction defective reduced to between p and $p + dp$. The matrix $K(p,p')$ is called the O.C. matrix.

The outgoing quality curve

The effect of a given inspection scheme on a process with P curve given by $P(p)$, in the case of an A.S. scheme is given by

$$Q(p)dp = O(p)P(p)dp \Big/ \int_0^1 O(p)P(p)dp \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

while with an R.I. scheme, the effect is represented by

$$Q(p)dp = \int_0^1 K(p',p)P(p')dp' \, . \, dp \Big/ \int_0^1\int_0^1 K(p',p)P(p')dp'dp \quad . \quad . \quad . \quad . \quad (27)$$

Thus, with an R.I. scheme, as compared with an A.S. scheme, the function of one variable, $O(p)$ is replaced by the functional operator

$$\int_0^1 K(p',p)(\quad)dp'$$

The function $Q(p)$ represents the relative frequency of outgoing batches, whose fraction defective lies between p and $p + dp$. The curve of $Q(p)$ against p is therefore called the Outgoing Quality Curve, or O.Q. curve.

The consumer's quality curve

The O.Q. curve gives the relative frequencies of batches of various qualities as passed by the inspection scheme. But it is not necessarily the curve in which the consumer is interested, because he may not be directly interested in the quality of single batches.

Suppose, for example, we have a consumer who uses the components inspected as parts of a larger assembly. What this consumer will be chiefly interested in is maintaining a steady flow over his production lines, and not having to shut down every so often through finding he has no "effective" components in stock. Thus the group of items on whose quality this consumer is interested will be a group whose size is determined by the number of components he habitually carries in stock. This group of items, in whose quality the consumer is primarily interested, is what we call the "Consumer's Lot."

The "Consumer's Lot" size will be determined by the consumer's requirements; while the Producer's Batch size will be determined by the producer's convenience, and by the principles already indicated. So the Consumer Lot and the Producer Batch will not always be the same thing.

Sometimes there may be several different consumer lot sizes in which the consumer is interested —if, for example, he runs several factories, with store-houses of different capacities.

In general, we assume that the consumer lot size L is an integral multiple of the batch size B. If $L/B = n$, then the Consumer's Quality Curve (C.Q. curve) will be obtained from the O.Q. curve by "n-tuple convolution." That is, the distribution of consumer lot quality is obtained from the distribution of batch quality by finding the distribution of means of samples of n from the O.Q. curve.

In particular, when n is very large, the C.Q. curve will have a single ordinate, at the mean of the O.Q. curve:

$$\bar{Q} = \int_0^1 pQ(p)dp \qquad (28)$$

This $\bar{Q}$ is then called the Average Outgoing Quality (A.O.Q.). If the consumer is interested only in $\bar{Q}$, this corresponds to the case considered by Dodge and Romig under the heading "Average Lot Quality Protection."

Another case is when $n = 1$, and the consumer is anxious to avoid having more than a certain proportion (say, one tenth) of his lots with quality worse than a certain "tolerance quality" t. His requirements will then take the form that

$$\int_t^1 Q(p)dp \text{ must be less than } 0{\cdot}1$$

This value "t" then corresponds (nearly, but not exactly) to Dodge and Romig's "Lot Tolerance Quality."

In practice, the requirements we have to meet will take the form of requirements about the shape or form of the C.Q. curve. These will then have to be translated into requirements on the O.Q. curve. Then, having found the shape of the P curve, we can determine the form of the O.C. curve or the O.C. matrix which is necessary to meet requirements, using the relationships (26) and (27).

The sample size matrix

In designing an inspection scheme to have a given O.C. curve or O.C. matrix, we shall also be concerned with the amount of inspection involved. For a sequential scheme, the sample size will not be always the same number, even when the fraction defective p remains constant. For constant p, we shall get a sample size distribution. And for various values of p, we get a family of sample size distributions.

Thus we have a sample size matrix, $s(x,p)$, such that $s(x,p)dxdp$ represents the probability that the sample size used will lie between x and $x + dx$, when the fraction defective lies between p and $p + dp$.

The actual sample size distribution will depend on the process curve, and will be given by

$$S(x)dx = \int_0^1 s(x,p)P(p)dp \,.\, dx \qquad (29)$$

and the mean sample size is

$$\bar{S} = \int_0^\infty xS(x)dx = \int_0^\infty\int_0^1 xs(x,p)P(p)dpdx \qquad (30)$$

while the variance is $\int_0^\infty (x - \bar{S})^2 S(x)dx$.

In practical problems we shall wish not only to minimize $\bar{S}$—*i.e.*, to minimize the total amount of inspection in the long run—but also we shall not want the variance of S to be unduly large, since this would imply heavy fluctuations in the amount of inspection required per batch. This would mean fluctuating demands for inspection labour, or else an unsteady flow of batches through the inspection, and either of these would increase inspection overhead costs.

General problems of design

It should now appear that, given only the consumer's requirements, in the form of restrictions on the acceptable O.Q. curve, the problem of finding the " best " type of inspection plan has no solution. In fact, there is no such thing as a " good " type of inspection scheme, if by " good " we mean " always good." The suitability of a given type of inspection scheme in a given situation depends on a host of circumstances, among which we may enumerate the P curve, the relationship between batch and lot, and the relative importance of overhead and running costs in the inspection department.

In particular, the P curve is of fundamental importance. The expressions (26), (27), (28), (29) and (30) all indicate that the results of our scheme depend essentially on the form of the P curve. And we shall shortly see that sometimes the P curve may be of such a kind as to make nonsense of some inspection schemes which, nevertheless, are quite good in other cases.

Use of the inspection diagram

In working out special inspection schemes to suit special circumstances, we have found the inspection diagram to be a great help.

We have to make an addition to the inspection diagram as already outlined, in the case of R.I. schemes, because it is a necessary feature of such a scheme that the batch size B should be finite. This can be done by drawing in the line

$$x + y = B \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

and then, when we are dealing with a batch whose fraction defective is p, this means that all our permissible inspection paths must end at the point Q on this line, whose co-ordinates are (qB,pB). Then, whatever the acceptance or rejection boundaries, we shall always have, with the notation of section I,

$$P(X) = N'(OX) \,.\, N(XQ)/N(OQ) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

where $N(XQ)$ is the number of paths from X to Q, regardless of the inspection boundary.

As one application of the inspection diagram, we can determine the general form of the function $K(p,p')$ for a quality inspection scheme. Because, if we accept at the point (x,y), and we assume (as always) that we replace defective items by effective ones as soon as we find them, then acceptance at (x,y) means reduction of the fraction defective from p' to p, where

$$p = p' - (y/B)$$

and the sample size used in the case will be

$$s = x + y$$

which shows that the general form of $K(p,p')$ must be, for a given inspection boundary,

$$K(p,p') = L(p' - p)\binom{B - \beta(p' - p)}{Bp}\Big/\binom{B}{p'B} \quad . \quad . \quad . \quad . \quad . \quad (33)$$

where $L(x)$ is an increasing function of x which vanishes when x is negative. Thus the functional operators we are concerned with in R.I. schemes are generalisations of the " closed cycle " operators much studied by Volterra.*

Similarly, if we write the equation to the A boundary as

$$x + y = \beta(y)$$

and if we have no R boundary (as in Dodge and Romig's schemes, where we replace rejection by 100% inspection), then the probability of arriving at (x,y), and accepting there, is the probability of using the sample size s, and so $S(x + y,p)$ must satisfy

$$S(\beta(B(p' - p)),p') = K(p,p') \qquad (p \neq 0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

provided that our scheme is " contracted," so that there is only one point on the boundary giving the sample size s.

* In fact, K defines a " transition operator " on a " space (AL)." See Birkhoff, " Lattice Theory " (1940), p. 133.

As another application of the general formula (32), suppose we have a P curve which is binomial in form, so that the *a priori* probability that the end-point will be $Q = (q'B, p'B) = (E, D)$ say, is

$$\binom{E}{D} p^D (1-p)^E = N(OQ) \, . \, p^D q^E$$

and the *a priori* probability of reaching $X \quad (x, y)$ is

$$N'(OX) \, . \, N(XQ) \, . \, p^D q^E = N'(OX) \, . \, p^x q^y \, . \, N(XQ) p^{D-x} q^{E-y}$$

Now, x is the number of defectives found by the inspector, while $D - x$, is the number remaining in the batch. If we now consider a single sampling scheme, with the boundary made up of points on the line

$$x + y = n \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

we shall have $\qquad y = n - x$

and $\qquad E - y = (B - n) - (D - x)$

so the *a priori* probability of reaching the point P on the line (35) splits up into two factors, one of which contains only the number of defectives found in the sample of n, while the other contains only the number of defectives found in the batch.

This means, that if our P curve is binomial in form, and we take a single sample of n from each batch, then the number of defectives found in the sample is distributed entirely independently of the number of defectives left in the batch. In practical terms, if our P curve has the binomial form, and we take single samples of n, accepting up to k defectives and rejecting more than k defectives, then the fractions defective remaining in the batches we reject will have exactly the same distribution as the fractions defective remaining in the batches we accept. Our O.Q. curve is exactly the same as our P curve, and our single sampling scheme is a pure waste of time.

Mood has obtained a similar result by another method. He also shows that the correlation between defectives found and defectives remaining is negative, if the P curve is more lepto-kurtic than the binomial curve; while it is positive only if the P curve is more platykurtic than the binomial curve. This further serves to emphasize the importance of having a " well-separated " process curve.

Another application of the inspection diagram is made in what we have called the " matrix method " of determining the properties of any inspection scheme. This depends on dividing the interior of the inspection boundary by lines at 45° to the axes, into " slabs." It can be shown that each slab corresponds to a " transition probability matrix," which can have only one out of four possible forms. The total effect of the scheme is then obtained by multiplying up these matrices. Mr. Armitage and Miss Stockman have applied this method to the examination of " closed " sequential schemes in the paper already referred to.

Finally, the inspection diagram is of direct practical use in carrying out sequential methods of inspection. It suggests the construction of an electrical or mechanical network which will serve to carry out automatically the functions of counting, and registering acceptance or rejection as required. The possibility of making machines of this sort, which absolve the inspectors from the necessity of keeping running records of results, means that we need not be deterred from making up sampling schemes by the thought that they may be too complicated to carry out in practice.

Conclusion

May I conclude with some speculative statements, designed to provoke controversy?

First, with regard to sequential tests in general. The startling simplicity and efficiency of the P.R.S. test, which evidently was evolved by Wald by an extension of Neyman and Pearson's ideas on testing hypotheses, would seem to be a strong argument in favour of the adoption of these ideas as a basis for developing statistical tests. Even if we approach the matter from the point of view of L.S. tests, we have seen that the sequential method leaves us with an extra degree of freedom in the choice of test to use, after we have satisfied the requirements with regard to significance level and minimum sample size. In fact, for all the tests I have described, except only the test for comparative trials given on page 14, some idea equivalent to Neyman and Pearson's idea of " power " seems necessary to give us a unique test.

But it may be that those who reject the ideas of Neyman and Pearson would in any case reject many sequential tests for another reason. For it seems difficult, if not impossible, to apply a

physical process of randomization to an "unclosed" sequential procedure. Wald's test for double dichotomies is an exception, but in general it seems we can apply sequential procedures only to cases where some sort of randomization is already imbedded, as it were, in the problem considered. For example, in inspecting components from a box, we suppose the components to have been already mixed up in the box, so that the sequence of results we get on inspecting them can be considered to be a random sequence.

On the other hand, it may be said that the notion of a sequential *procedure*, which is intimately bound up with every sequential test, really amounts to a change in the Neyman–Pearson theory. If so, the change is for the better, in that the actual procedure used in carrying out the experiment now does determine the test to be used.

Next, with regard to the theory of statistical estimation by "fiducial limits" or by "confidence intervals," it has always seemed to me to be a defect of both these methods, that the distance between the upper and lower limits for the estimate were dependent on the results of the trial, and could not be fixed in advance. May we speculate that the extra degree of freedom we have with sequential procedures may enable us to estimate parameters within limits fixed in advance? Certainly Haldane's work [2] is a good step in this direction.

Thirdly, may I suggest that there is a rich, almost virgin field for exploration to be found in studying the rules of games of chance and of skill? Many apparent eccentricities of these rules are due in reality to sound foresight on the part of the rule-makers. And we may be able to use their foresight in making tests for practical situations. To give one example: the rules of tennis require that at least four points must be played before the game is over, yet after these four points have been played, a majority of two is sufficient for game. This rule presumably is made to safeguard against the result being determined before the players have really settled down. Now we have a similar situation in sampling inspection, caused by the phenomenon christened by Mr. Womersley "Foreman's fingers." This refers to the remarkable ability, often shown by experienced inspectors, to pick out most of the defectives in a batch in their first sample. To guard against this sort of thing, it is often advisable to put a lower limit on sampie sizes, similar to the limit we have in tennis.

Finally, in the general problems of sampling inspection we have an example of a practical situation in which we not only can, but must, apply Bayes' Theorem (equation 26). Much earlier work on inspection seems to me to have been stultified by an attempt to carry over into this field the unmodified results of the theory of significance tests, where we take care to avoid Bayes' Theorem. Of course, as Mr. Kendall has made very clear, there is a difference between Bayes' Theorem and Bayes' Axiom. But I rather think that, in assuming a specific form for the Process Curve, often in circumstances where we have little but "engineering judgment" to go upon, we are close to assuming at least a weak form of the axiom, as well as the theorem.

Acknowledgements

My debt to Wald is obvious. Much of the above is based on Wald's original report on sequential tests, published in 1943. His later paper [1] came to hand while this paper was being prepared, and it contains much that overlaps with the above.

My debt to friends and colleagues in the Ministry of Supply Advisory Service on Quality Control, where most of this work was done, is very great. At one time or another, almost every member of the department, besides those I have already mentioned by name, was drawn into work on theoretical or practical aspects. Over us all was Mr. Womersley, who saw immediately the possibilities of the new developments and gave us every encouragement.

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Discussion on Mr. Barnard's Paper

Mr. Womersley, in proposing a vote of thanks, said this was an occasion of peculiar pleasure to him, because here an enterprise in which he had taken a little part was giving this new or rejuvenated Section its first fruits. He hoped this would be the first of a number of papers publishing openly what had been done in secret. Mr. Barnard had said one or two kind words about him which were not wholly deserved. He did not see how anyone, seeing this sort of thing arise, could possibly have failed to encourage it in every possible way. This work, which was done at the same time independently by Barnard and his associates here and by Wald and his associates in the United States, did open a new chapter in statistical theory.

He would not say anything about the technical content of the paper because he had seen it so many times that he felt he was not in a position to make any criticism. He hoped this work would not in future be confined entirely to the problems which arose in industry. Those of them who had been concerned with the application of statistics in industry owed a great debt to those working in biological and agricultural fields, and they could in this way repay a little of the debt.

They would all agree that it was a very great pity that Mr. Barnard could not go into the technical and industrial detail a little more on this occasion, but perhaps he might be able to extend the practical end of this work at a future date.

Dr. Bartlett said that he was very glad to second this vote of thanks because, as they all would now realize, but as some already knew before the paper was presented, the author had played a major part in this country in the development of these sequential methods. Mr. Barnard had rather modestly not referred that evening to one of his first contributions, which he had described on page 14 of the paper (the section beginning, "Yet another kind of sequential test for 2×2 trials," etc.). It was the application of Fisher's z test in a rather ingenious way to discriminate small probabilities. That particular test which had been very useful (at least, he had found it useful), was actually in the hands of war-workers before any work from America by Wald had reached this country. He found it useful to distinguish here, however, between what Wald had called sequential tests and what Mr. Tweedie had called inverse sampling methods. The distinction that he saw, in the important case of the binomial distribution which Mr. Barnard had illustrated that evening, was that the inverse sampling method was one in which, instead of in the ordinary way taking a fixed number of articles n and then considering the number of defective articles, one turned the thing, as it were, upside down, and decided to get a fixed number of defectives, and then went on and determined the size of sample. That was the first kind of inverse sampling which was used. Possibly the name "sequential tests" might—though this was a matter of opinion—be reserved for the strict procedure which was suggested by Wald, corresponding to his theory, where, as Mr. Barnard had said, by starting from first principles, he was able to formulate a uniquely defined technique.

However, as Mr. Barnard in his paper had shown, and as Professor Wald had shown in his published paper in the *Annals of Mathematical Statistics*, in the historical notes at the beginning of both papers, it was quite clear that a number of workers, during the war especially, had been groping towards this kind of technique. From that point of view a rather curious situation arose during the war, because there were a number of outside workers, Dr. Case, Professor J. B. S. Haldane, and Mr. Tweedie, who were advocating these inverse sampling methods, and it placed those of them who were, so to speak, in the "know" on this rather in a predicament as to whether they should adhere to rigid secrecy (like Brer Rabbit, "lie low and say nothing"), or should divulge what they knew.

He wished to consider now, rather in a broad fashion, the relations of this technique to orthodox theory. Mr. Barnard had emphasized in his paper a very interesting parallel that existed between the theoretical problems in these sequential tests and in classical problems which had arisen in the past in connection with gambling and, in physics, in the theory of diffusion. Roughly the division was that the discrete or difference equations he discussed in his paper had been connected with gambling problems, and the approximate large sample theory which gave rise to differential equations had been useful in the theory of diffusion in physics. But both these sides of the theory had been really aspects of what, as Mr. Barnard had said, was called "random walk" theory. He was rather fortunate to read Wald's original report at about the same time as he was reading a review by Chandrasekhar on random walk theory (*Reviews of Modern Physics*, January, 1943). He mentioned this because, while he confined some slight work of his own at the time to large sample theory, it did enable him to see these various broad relations. For example, the general gain in efficiency for the mean of a normal sample in sequential tests (at least in the region of the two points which, as Mr. Barnard had shown, determined the curve) must be a general result which approximately applied to any sequential test; moreover, being linked to the ordinary theory of likelihood estimates and the information function, the theory could be immediately extended to the case of more than one unknown parameter. This had other applications. For example, Mr. Barnard had stressed the value of the linear sequential test where the population had a certain

functional form; this made the test much simpler. That property appeared to him identical with the simplicity property of likelihood equations which determined a sufficient statistic.

Again, later on, Mr. Barnard discussed the efficiency of a median, but one could see that the relative efficiency of different statistics must tend to be just the same in this theory as in the ordinary theory. The efficiency of a median would tend to be of the order of 64 per cent., as it was in ordinary theory. He, too, was therefore rather surprised that the median was as good as was suggested, because if the gain in a sequential test be considered to be a factor of about 2, it would imply that the median of the sequential test would be not very much better than the mean of the ordinary test. But he admitted that the transition to finite samples made that figure an approximation and no more.

Finally, he wished to recall the warnings which Mr. Barnard had given them that these sequential procedures were not always practicable, and even if they were, one must be careful to take random samples. But it was quite clear, apart from that, that this technique was of permanent theoretical value, and he thought that Mr. Barnard had done a great service by describing it to them that evening.

Dr. Vajda said that there had been little time to study the paper, and therefore his remarks would probably cover some of the problems dealt with by the author, but it would be a pity if the experience of workers in this field were not exchanged. He had been encouraged also by the fact that all this work was to a large extent carried out in wartime, when they could not speak to their friends as they would have liked, and thus discover what others thought about ideas which were slowly developing.

He thought the author would agree with him that this paper should be interpreted as a means of encouragement, and he would like, therefore, to mention a few points which occurred to him.

The author stated at the opening of the first section, "We assume that the number of components in the box is large compared with the number of components we actually examine. . . ." The speaker supposed that they must assume that the probabilities did not change during the sampling—that if they took something out of the box, the relative probabilities remained the same as before. Was anything known of what happened—or in what direction changes happened—if they were sampling from a finite, not a large, population, and during the process of sampling the probability changed? Could anything be done on the safe side? He himself had come up against this problem in tests in which, once the thing had been tested, it was destroyed and could not be used again, but also the remaining probabilities were changed in two ways. If a component were found defective, this defective component would no longer remain among these individuals, so that not only would the probability of finding the defective be changed, but also the quality, the quality depending on whether the defective were found and eliminated or not.

Another thing which had some connection with the sequential tests had happened in some work he was doing when they had no idea whatever in advance what the quality might be, and therefore had to start on some assumptions. During the test they found, perhaps, that it was necessary to revise the specification, to take over many batches which might have been quite good, even if they did not satisfy the original specification, and therefore they had thought it reasonable to develop a system by which they "labelled" the batches. Of one batch, for example, they might be reasonably sure that the components were to their specification, but for other batches, they had to find an appropriate specification. In the end it was decided that it was not necessary to start with any specification at all, but during the test they decided which specification could be reasonably satisfied by these batches. The inverse might also arise namely, the investigator might start with a planned scheme and find that his batches were so good that it was a pity to stop there and say, "This satisfies my specification anyway, and therefore I will not go on sampling." Circumstances might arise in which one said, "I am perfectly prepared to go on if it helps me to be reasonably sure that this sample is much better than I had assumed." It might happen that more could be stated about these points and more information given from the laboratory.

Mr. M. C. K. Tweedie, who was called upon by the Chairman but had not prepared a contribution to the discussion, said he could give a rough idea of how he became interested in the subject. At the beginning of the war he was starting research in a physics department which had made a large number of electrophoretic measurements on colloids. It had been noticed that the times taken by the colloid particles to cover a fixed distance had some very strange properties. One that stood out was that there was a considerable correlation between the variance and the mean. He therefore got down to an investigation of the problems involved, a principal one being that of inverse sampling with a continuous variate, which, as Dr. Bartlett had said, corresponded to a large sample theory in sequential analysis. He succeeded in expressing the cumulants of a continuous distribution in terms of those of its inverse, and later proved the simple relation between the cumulant-generating functions.

On the general matter of Mr. Barnard's paper he could say little, as, having received it only the day before, he had had no chance to read it with the attention it deserved. However, he agreed with Dr. Bartlett that the L.S. test and the P.R.S. test were probably equivalent in those distributions

which Fisher had shown to possess a sufficient statistic. This comprised a much wider class than Mr. Barnard had indicated, and its members had the property that their inverse distributions also belonged to the class.

MR. A. E. JONES said that it seemed to him that the efficiency of the sequential sampling test could not be judged without taking into consideration the possibility that no conclusion would be reached without 100 per cent. sampling.

In certain circumstances it might be important to know that the probability that the lot fraction defective exceeded specification was small. It might not be adequate to say "the probability that the lot fraction defective exceeds specification by more than a certain small quantity is small."

With parallel boundaries it would be necessary to limit the sample size, and in that proportion of cases when no conclusion could be reached, 100 per cent. sampling might be necessary. Therefore he thought it would be desirable to develop sequential tests of a type that would enable sampling to continue indefinitely without having to fix a maximum sample size. What seemed to be required was an expanding boundary, such that if the lot fraction defective was near to the limit of the specification, the probability of incorrect acceptance or rejection might continue to be some comparatively small quantity, however large the sample. He made the suggestion tentatively, but if such a test could be developed it might be more satisfactory.

DR. YATES congratulated Mr. Barnard on his admirable exposition of what was virtually a new subject, and one which was of considerable fascination to mathematical statisticians. It was always interesting and encouraging to find a field of statistics where mathematical theory was of real value, the more so because, in spite of all that had been done to improve the methods followed in the collection of data and the planning of experiments, statisticians still spent a great deal of time on the rather empirical task of "making sense of figures."

He desired to raise one point on the paper. After developing sequential tests of the ordinary type, Mr. Barnard had gone on to discuss the development of sequential tests for observations falling into the 2×2 type of contingency table. The situation envisaged was that one had a piece of apparatus, made some improvement in design, and then wished to test whether that improvement had really made the apparatus better. Mr. Barnard had reduced that problem to the question, "Is the apparatus better or not?" Stated in that form it could be answered by the ordinary test of significance applicable to 2×2 contingency tables, or by a sequential test, which in certain circumstances would do the same kind of thing more efficiently. But the speaker thought that the practical problem was not merely whether the improved apparatus was better, but also how much better. In fact the problem was one of quantitative estimation, and not merely of acceptance or rejection of a given hypothesis. Consequently it could best be treated by the ordinary types of experimental test based on quantitative measurements.

This question of the postulation of the problem—in this case what they really wanted to learn from the test—was very important, and change in postulation often altered radically the appropriate type of solution. There was always a tendency in any new development of statistics to attempt to extend it to problems which in fact were really better covered by procedures which had been developed specifically to deal with those problems. An analogous situation had arisen in agriculture, when formal tests of significance applicable to agricultural experiments were first developed. It then became the fashion on all occasions to test whether a particular treatment was significantly better than another treatment, whereas, looked at critically, the problem was usually one of estimation; what the experimenter really wanted to know was the quantitative difference between the two treatments. Fortunately agricultural experiments were designed to give this quantitative information, and the fact that a few irrelevant tests of significance were made on the results was of no great consequence; but if, as in the present case, the tests themselves were so designed that the quantitative information was not available, the matter was much more serious.

As he saw it, sequential tests gave high precision in a particular region but low precision outside that region. This was exactly what was wanted when testing material to see whether it conformed to a given standard, but it was not what was wanted when testing improvements in apparatus; then one required equal precision over a wide range—in fact, over the whole of the range that was likely to be experienced in practice.

At the end of the paper the author had made a number of statements which he said were intended to be provocative. He, Dr. Yates, did not wish to enter into controversy on these points, beyond stating that he considered that many of these apparently irreconcilable differences of theory depended largely on differences of approach, and were likely to be gradually synthesized into a more general theory. Dr. Bartlett, for example, had already drawn attention to the relationship with the ordinary theory of estimation. Mr. Barnard had raised the question of Bayes's theorem as if this were a heresy which it was dangerous to utter. But in fact in the sequential type of problem there existed a distribution of the means (or other parameters) of the various batches, and it was consequently perfectly reasonable to consider what would happen when this distribution assumed different forms.

MR. ANSCOMBE desired to mention briefly some work with which he had been concerned at the Ministry of Supply following upon the work of which the author had spoken. It was concerned with the first problem he discussed—namely, sampling from a bulk of items to decide whether the proportion of defective articles was allowable. A sequential scheme of this kind was defined, in Mr. Barnard's notation, by two handicap numbers (which at the beginning were denoted by H and H' and later by K and L) and a penalty number b. Furthermore, if the scheme was closed at some maximum sample size, that would be a further defining number.

The work the speaker was referring to had consisted of taking specimen schemes and working out numerically what their properties were—that is to say, their operating characteristics and average sample size curves, using the formulae developed by Mr. Burman, Miss Stockman and Mr. Armitage. Later on a contract for further work was placed with Dr. Hartley of the Scientific Computing Service.

If it was desired to apply one of these sequential schemes in practice, there were two questions on which some guidance was needed. Normally should we choose equal handicap numbers or unequal, and, if unequal, how far should they be unequal? Again, should the sequential test be closed at some maximum sample size, and, if so, how early should it be closed? The charts which Mr. Barnard had himself prepared were for unclosed sequential schemes with equal handicaps. But there was no reason why the handicaps should be equal, nor why the schemes should be unclosed. To try to obtain any general guidance on the type of scheme to use, it was necessary to examine these numerical cases, which furnished some idea empirically of when to use which sort of scheme.

He would not attempt to summarize the results of the investigation, but would mention one point. In discussing the operating characteristic curve, the author said that if drawn on logarithmic probability paper most operating characteristic curves were sufficiently well represented by a straight line. In other words, they were defined by two points, a "safe point" and a "risk point." That was true as a first approximation, and, of course, it was a very convenient fact, but it was not true exactly. Indeed, some operating characteristic curves could look very far from straight lines, and this factor did enter into the question of which type of scheme to use. Suppose one wanted to compare two sequential schemes, say a closed scheme with unequal handicaps and an unclosed scheme with equal handicaps. If the operating characteristic curve were defined by two points, a scheme of the second type could be found exactly equivalent to the first scheme, *i.e.*, having the same operating characteristic. Then it might be found that for one part of the quality range the first scheme was better—*i.e.*, gave lower average sample sizes—and for another part of the range the second scheme was better. To decide which would be better in a particular case it was necessary to consider the process curve and calculate the average amount of sampling to be expected. But the operating characteristic curve was not precisely defined by two points, and therefore any two schemes would always have a slightly different effect on incoming bulks. No two different schemes could have the same operating characteristic curve precisely, and the difference would be great enough to confuse this particular problem.

In order to reach a definite conclusion as to the superiority of one sampling scheme over another, it seemed necessary to take into consideration not only the process curve and the various costs of inspection but also the cost of accepting and the cost of rejecting a bulk of any given quality, so that the whole transaction of the consumer buying from the producer could be costed in a single figure.

The work to which he had referred would be issued shortly from the Ministry of Supply, and he hoped that further work would shortly be completed and published in the usual way.

MR. BOSANQUET, who had been working over somewhat similar tests himself, asked whether the lattice scheme which the author had depicted on the blackboard, while all very well for a "Go" or "Not go" test, could be used if the path were actually measured. Could cumulative error be plotted?

MR. BARNARD, replying to the discussion, thanked the audience for their kind reception of his disjointed exposition, and said that he would deal with most of the questions in writing. One or two points had been made, however, to which he could reply quickly. Dr. Vajda's point about the small batches, when the assumption set out at the beginning of section I of the paper was not true, was actually dealt with in section III under "General Inspection Problems." In particular it was a remarkable result, which should be called Mood's theorem, that when inspection took place by destruction methods from batches and the process was binomial in shape, it was useless to inspect at all with a simple inspection scheme. That emphasized the importance of the precise shape of the P curve.

Concerning Mr. Jones's point as to the possibility of having boundaries which were not parallel but which diverged, he had for some time thought that this would be a fascinating subject for exploration. He had not mentioned it in the paper because it had not been explored, and he wished someone would set about it. Provided the boundaries were parallel, he had shown that the mean sample size was finite, and therefore the probability of going on for ever was nought.

In reply to Dr. Yates and Mr. Bosanquet these sequential methods could be used with tests involving measurements, as well as with tests involving simple classification by attributes. That was not very clearly set out, but it was mentioned. It was possible to set up tests in that form which were based on the cumulative sums of scores. He thought the simplest formula for that sort of thing was obtained by assuming the distribution of scores to be normal and then applying the formula set out on page 10.

Mr. Barnard later added the following comments in writing:

Concerning Dr. Yates's comments, it is quite true that, in testing two designs of an apparatus, we are not usually interested in the mere question whether one design is better than the other. But many practical problems in such connections *can* be reduced to the problem "Should we adopt this design or that?" In other words, we wish to decide between two alternative courses of action. For example, we might consider the adoption of a complicated design rather than a simple one as justified only if the complicated design is twice as good as the simple one, on account of the higher cost of production. But such questions can be answered by sequential methods just as well as by classical methods—even if the experiment itself is used to determine the increase in cost of production as well as the actual improvement in performance—provided there is sufficient forethought. And always, of course, provided the experiment is not like the one on trees which I mentioned in the introduction.

It should also be made clear that the data obtained in a sequential procedure can be used to derive estimates of relevant quantities, provided the proper formulæ are used. The results of Dr. Case and Professor Haldane indicate that sequential methods may even have some advantages here.

With regard to Dr. Bartlett's points on the applicability of the theory of likelihood estimates and the information function, we have to remember that there are really two "sample sizes" involved in sequential procedures. We may take our observations n at a time, and use a statistic T calculated from these n as the basis of our test procedure. To arrive at a decision, we may then have to take N sets, each of n observations. So when we speak of "large-sample theory" we may mean that n is large, or that N is large. If n is large, then there is no doubt that the likelihood theory will apply to the choice of statistic T, subject to the usual conditions. But if N is large, while n is small, I think the likelihood theory will apply only if the sequential test used is a linear one.

In particular, the "binomial" type of test indicated in the paper as a possible alternative to the "sequential t-test" is not based on taking the median as statistic instead of the mean. The function of the observations which is used is not even a continuous one, so that the differential approximations use in the theory of the information function are inapplicable.

Finally, as Mr. Tweedie first pointed out, my original form for the condition that a P.R.S. test should be a linear test was incorrect, and I am grateful to him and to Mr. Lindley for drawing my attention to this. I have taken the liberty of amending my original formula according to a suggestion made by Mr. Lindley and Mr. Armitage, who also drew attention to a mistake in formula (33), and to several minor errors.

Symposium on Autocorrelation in Time Series

Contents

On the Theoretical Specification and Sampling Properties of Autocorrelated Time-Series

By M. S. Bartlett

[Read before the Research Section of the Royal Statistical Society, January 29th, 1946, Dr. J. Wishart in the Chair.]

Contents

1. *Preliminary remarks*

It was suggested at the R.S.S. meeting at which Mr. M. G. Kendall's recent paper (**9**) on the analysis of time-series was read that further discussion should be given to the problem of the arduous labour of calculating correlograms. While I understand that later speakers will describe new methods of calculating auto- or other serial correlations,* the purpose of the present symposium I interpret to be wider, for it is no use knowing how to calculate correlation coefficients if we do not know what they mean. Now, their interpretation depends on two interdependent things: the appropriateness of the theoretical scheme assumed and the magnitude of sampling fluctuations. Kendall, following Yule (**18**), has stressed that for most time-series an autoregressive or autocorrelation scheme is more relevant than the assumption of exact harmonic oscillations detectable by periodogram analysis. He has also pointed out the need for pooling theoretical results on this problem from the various fields of research where it has arisen - *e.g.*, in economics, meteorology, gunnery or in the theory of electrical fluctuations. Nevertheless, as in certain respects I felt he stopped short in his presentation of autocorrelation theory, both generally and on the particular question of sampling errors, my purpose here will be twofold:

(i) I shall amplify some suggestions I made in the discussion on his paper about the sampling errors of a correlogram. The formulæ I obtain are rather crude, and in some cases not new, but they serve to indicate the order of magnitude of the errors.

(ii) I shall try to link up the work of the "English school" with work which it has rather neglected—namely, the important mathematical work developed of recent years on the autocorrelation theory of continuous time-series. I shall not attempt any comprehensive review, but some acquaintance with it seems essential to anyone researching in the theory of time-

* Kendall has employed the term *autocorrelation* to denote a true value of which the observed value is the *serial correlation*. I shall use what I think to be a more logical terminology—viz., serial correlation for any correlation of one time-series with another, and autocorrelation for the particular serial correlation of a series with itself. The standard notation of ρ for a true correlation coefficient, and r for the sample value, will be used.

series. In particular, I shall show that for the second-order oscillatory process considered by Yule and Kendall it leads to a more fundamental grasp of the dual problem of specification and sampling errors.

2. *Standard error formulæ for the autocorrelations of discrete time-series*

It will be convenient to obtain straight away the standard error formulæ appropriate to discrete time-series of the general type discussed by Kendall (**7**, **8**, **9**) and others. Throughout this paper I shall consider only series for which expected values are functions merely of the intervals between the terms of the series; this property characterizes *stationary time-series*, in contrast with time-series which are "evolving" and for which the calculation of serial correlations from successive observations would be rather meaningless. For the simplest *linear* process we have

$$x_{s+1} = \rho x_s + \varepsilon_{s+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where ε_{s+1} is independent of x_s. For convenience, x_s is assumed "standardized"—*i.e.*, the expected value $E\{x_s\} = 0$, and $E\{x_s^2\} = 1$. In the scheme (1) x_{s+1} depends only on x_s, x_{s-1}, . . . through x_s, the partial correlations with x_{s-1}, . . . , when x_s is given, being zero. It is termed a Markoff process, owing to its relationship with the sequences or "chains" of variables studied by Markoff (cf. Khintchine, **10**, p. 604).

(a) *The Markoff process.*—We evaluate by straightforward algebra (cf. **2**) the expressions

$$\begin{aligned} \text{var (var)} &= E\{(\Sigma x_r^2)^2/n^2\} - 1, \\ \text{var (cov)} &= E\{(\Sigma x_r x_{r+s})^2/n^2\} - \rho^{2s}, \\ \text{cov (var, cov)} &= E\{(\Sigma x_r^2)(\Sigma x_r x_{r+s})/n^2\} - \rho^s, \end{aligned}$$

where the summation is over the n observations x_r $(r = 1 \ldots n)$. It is proposed to give only the dominant first term in the expansion of the results in powers of $1/n$; to this order, measurement from the sample mean, or "end effects" (for $s \ll n$), may be neglected. We obtain

$$\left.\begin{aligned} \text{var (var)} &\sim \frac{(\gamma + 2)(1 + \rho^2)}{n(1 - \rho^2)}, \\ \text{var (cov)} &\sim \frac{1}{n}\left[\frac{(1 + \rho^2)(1 - \rho^{2s})}{1 - \rho^2} + \rho^{2s}\left\{2s + \frac{(\gamma + 2)(1 + \rho^2)}{1 - \rho^2}\right\}\right], \\ \text{cov (var, cov)} &\sim \frac{\rho^s}{n}\left[2s + \frac{(\gamma + 2)(1 + \rho^2)}{1 - \rho^2}\right], \end{aligned}\right\} \quad . \quad (2)$$

where γ is the measure of non-normality $E\{x^4\} - 3$. Since to the same order, if r_s denotes the correlation with lag s obtained from the series,

$$\text{var } (r_s) \sim \text{var (cov)} + \rho_s^2 \text{ var (var)} - 2\rho_s \text{ cov (var, cov)},$$

we obtain finally, independently of γ,

$$\text{var } (r_s) \sim \frac{1}{n}\left[\frac{(1 + \rho^2)(1 - \rho_s^2)}{1 - \rho^2} - 2s\rho_s^2\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where $\rho_s = \rho^s$. When s is large enough for ρ_s to be small, this becomes

$$\text{var } (r_s) \sim \frac{1}{n}\left(\frac{1 + \rho^2}{1 - \rho^2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

a result which is more easily obtained from the formula for var (cov).

(b) *The general process.*—The above formulæ are given for reference, but they are of limited use in practice because we usually have to deal with time-series of more complicated character. For the generalization of (1) to a linear autoregressive scheme of any order, or in fact for any time-series, we can, of course, write down formal sums which correspond to the above results for the Markoff process. The most general result we require is (x_s standardized)

$$\text{cov (cov}_s, \text{cov}_{s+t}) = E\{(\Sigma x_r x_{r+s})(\Sigma x_r x_{r+s+t})/n^2\} - \rho_s \rho_{s+t},$$

which to the same order of approximation as before becomes

$$\frac{1}{n} \sum_{v=-\infty}^{\infty} (\rho_v \rho_{v+t} + \rho_{v-s} \rho_{v+s+t} + \kappa_{v,s,t}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where $\kappa_{v,s,t}$ is the first seminvariant involving all the four variables x_r, x_{r+s}, x_{r+v}, $x_{r+v+s+t}$, and is a function of their intervals apart characterized by the suffices v, s, t.

When the x_r are normally distributed, formula (5) reduces to

$$\frac{1}{n}\sum_{v=-\infty}^{\infty}(\rho_v\rho_{v+t}+\rho_{v-s}\rho_{v+s+t}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

a useful result previously given by Daniell (**5**).*

To obtain var (var); var (cov); cov (var, cov), we put $s = t = 0$; $t = 0$; $s = 0$ and $t = s$ respectively in (5) or (6). Thus we obtain from (6):

$$\text{var}\,(r_s) \sim \frac{1}{n}\sum_{v=-\infty}^{\infty}(\rho_v^2+\rho_{v+s}\rho_{v-s}+2\rho_s^2\rho_v^2-4\rho_s\rho_v\rho_{v-s}) \quad . \quad . \quad . \quad . \quad (7)$$

An important special case is when the true value ρ_s has become small. The sampling errors of correlations are then, as noted for the Markoff process, approximately equivalent to those of the corresponding covariances. We obtain from (6) when ρ_w is negligible for $w \geqslant s$,

$$\text{var}\,(r_s) \sim \frac{1}{n}\sum_{v=-\infty}^{\infty}\rho_v^2, \quad \text{cov}\,(r_s, r_{s+t}) \sim \frac{1}{n}\sum_{v=-\infty}^{\infty}\rho_v\rho_{v+t} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Formula (4) is the special case of var (r_s) in (8) when $\rho_s = \rho^s$. It should be noticed that the general term $x_r x_{r+s} x_u x_{u+s+t}$ to be averaged in the original expression for cov(cov) can contribute only one term $\rho_v\rho_{v+t}$ (where $v = u - r$), if s is large enough for the dependence of x_u on x_r to be negligible for $u > \frac{1}{2}s$, and, under this condition, (8) will be true irrespective of the normality condition. They are then equivalent to Slutsky's formula for the autocorrelations of the r_s series when ρ_s is negligible (**15**, equation (5), p. 128).†

However, we can go further than this for a wide class of time-series for which I am using the generic title of *linear processes*. These are characterized by the property that any term x_s is the linear superposition of the effects of a number of independent values ε_w of a random variable ε, so that we may write:

$$x_s = \sum_{w=-\infty}^{\infty} g(s-w)\varepsilon_w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where the function $g(s - w) = 0$ for $w > s$. We have had a simple example of such a linear process in the Markoff process (1). From (9) it follows at once from the properties of seminvariants that the simultaneous cumulant or seminvariant function

$$\begin{aligned} K(\tau_1, \tau_2, \tau_3, \tau_4) &= \log E\{\exp i(\tau_1 x_r + \tau_2 x_{r+s} + \tau_3 x_{r+v} + \tau_4 x_{r+v+s+t})\} \\ &= \sum_{w=-\infty}^{\infty} K_\varepsilon(\tau_1 g(w) + \tau_2 g(w-s) + \tau_3 g(w-v) + \tau_4 g(w-v-s-t)) \quad . \quad (10) \end{aligned}$$

where $K_\varepsilon(\tau)$ is the cumulant function of ε. Hence

$$\left.\begin{aligned} \sigma^2(x) = \text{var}\,(x) &= \sigma^2(\varepsilon)\Sigma_w g^2(w) \\ \text{cov}(x_r, x_{r+s}) &= \sigma^2(\varepsilon)\Sigma_w g(w)g(w-s) \\ \kappa_4(x) &= \kappa_4(\varepsilon)\Sigma_w g^4(w) \\ \kappa_{v,s,t} &= \kappa_4(\varepsilon)\Sigma_w g(w)g(w-s)g(w-v)g(w-v-s-t) \end{aligned}\right\} \quad . \quad (11)$$

and

$$\begin{aligned} \sum_{v=-\infty}^{\infty}\kappa_{v,s,t} &= \kappa_4(\varepsilon)\{\Sigma_w g(w)g(w-s)\}\{\Sigma_u g(u)g(u-s-t)\} \\ &= \gamma(\varepsilon)\text{cov}_s\,.\,\text{cov}_{s+t} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12) \end{aligned}$$

From this last result (12) we readily obtain from the general formula ($\sigma^2(x) = 1$)

$$\text{cov}\,(r_s, r_{s+t}) \sim \text{cov}\,(\text{cov}_s, \text{cov}_{s+t}) + \rho_s\rho_{s+t}\,.\,\text{var (var)} - \rho_s\,\text{cov (var, cov}_{s+t}) - \rho_{s+t}\,\text{cov (var, cov}_s)$$

* I am indebted to Dr. H. E. Daniels for this reference, which has not been generally published.

† In his paper Slutsky also refers to a previous paper "On the standard error of the correlation coefficient in the case of homogeneous coherent chance series" (in Russian), *Transactions of the Conjuncture Institute* **2** (1929), 94. Unfortunately I have not been able to locate this paper anywhere in this country.

the result that for any linear process of the type (9) cov (r_s, r_{s+t}) or var (r_s) are to the present order of approximation *independent of the distribution of* x_s; thus cov (r_s, r_{s+t}) becomes

$$\text{cov}\,(r_s, r_{s+t}) \sim \frac{1}{n} \sum_{-\infty}^{\infty} (\rho_r\rho_{r+t} + \rho_{r+s+t}\rho_{r-s} + 4\rho_s\rho_{s+t}\rho_r^2 - 2\rho_s\rho_r\rho_{r+s+t} - 2\rho_{s+t}\rho_r\rho_{r+s}) \quad . \quad (13)$$

The rather curious results in (8) that the sampling properties of r_s when ρ_s has become small depend on the "variance" and covariances of ρ_t in the correlogram seems sufficient to explain the reluctance of an observed correlogram to damp down to zero with ρ_s, a point which worried Kendall when he came across it empirically. From (8) we see that the standard error of r_s will always be larger than $1/\sqrt{n}$, and that the observed correlogram will preserve a misleading regularity even when ρ_s is zero, the correlogram for neighbouring values of r_s being the "correlogram" of the true correlogram.

For example, let us consider Kendall's artificial series (see 8, Table 3)

$$x_{s+2} = 1 \cdot 1x_{s+1} - 0 \cdot 5x_s + \epsilon_{s+2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

for which

$$\rho_{s+2} = 1 \cdot 1\rho_{s+1} - 0 \cdot 5\rho_s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Kendall, giving values of r_s up to $s = 30$, obtained an r of 0·57 for $s = 25$ (for an n of 65), with $r_{26} =$ 0·56 and $r_{24} = -0·43$, values which appeared unexpectedly high compared with the true values ρ_s, which have effectively dropped to zero after $s = 10$. But from the true values of ρ_s, most easily obtained in succession from (15), and recently given by Kendall (Table II in his Appendix to **18**), we obtain var $(r_s) \sim 2 \cdot 44/n$, and a "correlogram" of the correlogram as shown in Table I.

TABLE I

Correlations σ_t of the correlations ρ_s

t	σ_t	t	σ_t	t	σ_t
1	+0·832	7	−0·118	13	0·015
2	+0·434	8	+0·022	14	−0·027
3	+0·002	9	+0·096	15	0·024
4	−0·286	10	+0·102	16	−0·012
5	0·364	11	+0·071	17	−0·010
6	−0·276	12	+0·019	18	+0·005

If we consider r_s for $s = 11$ to 30, s is no longer small compared with the total number (65) of observations. It is therefore a rather better approximation for each s to consider n as the number of pairs of observations actually correlated (cf. Daniell, 5). This gives for the same range of s an average value of var (r_s) of 0·053. The observed value was computed to be 0·083, with an effective number of degrees of freedom less than 20 because of formula (8). If we suppose that we can treat the terms r_s analogously to terms in the original time-series, but with the correlogram of Table I, for which $\Sigma\sigma_t^2$ (summed over all t) is 3·42, the effective number of degrees of freedom will be more like $20/3 \cdot 42 \sim 6$. A ratio 0·083/0·053 with 6 d.f. would not reach the 5 per cent. significance level. This adaptation of standard tests is admittedly rough, but a test based on the highest absolute value observed, 0·57 (for which $n = 40$, and the effective size of sample of which it is the largest member again about 6), would yield a similar conclusion. Thus it may be concluded that the observed values of r_s have come out a little high, but not significantly so. With correlograms we must evidently take care not to allow the tail to wag the dog!

For a Yule–Kendall process like (14), ρ_s is of the form $\lambda\alpha_1^s + (1 - \lambda)\alpha_2^s$ and $\Sigma\rho_s^2$ theoretically summable; similarly for any more general linear process. But in practice it is often simpler to evaluate $\Sigma\rho_s^2$ directly from the numerical values as above. If the ρ_s are not known, it is, however, meaningless to consider Σr_s^2, since var (r_s) for large n is of order $1/n$, and the series cannot possibly converge. The only valid procedure would appear to be to fit a theoretical scheme containing one or two unknown parameters, such as the autoregressive scheme above, and obtain $\Sigma\rho_s^2$ from the corresponding theoretical correlogram. It may be that a purely autoregressive analysis is sufficient, and this then has the advantage that the usual regression tests of significance, though not exactly applicable, will be approximately valid for n large (see **12**). If, for example, a scheme like (14) were correct, the multiple regression of x_{s+2} on x_{s+1}, x_{s-2}, . . . when x_{s+1} and x_s are

held constant, should be zero. Unfortunately, as Yule and Kendall have pointed out, superposed error complicates such an analysis. Further complications which arise when such an analysis is applied to continuous time-series are discussed in sections 5 and 6.

In some of the preceding variance formulæ for autocorrelations, the effective number of degrees of freedom has been reduced by the factor $1/\Sigma\rho_s^2$. This result may be compared with Yule's factor $1/\Sigma\rho_s$ for the variance of the mean **(19)**, but, unlike the latter factor, it is essentially less than one. In the discussion on Kendall's paper **(9)**, Champernowne appears to have suggested the use of the Yule factor, or at least its value of $(1-\rho)/(1+\rho)$ if the series is a Markoff process, for testing significance in periodogram analysis. But in such an analysis we test the significance of a weighted mean, the weights being the appropriate harmonic coefficients; the factor will correspondingly be a function of these coefficients. It may, it is true, be shown that for a Markoff process the *minimum* value of the factor is $(1-|\rho|)/(1+|\rho|)$, which is equal to the Yule factor if ρ is positive. But it is also not clear what the interpretation of such a test would be. On the null hypothesis that there is no harmonic term we might identify the autocorrelations in this factor with those in the series (with appropriate precautions, as for $\Sigma\rho_s^2$ above). But if there *is* a harmonic term, $\Sigma\rho_s$ even for an infinite series would not converge (cf. Wold **21**, section 17). Unless we adopt the laborious procedure of isolating the residuals for separate study, we are in danger of eliminating the bias of finding harmonic terms when they do not exist at the cost of never finding them when they do exist. And of course it is still important to study the oscillations intrinsic in the autocorrelated series, which has corresponding to its correlogram a " periodogram " of an entirely different character (see section 3 of this paper). We must not throw away the baby with the bath-water !

3. *The specification of continuous time-series by their autocorrelation functions*

It will have been apparent that the discrete nature of our observations in many economic and other time-series does not reflect any lack of continuity in the underlying series. Thus theoretically it should often prove more fundamental to eliminate this imposed artificiality. An unemployment index does not cease to exist between readings, nor does Yule's pendulum * cease to swing. This new conception of continuous random or stochastic processes is still unfamiliar to many people; I owe my personal realization of its importance to Mr. J. E. Moyal, and to his own fundamental contributions to its development **(13)**. The general theory mainly originated with the " Russian school " of Khintchine, Kolmogoroff, Slutsky and others, but has been increasingly studied and applied elsewhere, especially in America and in this country during the war.

To see its relevance to the schemes considered in section 2, suppose first we generalize the autocorrelation for the Markoff process to the *autocorrelation function*

$$\rho(s) = e^{-\mu s}, \ (s \geq 0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where the time-lag s is no longer necessarily an integral multiple of a unit interval of time. This autocorrelation function still preserves the unique property that the partial correlations of x_{t+s} with $x_{t-\tau}$ $(\tau > 0)$ for given x_t are zero.

Mathematically, the properties of continuous processes can be studied through their autocorrelation functions. We must, of course, be careful to confine our attention to permissible autocorrelation functions, and here Khintchine's theorem **(10)** on the " spectrum " or harmonic analysis of the autocorrelation function is relevant. This states that a necessary and sufficient condition for $\rho(s)$ to be the autocorrelation function of a continuous stationary stochastic process is that

$$\rho(s) = \int_0^\infty \cos \omega s \, dF(\omega) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where $dF(\omega)$ represents a " distribution function." For example, if $\rho(s) = e^{-\mu s}$, $(s \geq 0)$, and (since $\rho(s)$ is a symmetric function in s), $\rho(s) = e^{\mu s}$ $(s < 0)$, we can obtain $dF(\omega) = f(\omega)d\omega$ by the inverse relation

$$f(\omega) = \frac{1}{\pi}\int_{-\infty}^{\infty} e^{-\mu|s|} \cos \omega s \, ds = \frac{2}{\pi}\frac{\mu}{\omega^2+\mu^2}$$

* Yule **(18)**; see section 5 of this paper.

Since $f(\omega)$ is a valid distribution function in this case, so is $\rho(s)$ a valid autocorrelation function. It is also known (*e.g.*, Rice, **14**, Part II, where the important mathematical work by Wiener, **20**, on this aspect of the theory is referred to) that the function $f(\omega)$ gives the intensities for different "frequencies" $\omega/2\pi$ corresponding to a harmonic analysis of the original time-series x_t, so that there is a unique relation between the harmonic and correlation analyses of a time-series (for the corresponding relation for discrete series, see Wold *loc. cit.*). The above spectrum for the Markoff process gives a continuous band of frequencies $\omega/2\pi$, thus stressing the possible irrelevance of a standard periodogram analysis for such processes. It is only when the integrated function $F(\omega)$ is a step-function that discrete frequencies and corresponding periods in the classical sense exist.

While the above theory enables us to study various permissible autocorrelation functions, it still appears to me important to set up if possible a more detailed theoretical mechanism to represent a time-series. If we can do this, we not only ensure automatically that the autocorrelation function is valid, but we find what it is, and perhaps obtain also further knowledge about the distributional properties of the process. For we have seen in the case of linear processes that the autocorrelation function does not exhaust the distributional properties unless the process is "normal." General consistency conditions for the higher product-moments are not, as far as I am aware, known. And further our postulated autocorrelation function, while at first sight reasonable, may turn out to be incorrect for the particular process we have in mind. For example, it is common to postulate the next simplest function to (16) for a continuous process as

$$\rho(s) = e^{-\mu s} \cos \lambda s, \ (s \geqslant o) \qquad (18)$$

in the case when oscillations are present, but it is shown in section 5 that this function, while a permissible autocorrelation function, is not correct without modification for the particular second-order process set up.

Coming back, then, to a justification of (16), we could follow the typical argument used in physical applications like the problem of Brownian motion, and assume that (1) still holds with both interval and random increment made infinitesimal. This, however, is too specialized an assumption for the present purpose, for while it leads to (16), it also leads to a normal distribution for x_t (cf., for example, **3**). It may be generalized by supposing that while increments may still be finite, their occurrence is random in time; this assumption (which is related to the homogeneous random process discussed by Cramer, **4**, Ch. 7) still leads to an autocorrelation function of the same form without restricting the nature of the distribution of x_t. This will be more conveniently demonstrated in detail for the second-order process. But first of all I want to record the standard error formulæ corresponding to those given in section 2.

4. *Standard error formulæ for the autocorrelations of continuous time-series*

If we assume that we have a continuous record of x_t, we may evaluate the results corresponding to those in (2) simply by replacing sums by integrals. We have, for example,

$$\text{var (var)} = E\left\{\left(\int_0^T x_t^2 dt\right)^2 / T^2\right\}.$$

(a) *The Markoff process.*—By such methods we obtain for the Markoff process when T is large,

$$\begin{aligned} &\text{var (var)} && \sim \frac{\gamma+2}{\mu T} \\ &\text{var (cov)} && \sim \frac{1}{\mu T} + \rho_s^2\left[\frac{\gamma+2}{\mu T} + \frac{2s}{T} - \frac{1}{\mu T}\right] \\ &\text{cov (var, cov)} && \sim \rho_s\left[\frac{\gamma+2}{\mu T} + \frac{2s}{T}\right], \end{aligned} \qquad (19)$$

where s is no longer necessarily an integer. Hence

$$\text{var } (r_s) \sim [1 - \rho_s^2(2s\mu + 1)]/\mu T \qquad (20)$$

or when ρ_s is small,

$$\text{var } (r_s) \sim 1/\mu T \qquad (21)$$

It may be asked "What is the relevance of such results when in practice we probably have to

take a finite set of observations?" The point is that they do indicate the intrinsic sampling accuracy of a series of length T in contrast with that in the arbitrary number of observations we happen to have made. We can always generate a discrete series as a set of observations made at regular intervals on the continuous series (in general the converse is not true—*e.g.*, if $\rho < 0$ in (1); cf. Wold *loc. cit.*). For the Markoff process the increment ε_{s+1} then becomes the sum of increments in the time-interval $(s, s+1)$. We now have the relation $\rho = e^{-\mu T/n}$, whence $\mu T = -n \log \rho$. Thus from formulæ (4) and (21), as $\rho_s \to 0$, we obtain a relative efficiency in estimating ρ_s from the discrete set n of observations, of

$$E_0 = \frac{1-\rho^2}{(1+\rho^2)\log 1/\rho} \qquad (22)$$

The corresponding ratio from (3) and (20) depends on ρ_s, but in the case $s = 1$, we have

$$E_1 = \frac{1-\rho^2(1+\log 1/\rho^2)}{(1-\rho^2)\log 1/\rho} \qquad (23)$$

The values of E_0 and E_1 are plotted against ρ^2 in Fig. 1.

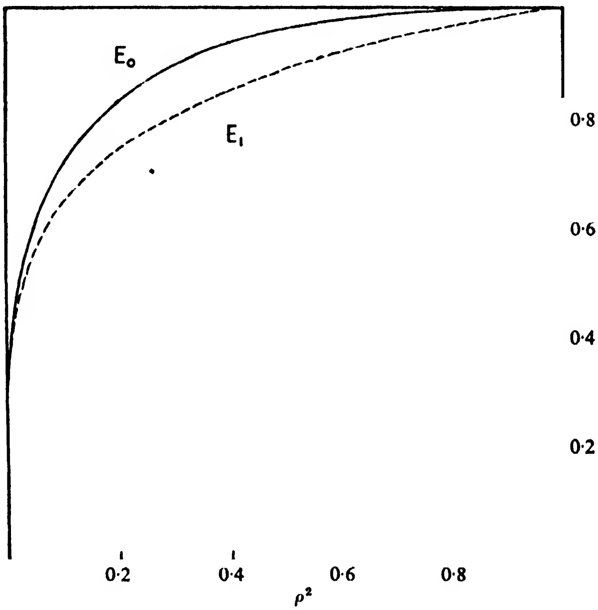

FIG. 1.—Efficiency ratios E_0 ($\rho_s = 0$) and E_1 ($s = 1$) for estimating ρ_s from observations made at regular intervals, plotted against ρ^2, where ρ is the true correlation ρ_1 and the process is a continuous Markoff one.

(b) *The general process.*—For the general process it will be sufficient to note the integral corresponding to (5); this is

$$\text{cov}\,(\text{cov}_s, \text{cov}_{s+t}) \sim \frac{1}{T}\int_{-\infty}^{\infty} (\rho_v\rho_{v+t} + \rho_{v-s}\rho_{v+s+t} + \kappa_{v,s,t})dv \qquad (24)$$

from which other formulæ may be deduced. For a continuous *linear* process analogous to the discrete linear process (9) I shall write formally (cf. the next section)

$$x_t = \int_{-\infty}^{\infty} I_v g(t-v)dv,$$

where $\int^t I_v dv$ represents the total random increment up to time t corresponding to $\Sigma^t \varepsilon_w$ in (9). Then analogously to (10) we have

$$K(\tau_1, \tau_2, \tau_3, \tau_4) = \int_{-\infty}^{\infty} \overline{K}_I(\tau_1 g(u) + \tau_2 g(u-t) + \tau_3 g(u-v) + \tau_4 g(u-v-s-t))du \qquad (25)$$

where $K_I(\tau)$ is the *rate of increase* of the cumulant function of the total random increment $\int^t I_v dv$. From (25) we have formulæ analogous to (11) and (12), so that cov (r_s, r_{s+t}) is again independent, to our order of approximation, of $\kappa_{v,s,t}$. Incidentally we note from (25) that x_t cannot be normal unless the variable I represented in $K_I(\tau)$ is normal—*i.e.*, the increments are intrinsically normal—or else (roughly speaking) the individual increments I_v are sufficiently small and numerous for their sum, in a small interval of time for which $g(t)$ is constant, to have become normal.

The results for the mean and variance of x_t contained in (25) are known as Campbell's theorem; the generalization to other seminvariants has been given by Rice (**14**, sections 1.5 and 3.11), equation (25) above representing a further extension required for the theoretical developments of this paper.

Omitting the term $\kappa_{v,s,t}$, we may from the theory of Fourier transforms write (24) in the alternative form

$$\text{cov}\,(\text{cov}_s, \text{cov}_{s+t}) \sim \frac{2\pi}{T}\int_{-\infty}^{\infty} f^2(\omega)\{e^{i\omega t} + e^{i\omega(2s+t)}\}d\omega \quad . \quad . \quad . \quad . \quad (24a)$$

where $f(\omega)$ was defined in section 3. Specific formulæ for the second-order linear process are recorded later.

5. *Detailed specification of the second-order process*

Coming now to a detailed examination of the continuous second-order process, I shall re-consider the problem which Yule used in his pioneering paper (**18**) as a basis for the second-order difference equation of the type (14). He imagined a swinging pendulum subject to bombardment by boys equipped with peashooters. This problem in another guise is of practical importance, for equally we may think of a sensitive instrument disturbed by impulses of a Brownian motion character (*e.g.*, a galvanometer with suspended mirror whose torsional oscillations are disturbed by impacts from gas molecules).* As in the case of the Markoff process, this form of Brownian motion has usually been studied with the assumption that even in a small interval of time the disturbances are infinitesimal but numerous; the distribution of x_t then becomes normal. But again I shall not impose this restriction here, but leave the distribution of x_t unspecified in general. This allows an exact representation of Yule's problem of the swinging pendulum subject to any type of instantaneous random impulse; and of similar or more complicated processes. Yule's problem I shall, without stopping here to rigorize the argument completely, denote by the equation

$$\ddot{x}_t + \alpha\dot{x}_t + \beta x = I_t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

where dots denote differentiation with respect to the time t. In this equation I_t represents a random impulse function which changes $\dot{x}_t$ discontinuously, but $\ddot{x}_t$ may be regarded as formally defined by (26) in terms of I_t, which is an improper function possessing a proper integral $\int^t I_v dv$.†

The solution of (26) involving I_t is

$$x_t = \int_{-\infty}^{t} I_v\left\{\frac{e^{\mu_1(t-v)} - e^{\mu_2(t-v)}}{\mu_1 - \mu_2}\right\}dv \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

where μ_1 and μ_2 are the roots of $x^2 + \alpha x + \beta = 0$. Hence, if $E\{I_v\} = 0$, $E\{I_u I_v\} = 0$ when $u \neq v$, and $\sigma^2(I)$ is a finite quantity representing the rate of increase of variance of the integrated impulse $\int^t I_v dv$, we obtain

$$E\{x_t x_{t+s}\} = \frac{\sigma^2(I)}{2\alpha\beta}\left\{\frac{\mu_2 e^{\mu_1 s} - \mu_1 e^{\mu_2 s}}{\mu_2 - \mu_1}\right\}, \; (s \geqslant 0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

whence $\sigma^2(x) = \text{var}\,(x) = \sigma^2(I)/2\alpha\beta$ and

$$\rho_s = \frac{\mu_2 e^{\mu_1 s} - \mu_1 e^{\mu_2 s}}{\mu_2 - \mu_1} = \frac{e^{-\frac{1}{2}\alpha s}\cos(\lambda s - \theta)}{\cos\theta}, \; (s \geqslant 0) \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

* My attention has kindly been drawn by both Mr. J. E. Moyal and Mr. P. A. Moran to references (**6** and **1**) on this application.

† This comment suffices to define $\dot{x}_t$ and $\ddot{x}_t$ in terms of (26) and its solution. In other problems $\dot{x}_t$ may be a random quantity whose existence is established only by a wider definition of differentiation (due to Slutsky, **15**; see also Moyal, **13**). On Slutsky's definition, $\dot{x}_t$ is continuous but still not strictly differentiable.

where $\lambda^2 = \beta - \frac{1}{4}\alpha^2$, $\tan\theta = \frac{1}{2}\alpha/\sqrt{\beta}$. Equation (28) shows that if the series has been generated a long time ago, so that the effect of initial conditions has become negligible, the series stabilizes at $\sigma^2(x)$ given by $\sigma^2(I)/2\alpha\beta$ (cf. Kendall, 8, equation (12), for the corresponding result for a discrete process). The formula (30) for ρ_s should be contrasted with (18), and also with Kendall's result for a discrete process (8, equation (13)). It is, I find, not new, having been given in the case of Brownian oscillations by Zernike (22, equation (6), p. 518).*

The corresponding frequency spectrum is obtained by inverting the function ρ_s as

$$f(\omega) = \frac{1}{\pi}\frac{\alpha\beta}{(\omega^2 - \beta)^2 + \omega^2\alpha^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

(cf. 6, equation 2209). It may equivalently be written

$$\frac{1}{\pi}\left\{\frac{\frac{1}{2}\alpha}{(\omega-\lambda)^2 + \frac{1}{4}\alpha^2} + \frac{\frac{1}{2}\alpha}{(\omega+\lambda)^2 + \frac{1}{4}\alpha^2} + \frac{\alpha(\lambda^2 + \frac{1}{4}\alpha^2 - \omega^2)}{[(\omega-\lambda)^2 + \frac{1}{4}\alpha^2][(\omega+\lambda)^2 + \frac{1}{4}\alpha^2]}\right\}$$

where the first two terms correspond to the spectrum of the function (18). For small damping (α small), and $f(\omega)$ considered for positive ω, only the first term of all is large as $\omega \to \lambda$, and the difference between the spectra of (29) and (18) becomes small.

The function ρ_s is a solution of the differential equation

$$\rho_s'' + \alpha\rho_s' + \beta\rho_s = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

which can be obtained directly from (26) by multiplying the equation at time $t + s$ by x_t, and averaging, I_{t+s} being uncorrelated with x_t.

From (29) are obtained explicit formulæ for any of the sampling error expressions given in section 4. One of the most useful is

$$\text{var}\,(r_s) \sim (\alpha^2 + \beta)/\alpha\beta T \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

when ρ_s is small. The following expression for cov $(\text{cov}_s, \text{cov}_{s+t})$, apart from the term involving $\gamma(I)$, is also given for reference. It was obtained from (24) and checked from (24a).

$$\text{cov}\,(\text{cov}_s, \text{cov}_{s+t}) \sim \theta(t) + \theta(2s+t)$$

where

$$T\theta(t)e^{\frac{1}{2}\alpha} = \frac{\alpha^2+\beta}{\alpha\beta}\cos\lambda t + \frac{\alpha^2(5\beta-\alpha^2)}{8\lambda^3\beta}\sin\lambda t + \frac{\alpha t}{2\lambda}\sin\lambda t + \frac{(2\beta-\alpha^2)t}{4\lambda^2}\cos\lambda t \quad . \quad (33)$$

In the "aperiodic" case $\lambda = 0$, we obtain the comparatively simple result from (24a),

$$\text{cov}\,(\text{cov}_s, \text{cov}_{s+t}) \sim -\frac{4}{3}\beta^2\left(\frac{\partial}{\partial\beta}\right)^3 \frac{e^{-\sqrt{\beta}|t|} + e^{-\sqrt{\beta}|2s+t|}}{T\sqrt{\beta}} \quad . \quad . \quad . \quad . \quad . \quad (34)$$

From the exact solution (27) for x_t, we may investigate whether any exact difference equation for x_t exists in place of the differential equation (26). We obtain

$$x_{t+2h} + ax_{t+h} + bx_t = [J]_t^{t+2h} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

where $a = -(e^{\mu_1 h} + e^{\mu_2 h})$, $b = e^{(\mu_1+\mu_2)h}$, and the symbol on the right-hand side denotes a definite integral from t to $t + 2h$ involving I_t, viz.,

$$[J]_t^{t+2h} = \int_{t+h}^{t+2h} I_v\left\{\frac{e^{\mu_1(t+2h-v)} - e^{\mu_2(t+2h-v)}}{\mu_1 - \mu_2}\right\}dv - b\int_t^{t+h} I_v\left\{\frac{e^{\mu_1(t-v)} - e^{\mu_2(t-v)}}{\mu_1 - \mu_2}\right\}dv.$$

Thus the Yule–Kendall relation

$$x_{t+2h} + ax_{t+h} + bx_t = \varepsilon_{t+2h} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

where ε_{t+2h} is a random increment arising subsequent to x_{t+h}, is not exact for the process originally considered by Yule. There must, of course, be some discrete process corresponding to (26), yielding the correct ρ_s solution (29), but this is defined by (35). Multiplying this equation by x_{t-s}, $(s \geqslant 0)$, and averaging, we obtain

$$\rho_{s+2h} + a\rho_{s+h} + b\rho_s = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

an equation which is satisfied by the solution (29). Although (37) is identical with the difference

* Zernike goes on to consider the efficiency of estimation of the mean, a simpler problem than those considered here, but the relevant one in his case, and one, as noted in section 2, recently discussed by Yule (19).

equation for ρ_s given by Kendall, its solution is different because it does not hold for $s = -h$, but only for $s \geqslant 0$, owing to the dependence of x_{t+h} on $[J]_t^{t+2h}$. In fact

$$E\{x_{t+h}[J]_t^{t+2h}\} = b(\rho_h - [\rho_{-h}]) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

where $[\rho_{-h}]$ denotes the analytic continuation of ρ_h for negative h, and is unequal to the value $\rho_h = \rho_{-h}$ except in the limiting case when $\alpha = 0$. Thus in general an autoregression analysis of the type (36) will yield values of a and b inappropriate for the process (35); this is so *even in the limit when h becomes infinitesimal.* If $\lambda/2\pi$ is the "frequency," I find the limiting value of λ estimated from (36) as $\sqrt{(\beta - \frac{1}{9}\alpha^2)}$ instead of $\sqrt{(\beta - \frac{1}{4}\alpha^2)}$. The difference will be negligible as α becomes small, but if $\alpha^2 \to 4\beta$ gives rise to a spurious frequency when the true frequency is zero. Correspondingly the "period" or "wavelength" $2\pi/\lambda$, which in general, at least for small h, will be underestimated,* will remain finite even when the true period is infinite.

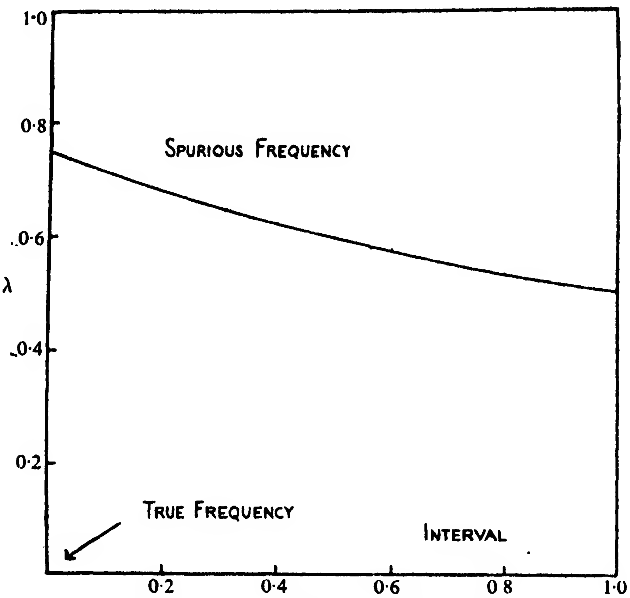

FIG. 2.—Values of λ estimated from the Yule–Kendall finite difference equation plotted against the observational interval, when the process is a continuous second-order one with true frequency $\lambda/2\pi = 0$ (and $\alpha = 2$).

To check this, I considered the solution of (26) in this aperiodic case, for which $\beta = \frac{1}{4}\alpha^2$. The solution is

$$x_t = \int_{-\infty}^{t} I_v e^{-\frac{1}{2}\alpha(t-v)}(t - v)dv,$$

$$\rho_s = (1 + \tfrac{1}{2}\alpha s)e^{-\frac{1}{2}\alpha s}, \ (s \geqslant 0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

If for such an autocorrelation, we attempted to estimate the frequency by means of (36), we should obtain the spurious frequency shown in Fig. 2, where λ is plotted as a function of the value αh, where h is the interval between observations. For definiteness α was taken to be 2, so that the limiting value of λ is $\frac{1}{3}\sqrt{5} = 0{\cdot}745$.

The invalidity of (36) raises the question whether any other finite difference equations (apart

* This bias appears related to that obtained by Spencer Smith (17), who considered continuous periodic time-series subject to independent disturbances in amplitude, phase and trend. I am doubtful, however, of the possibility of analysing a disturbed oscillatory series into components corresponding to independent disturbances of this kind, when for natural disturbances of the type considered here the effects are necessarily related. From his concluding remarks Spencer Smith appears to recognize these limitations of his method.

from the relation (35)) exist for the process (27). We have seen that for this process $\dot{x}_t$ exists as well as x_t; in fact, from (27) we have

$$\dot{x}_t = \int_{-\infty}^{t} I_v \left\{ \frac{\mu_1 e^{\mu_1(t-v)} - \mu_2 e^{\mu_2(t-v)}}{\mu_1 - \mu_2} \right\} dv \qquad (40)$$

whence

$$\left.\begin{aligned} E\{\dot{x}_t^2\} &= \beta E\{x_t^2\} \\ \rho(\dot{x}_t, \dot{x}_{t+s}) &= \frac{\alpha}{\beta}\frac{\partial \rho_s}{\partial s} + \rho_s \\ \rho(\dot{x}_t, x_{t+s}) &= -\rho(x_t, \dot{x}_{t+s}) = -\frac{1}{\sqrt{\beta}}\frac{\partial \rho_s}{\partial s} \end{aligned}\right\} \qquad (41)$$

We obtain the *simultaneous* pair of first-order difference equations in x_t and $\dot{x}_t$,

$$\left.\begin{aligned} \dot{x}_{t+h} - \frac{E\{\dot{x}_{t+h}\dot{x}_t\}}{E\{\dot{x}_t^2\}}\dot{x}_t - \frac{E\{\dot{x}_{t+h}x_t\}}{E\{x_t^2\}}x_t &= [G_1]_t^{t+h} \\ x_{t+h} - \frac{E\{x_{t+h}\dot{x}_t\}}{E\{\dot{x}_t^2\}}\dot{x}_t - \frac{E\{x_{t+h}x_t\}}{E\{x^2_t\}}x_t &= [G_2]_t^{t+h} \end{aligned}\right\} \qquad (42)$$

where

$$E\{[G_1^2]_t^{t+h}\} = \beta\sigma^2(x)\left\{1 - \frac{1}{\beta}\left(\frac{\partial \rho_h}{\partial h}\right)^2 - \left(\frac{\alpha}{\beta}\frac{\partial \rho_h}{\partial h} + \rho_h\right)^2\right\},$$

$$E\{[G_2^2]_t^{t+h}\} = \sigma^2(x)\left\{1 - \frac{1}{\beta}\left(\frac{\partial \rho_h}{\partial h}\right)^2 - \rho_h^2\right\},$$

$$E\{[G_1G_2]_t^{t+h}\} = \alpha\sigma^2(x)\left(\frac{\partial \rho_h}{\partial h}\right)^2.$$

As $h \to 0$, these two equations reduce respectively to (26) and to $\dot{x}_t = Lt(x_{t+h} - x_t)/h$, showing that for h small enough it is sufficient to consider the formal equation (26).

6. *The estimation problem; theoretical information available on the unknown parameters*

Formally the estimation problem for the second-order process is simple, since it is linear in the unknowns α and β. We obtain the least-square estimates α_e, β_e, where

$$\int_0^T (x_t\ddot{x}_t + \alpha_e x_t\dot{x}_t + \beta_e x_t^2)dt - \int_0^T (\dot{x}_t\ddot{x}_t + \alpha_e \dot{x}_t^2 + \beta_e \dot{x}_t x_t)dt = 0$$

or since $\int_0^T x_t\dot{x}_t dt/T \to 0$ as T increases, we have

$$\left.\begin{aligned} \alpha_e &\sim -\int_0^T \dot{x}_t\ddot{x}_t dt \Big/ \int_0^T \dot{x}_t^2 dt \\ \beta_e &\sim -\int_0^T x_t\ddot{x}_t dt \Big/ \int_0^T x_t^2 dt \sim \int_0^T \dot{x}_t^2 dt \Big/ \int_0^T x_t^2 dt \end{aligned}\right\} \qquad (43)$$

We know further that these least-squares estimates, which would have minimum standard errors in orthodox regression analysis, will have asymptotically minimum errors in the present case, irrespective of the distribution of I_t (cf. **12** and section 2 of the present paper), given by

$$\left.\begin{aligned} \text{var}(\alpha_e) &\sim \sigma^2(I) \Big/ \int_0^T \dot{x}_t^2 dt \sim 2\alpha/T \\ \text{var}(\beta_e) &\sim \sigma^2(I) \Big/ \int_0^T x_t^2 dt \sim 2\alpha\beta/T \end{aligned}\right\} \qquad (44)$$

In some problems, where a continuous track of the time-series is available (in the case of torsional Brownian oscillations continuous records from an oscillating mirror system are reproduced in **6**, Fig. 79) it is possible that direct optical or electrical devices could be invented to measure the quantities occurring in (43), where it should be noted that owing to the existence of $\ddot{x}_t$ being only formal, $\int_0^T \dot{x}_t\ddot{x}_t dt$ is to be interpreted as $\underset{h \to 0}{Lt}\frac{1}{h}\int_0^T (\dot{x}_t\dot{x}_{t+h} - \dot{x}_t^2)dt$. In other cases, the formulæ (44) are a gauge by which the efficiency of any actual method of estimation used can be investigated.

The principles involved are best illustrated first for the simpler Markoff process, since we have seen that the use of discrete series for the second-order process raises special difficulties.

For the Markoff process we have corresponding to the equation (26) for the second-order process, the formal equation

$$\dot{x}_t + \mu x_t = I_t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

whence

$$\left.\begin{aligned} x_t &= \int_{-\infty}^{t} I_v e^{-\mu(t-v)} dv \\ \sigma^2(x) &= \sigma^2(I)/2\mu \\ \rho_s &= e^{-\mu s}, \ (s \geqslant 0) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46)$$

The least-squares estimate of μ is

$$\mu_e = -\int_0^T x_t \dot{x}_t dt \Big/ \int_0^T x_t^2 dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (47)$$

where

$$\text{var}\,(\mu_e) \sim \sigma^2(I) \Big/ \int_0^T x_t^2 dt \sim 2\mu/T \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

In (47) $\dot{x}_t$ is to be replaced by $(x_{t+h} - x_t)/h$ but if we do not proceed to the limit $h = 0$, we are obviously estimating μ by means of the autocorrelation ρ_h. Thus for finite h, bias may be intro-

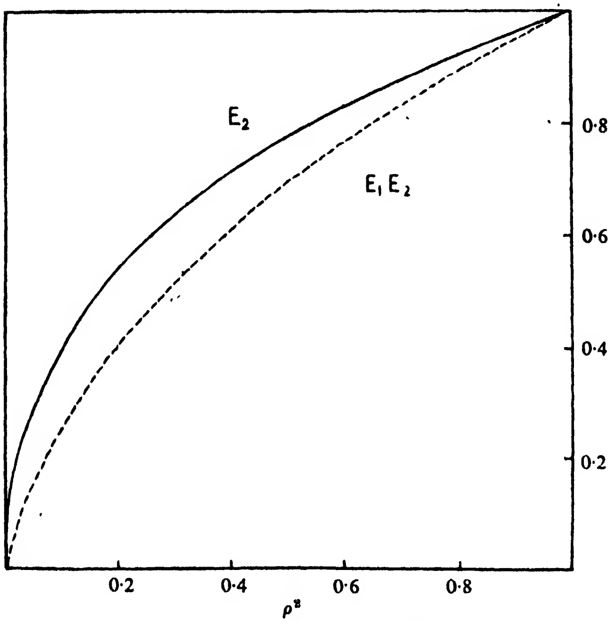

FIG. 3.—Efficiency ratios: (i) E_2 and (ii) E_1E_2, plotted against ρ^2 ($\rho = \rho_h$), for estimating the unknown parameter in a continuous Markoff process from the autocorrelation obtained (i) from a continuous record, (ii) from observations made at intervals h.

duced if we keep to formula (47), and it will be more consistent to use the formula for ρ_h directly—*i.e.*,

$$h\mu_e = -\log r_h \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (49)$$

For this estimate, if r_h were still obtained from a continuous record as $\int_0^T x_t x_{t+h} dt \Big/ \int_0^T x_t^2 dt$, we have var $(\mu_e) \sim$ var (r_h/ρ_h) or from (20), for any distribution of I_t,

$$\text{var}\,(\mu_e) \sim [1 - \rho_h^2(2h\mu + 1)]/\mu T \rho_h^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (50)$$

The ratio of (48) to (50) can be written as

$$E_2 = \frac{\frac{1}{2}\log^2 1/\rho^2}{\log \rho^2 - 1 + 1/\rho^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

which is plotted in Fig. 3. If further r_h were obtained from a discrete set of observations with interval h, we should have var (r_h) given by (3) instead of by (21), and the overall efficiency is reduced to E_1E_2, where E_1 was shown in Fig. 1. E_1E_2 is also plotted in Fig. 3; it will be seen that the fall in efficiency as h increases is fairly rapid.

Thus in fitting the Markoff process (45) with finite h we revert from the regression estimate to a consistent estimate obtained from the first available autocorrelation in the correlogram. The remainder of the correlogram would be used in conjunction with the known magnitude of its sampling errors to consider the adequacy of the fit.

Let us try to consider now the appropriate procedure for the second-order process. We first of all make the inevitable substitution $(\dot{x}_{t+h} - \dot{x}_t)/h$ for $\ddot{x}_t$. If the nature of the observations prohibits the direct use of $\dot{x}_t$, we further substitute $(x_{t+k} - x_t)/k$ for $\dot{x}_t$, where for the moment k ($\leqslant h$) is not assumed equal to h. The limiting estimate of β presents no difficulty, the regression estimate becoming

$$\beta_e \sim 2(1 - r_k)/k^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (52)$$

this being valid for small k as can be seen from the expansion

$$\rho_k = 1 - \tfrac{1}{2}\beta k^2 + \tfrac{1}{6}\alpha\beta k^3 \ . \ . \ . \ , (k \geqslant 0).$$

But for α_e the regression estimate becomes

$$\alpha_e \sim \frac{r_{h+k} + r_{h-k} - 2(r_h + r_k - 1)}{2h(1 - r_k)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (53)$$

whereas for both h and k small the corresponding expression in ρ is equal to $\alpha(1 - \tfrac{1}{3}k/h)$. Thus (53) unless corrected would lead to an under-estimate even when $h \to 0$ unless k/h is small, the $\tfrac{2}{3}$rds factor multiplying α being reminiscent of the limiting bias in λ arising from the use of the finite difference equation (36). In practice if r_s is available only for integral values of s, we should use the functional relations of ρ_s to α and β directly to give consistent estimates of α and β, analogously to the procedure for the Markoff process, but the above point is important because it throws some doubt on the efficiency of taking the first two autocorrelations r_1 and r_2. All we know from (53) is that this limiting formula is not valid unless k/h is small. The direct use of the functional relations for ρ_s must therefore for h small correspond to a correction for bias which in the case of $k = h$ is a multiplying factor of $1\tfrac{1}{2}$. Unless the variance of the expression in (53) also alters as k increases to h the efficiency of the estimate would not be greater than 4/9.

A complete answer to this problem must await a detailed investigation of the asymptotic efficiency of the functional use of r_1 and r_2, using their variance and covariance relations (obtainable from (33)). So far I have carried this out only in the special but rather important case of small damping (α small). The formula for cov (r_s, r_{s+t}), for which the general form in terms of cov (cov, cov), etc., was given in section 2, becomes in this case

$$\frac{\operatorname{cov}(r_s, r_{s+t})}{\alpha} = \frac{s(s+t)\cos\lambda t}{2} + t\sin\lambda t + \frac{(2s+t)\sin\lambda(2s+t)}{4\lambda} + \frac{\sin\lambda s\sin\lambda(s+t)}{2\lambda^2} \quad . \quad (54)$$

From the relations for var (α_e) and var (β_e)—*e.g.*,

$$\operatorname{var}(\alpha_e) \sim \frac{\left(\frac{\partial\rho_s}{\partial\beta}\right)^2 \operatorname{var}(r_{s+t}) + \left(\frac{\partial\rho_{s+t}}{\partial\beta}\right)^2 \operatorname{var}(r_s) - 2\left(\frac{\partial\rho_s}{\partial\beta}\right)\left(\frac{\partial\rho_{s+t}}{\partial\beta}\right)\operatorname{cov}(r_s, r_{s+t})}{\left(\frac{\partial\rho_s}{\partial\beta}\frac{\partial\rho_{s+t}}{\partial\alpha} - \frac{\partial\rho_s}{\partial\alpha}\frac{\partial\rho_{s+t}}{\partial\beta}\right)^2}.$$

I established after somewhat tedious algebra that the formulæ (44) still hold when α and β are estimated from r_s and r_{s+t} *for any small values of s and t;* thus, at least in the case of α small, the use of the first two autocorrelations r_1 and r_2 for estimating α and β is justifiable.

The importance of the form of the autocorrelation function as $s \to 0$ is worth stressing even if superposed error is present, for while the latter would vitiate the above analysis unless allowed for, its effect on the correlation function in this region may itself help to indicate its character. Thus a random and entirely independent error superposed on each observation depresses the correlation function a finite amount at s near zero, this being the efficient estimate of its magnitude approximated to in practice by considering observational differences; a superposed Markoff process, which might also be a form of error for a continuous process, will lead to a function with a finite slope at $s = 0$. The further terms in the expansion of ρ_s depend, as we have seen, on the values of β and α (in that order) in the second-order oscillatory process. The loss of efficiency if an error effect as well as the constants of the main process have to be estimated from values of ρ only available for integral values of s will evidently be considerable.

Application to Wolfer's sunspot numbers.—In view of Yule's development of the finite difference equation with specific regard to the analysis of the Wolfer sunspot numbers, it seems desirable (without attempting here any complete discussion of these figures) to illustrate the present theory on the same data. Following Yule, I have assumed first that the series of annual numbers quoted might be represented by a second-order process. The two unknown constants are now estimated from the first two correlations r_1 and r_2, this being fairly rapidly done by an interpolatory method. The result is given in Table II. While I have stressed that the large standard errors for r_s when ρ_s is small allow comparatively large departures from expectation, the estimated damping factor appears excessive, being even greater than in Yule's analysis. Yule suggested that the data were effected by random observational errors, and while this is not very apparent from the annual averages he depicts, it is much more evident in the original quarterly figures (see **11**, Fig. 1). For comparison I include also therefore an analysis for Yule's smoothed figures. Of course the use of averages and graduated figures is highly dangerous in analysing time-series for periods, and it would seem more satisfactory to use the original quarterly figures with appropriate inclusion in the estimation equations of the effect of any observational error. However, the analysis for the smoothed figures is of some interest. The estimated period still comes out a little lower than that usually accepted, but the discrepancy appears trivial compared with the estimated period's standard error of the order of 11 per cent. The asymptotic formula used, corresponding to 100 per cent. efficiency of estimation (which is certainly not reached), was

$$\text{var}\,(\beta_e - \tfrac{1}{4}\alpha_e^2) \sim \alpha(2\beta + \tfrac{1}{2}\alpha^2)/T \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (55)$$

this formula follows from the variances of the efficient estimates of α and β, and from their zero correlation.

TABLE II

Autocorrelations of Wolfer's sunspot numbers

s	Annual averages			Graduated averages		
	Observed (Yule)	Theoretical (Yule)	Theoretical (present theory)	Observed (Yule)	Theoretical (Yule)	Theoretical (present theory)
1	0·8112	(0·8112)	(0·8112)	0·8407	(0·8407)	(0·8407)
2	0·4340	(0·4340)	(0·4341)	0·4714	(0·4714)	(0·4713)
3	0·0316	0·0513	0·0855	0·0470	0·0397	0·0605
4	−0·2645	−0·2154	−0·1290	−0·2641	−0·3181	−0·2562
5	−0·4041	−0·3228	−0·1990	−0·4043	−0·5139	−0·4091
	$\alpha_e = 0{\cdot}3160$, $\beta_e = 0{\cdot}6158$ $\sigma(\alpha_e) \sim 0{\cdot}085$, $\sigma(\beta_e) \sim 0{\cdot}059$ $\sigma(r_s) \sim 0{\cdot}129$, $(\rho_s = 0)$. Estimated period 10·2 yrs. ± 1·7 Cf. estimated period (Yule) 10·6 yrs.			$\alpha_e = 0{\cdot}1593$, $\beta_e - 0{\cdot}5811$ $\sigma(\alpha_e) \sim 0{\cdot}060$, $\sigma(\beta_e) \sim 0{\cdot}036$ $\sigma(r_s) \sim 0{\cdot}152$, $(\rho_s = 0)$. Estimated period 10·8 yrs. ± 1·2 Cf. estimated period (Yule) 11·2 yrs.		

The present analysis does not refute Yule's original analysis, the bias noted in section 5 being apparently negligible owing to the small damping for this series, and the estimated period actually less than Yule's for both ungraduated and graduated data. The apparent adequate fit (for the graduated series) does not, of course, prove that the theoretical model is correct, but it does place the onus of proof on those who claim more complicated schemes or more accurate estimates to provide sampling errors and tests in support of their claims.

The alternative suggestion by Yule that the sunspot series might represent the square x_t^2 of the amplitude x_t of an oscillating series rather than x_t itself is one which seems to merit further investigation, but the following notes indicate the difficulty of handling such a theory.

(i) The autocorrelation function $\rho_s(x^2)$ is no longer independent of the nature of the distribution of I_t.

(ii) The autocorrelation function for various α may have a discontinuity at $\alpha = 0$, being given by $\cos 2\lambda s$ when $\alpha = 0$, and by $\rho_s^2(x)$ for $\alpha \neq 0$ for I_t normal.

(iii) The expected value of $r_s(x^2)$ as T increases converges more and more slowly to $\rho_s(x^2)$ as α approaches 0.

These comments show that no simple x_t^2 theory, such as assuming the disturbances I_t to be normal, will fit the observed autocorrelations, which have both positive and negative values.

7. *Concluding remarks*

Before I make way for other speakers, let me anticipate some obvious limitations of the above theory. Some reflect the rudimentary state of our knowledge, as, for example, the use of standard errors of serial correlation coefficients which approach unity. Here I would note that no simple transformation is available to convert to a more suitable scale, as for ordinary correlation coefficients, but that these particular formulæ have been used only in conjunction with the asymptotic regression theory, and will tend to have the same approximate validity as the latter. The extent to which this "large-sample" theory can be used is clearly a matter for further investigation; it will depend very much on the nature of the time-series, for we have seen that the extent to which we can estimate expected values accurately depends on the serial relations between the observations. For example, with the second-order process the less frequent the disturbances the longer the series we have to take before our data can be representative. This reflects theoretical boundaries to our statistical analysis (this is not peculiar to time-series) in that we cannot always hope to distinguish empirically between, say, an exact harmonic series and one with very infrequent disturbances. The importance of specifying when possible the theoretical form of the process has been stressed in this paper, and an attempt made to develop a reasonably logical interplay of theoretical structure and corresponding sampling theory for some typical time-series. Thus I hope I have tied up a few of the loose ends in this straggling subject; I am conscious of many more still left to trip over.

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SOME INSTRUMENTS FOR THE ANALYSIS OF TIME SERIES AND THEIR APPLICATION TO TEXTILE RESEARCH

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I. INTRODUCTION

The analysis of oscillatory time series has received considerable attention from statisticians during the last few years. The work described in this paper was completed before these more recent developments. It will, nevertheless, I think, be interesting as an illustration of the use of the periodogram and correlogram methods as tools in experimental physical research, and will serve to call attention to some of the questions, still outstanding, which the physicist would like to see answered.

The large numbers of periodogram and correlogram analyses required for this work would not have been possible without instruments to carry out the analyses rapidly and automatically. As the instruments may be useful to others working on time series, I shall commence with a brief account of each instrument, sufficiently complete to enable its advantages and disadvantages to be appreciated. For working details reference should be made to the original papers.

II. INSTRUMENTS FOR THE ANALYSIS OF TIME SERIES

1. *The Grating Periodograph* [2]

This is an optical method of performing the periodogram analysis of a series of observations. The observations to be analysed are plotted and the area under the curve is made white on a black background, as shown in Fig. 6. A curve on thin paper may either be used directly in the periodograph or it may be photographed on to a process plate to obtain greater transparency.

The curve is set up in a vertical plane along the line PT_3, Fig. 1, with the time axis horizontal, and illuminated from behind by diffused light. Parallel to the curve is a grating consisting of a series of parallel equidistant vertical slits, and beyond the grating is a vertical ground-glass screen. The arrangement is shown in perspective in Fig. 2, in which, however, the screen is shown inclined. To return to Fig. 1, if the curve is uniformly illuminated and its ordinates are small compared with its distance from the ground-glass, the illumination at B on the screen through the slit D is proportional to the ordinate of the curve at T_1, that through the slit D_2 to the ordinate at T_2, and so on. The total illumination at B is therefore proportional to the sum of a number of equidistant ordinates of the curve. The spacing of these ordinates is given by

$$q = \frac{PR}{QR} \cdot D_1D_2 = \frac{v+u}{v} s \; . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (1)$$

where s is the spacing of the grating.

If q corresponds to a peak on the periodogram, the screen is crossed by a series of alternate light and dark fringes, but for other values of q the illumination is nearly uniform or the fringes are faint. The formation of the fringes is seen more clearly in another way; if a period of length T_1T_2 is present, and its maxima are T_1, T_2, T_3, then there will be a bright band at B, whereas at a neighbouring point C, which receives light from the minima, the illumination is less, while at B' it is again a maximum. The positions of the peaks of the periodogram can thus be observed by measuring the positions of the grating and screen which made the visibility of the fringes a maximum, and calculating the length of the period from equation (1). For this purpose the curve and screen are fixed on an optical bench, and the grating mounted on a sliding stand between them. It is an advantage to incline the screen as shown in Fig. 2, for it then cuts through the planes corresponding to the different values of q, and each set of fringes appears as an apparently narrow corrugated band stretching horizontally across the screen. It is then only necessary to move the grating until the band of fringes is bisected by a horizontal line on the screen.

In this form the periodograph carries out the first part of the periodogram analysis—that is, the addition of equally spaced ordinates. Periods whose lengths are submultiples of p will therefore also produce fringes for the same setting of the grating. These higher-order fringes can be

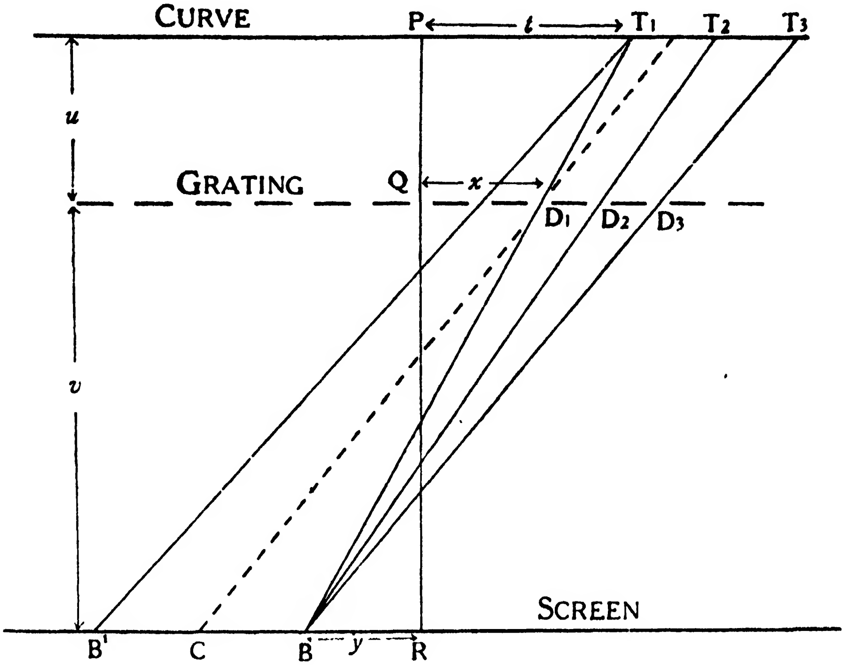


FIG. 1.
Plan of Grating Periodograph.

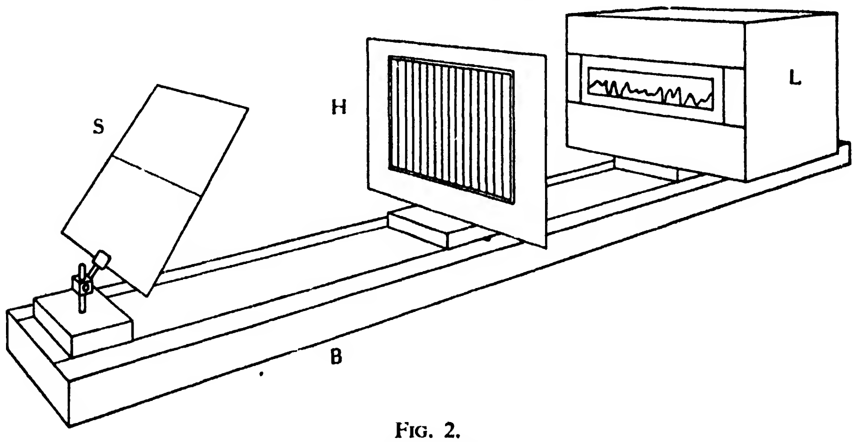


FIG. 2.
Grating Periodograph.

eliminated by the use of a harmonic grating—*i.e.*, a grating for which the light transmitted is proportional to $(1 + a \cos 2\pi x/s)$. Such gratings can be prepared by photographing sine curves in the same form as Fig. 6, with a short focus cylindrical lens placed in front of the camera lens. With a harmonic grating the periodograph performs the complete periodogram analysis, except,

of course, that the amplitudes of the periods can only be assessed qualitatively from the visibility of the fringes. These amplitudes can, however, either be calculated from the original data with the saving of the labour of calculation on all the trial periods which do not correspond to peaks on the periodogram, or they can be measured on the correlation periodograph described in the next section.

It will be seen from Fig. 1 that, if the periodograph analysis is to include the whole of the curve, the width (d) of the grating must be greater than $l \div (1 + u/v)$, where l is the length of the curve. When the grating is narrower than this, the analysis extends only over a length $d(1 + u/v)$ of the curve. By screening the sides of the grating and sliding the curve lengthways to bring each fringe in turn on to a vertical line on the (vertical) screen, the mean phase of the period in successive overlapping portions of the curve can be determined. This corresponds to Schuster's method of secondary analysis; it was used in the work described below in the extreme form when all the grating is screened except the central line.

Since the length of the trial period can be varied continuously, the accuracy with which the

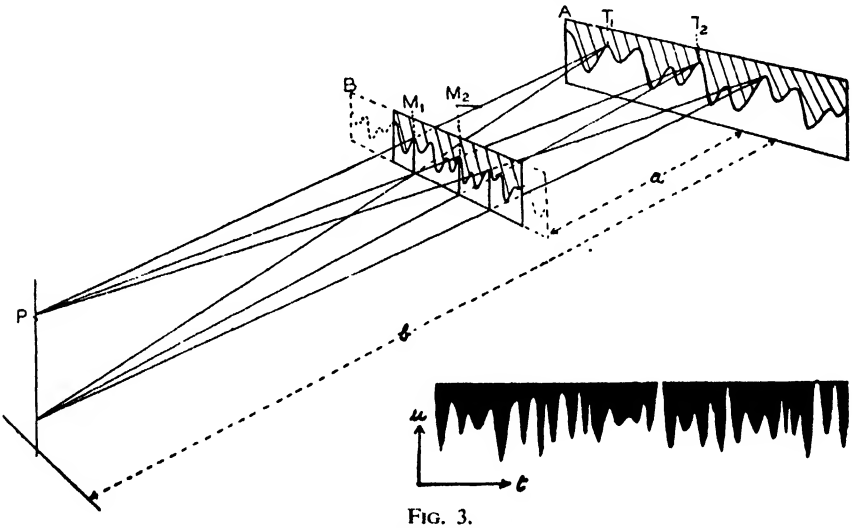


FIG. 3.
Correlation Periodograph.

periodograph determines the position of a peak is probably slightly greater than that of the periodograph. As with the periodograph, the width of the band of fringes corresponding to a period of length p corresponds to a change in $1/p$ of $1/l$, where l is the length of the curve. Experiment shows that the middle of the band can be fixed to within about a twentieth of its width, so that the error in $1/p$ is $1/20\ l$.

The dimensions of the instrument are as follows. The curve and screen are fixed 50 cm. apart, and the curve can be any size up to 20 × 5 cm. Three gratings, 10 × 15 cm. of spacings 4, 1·5 and 0·75 mm. containing 37, 100 and 200 lines, respectively, are used.

2. *The Correlation Periodograph*

In the correlation periodograph, due to Martindale,[9] the grating of the Grating Periodograph is replaced by a replica of the curve on a reduced scale. The arrangement is shown diagrammatically in Fig. 3. If the curve A is illuminated by diffused light, fans of rays through corresponding ordinates of the two curves, such as those at T_1 and M_1, T_2 and M_2, all intersect on a line through P parallel to the ordinates and distant b from A, where $b/(b - a) = C$ is the ratio of the scale of curve A to that of curve B. If $\phi(t)$ the ordinate of curve A at t, the total amount of light falling on P is therefore proportional to

$$\int_0^l \phi^2(t)\,.\,dt$$

If the curve B is displaced a distance s, the amount of light is proportional to

$$\int_0^t \phi(t) \cdot \phi(t+s)dt,$$

so that by displacing the curve B known amounts the correlogram is obtained.

In the actual instrument a narrow vertical slit long enough to collect all the light passing through the highest ordinates of the curves A and B is placed at P. The light passing through the slit is concentrated on to a photo-cell by means of a cylindrical lens and a large condensing lens. The photoelectric current is amplified and measured on a galvanometer. The measurements give accurate relative values of $(1 - r_s)$; to obtain absolute values it is necessary to calibrate the instrument by replacing the curves in turn by rectangular apertures of known dimensions. The present arrangement of the instrument does not, however, permit this to be done with sufficient accuracy, because of fluctuations in the voltage on the lamps illuminating the first curve. During the determination of the correlogram these fluctuations are almost cancelled out by a compensating beam of light from the lamps which fall on to a second photo-cell. This cell and that measuring the correlogram are in the opposite arms of a bridge circuit, so that the deflections of the galvanometer indicate the amount of out of balance of the bridge. Unfortunately the compensating beam cannot be used during the calibration. To obtain absolute values would either require a controlled voltage on the lamps or the use of a null method similar to that adopted by Gray [6] in his photo-electric integraph. The instrument should then be sufficiently accurate for all practical purposes—in fact, the maximum error in the correlation coefficient should be about 0·01. As absolute values were not essential in the work on cotton slivers and rovings, these refinements of the Correlation Periodograph have not yet been carried out.

It will be noticed that, if the curve B, Fig. 3, is replaced by a sine curve, the periodograph can be used for determining the amplitudes of the periods in a periodogram analysis. The length of the trial period can be altered by using sine curves of different wave-lengths and by adjusting the distance a between the curves. The most rapid procedure would be to determine the positions of the peaks on the periodogram by means of the Grating Periodograph and to use the Correlation Periodograph to measure their amplitudes.

Periodogram and correlogram analysis by these optical methods is very rapid once the curves have been prepared. The time taken to plot a complete correlogram depends on the number of serial correlations it is required to observe; but since each correlation can be observed easily in 5 seconds, the time, including calibration and calculation of absolute values, is not likely to exceed an hour.* When the original data are in numerical form the plotting and blackening of the curves is the most laborious part of the process, but, even so, the time taken is negligible compared with that needed for arithmetical computation. A fairly simple machine would, however, enable numerical data to be photographically plotted on to process plates. In the study of the variations of thickness of cotton slivers and rovings the curves are obtained directly in the required form on a photographic recorder.

3. *The Planimeter Integrator* [3]

An alternative instrument for calculating the correlogram is shown in Fig. 4. It makes use of an integrating wheel rolling on the surface of a rotating disc as shown in the plan, Fig. 4. The integrating wheel, X, is a planimeter wheel (4 cm. diam.) with a scale and vernier reading to 1/1000 revolution, and is held in a frame attached to the rod R, which is free to slide in a direction parallel to the axis of rotation of X, while X rests on the surface of the disc D. If D rotates at uniform speed and the displacement of X from the centre of D at time t is x, X will integrate xdt. A second horizontal disc, C, is supported by a vertical spindle passing through bearings fixed to the frame of the lower wheel, and rests on the upper side of the rim of X at a fixed distance from the centre of C, being carried along with X as the rod R is displaced. The rotation of C is thus proportional to that of X. A second planimeter wheel, Y, rests on the upper surface of C, and is fixed relatively to D in such a position that it is over the centre of the upper disc when X is over the centre of the lower. Upon displacement of R, Y is thus the same distance, x, from the

* This applies to the instrument in its present form, in which the deflections of the galvanometer or the setting of the compensating beam have to be read and plotted. It could, however, easily be made to draw the correlogram directly, and the time would then be reduced to, probably, about a quarter of an hour.

centre of C as X is from the centre of D, and its rotation is therefore proportional to x^2. It thus integrates x^2dt. To obtain correlation coefficients, the rod R is pivoted to the mid point of a lever, not shown in Fig. 4, the ends of which are displaced distances equal to the observations x_t, x_{t+s}. The displacement of R is then $\frac{1}{2}(x_t + x_{t+s})$ and the upper wheel Y integrates $(x_t + x_{t+s})^2 = x_t^2 + 2x_tx_{t+s} + x^2_{t+s}$.

For some purposes the instrument may be more suitable than the optical periodograph, since it can be used in several ways. Pointers attached to the ends of the pivoted lever may either move over scales or may follow a graphical record which is driven at a speed proportional to that of the disc D. For discrete observations a ratchet wheel is fixed to D so that it can be turned one tooth for each observation. The ends of the lever could also be driven either directly or through a servo mechanism from a testing instrument. Thus the variations in diameter of a

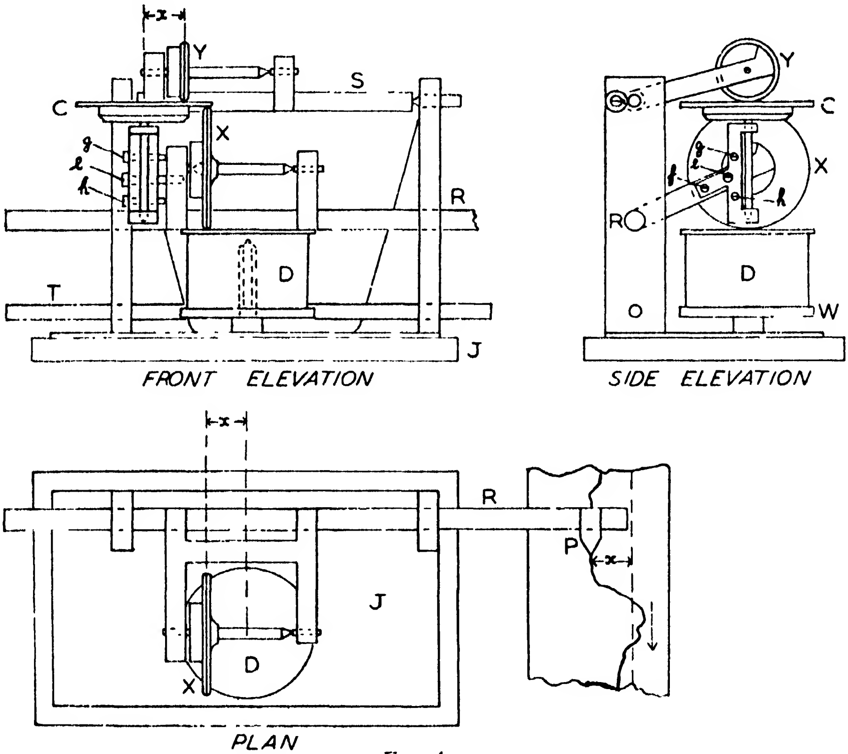


FIG. 4.

The Planimeter Integrator.

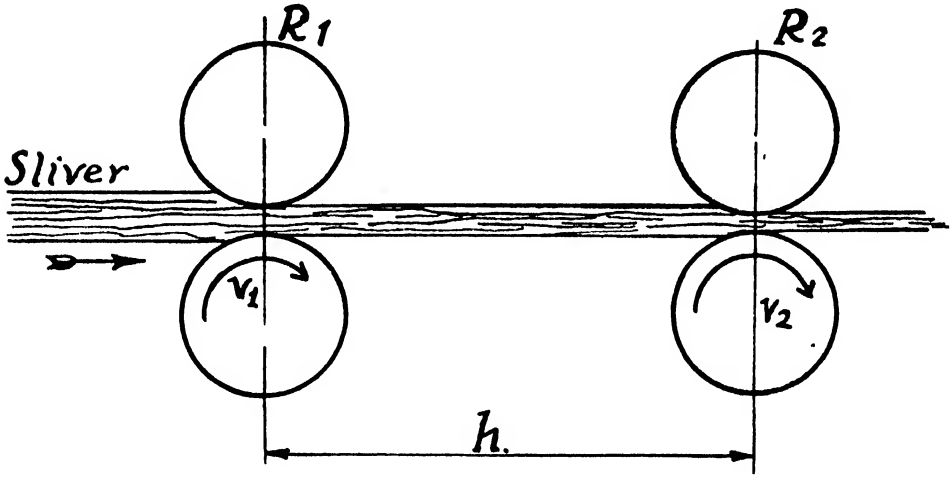
cotton yarn are usually measured by passing it between a steel plate and a light steel shoe which rides on the surface of the yarn, and measuring the vertical movements of the shoe. Two such shoes spaced s apart might through a servo mechanism be used to control the integrator and to so obtain the correlogram directly by repeated passages of the yarn through the instrument. In this particular example it happens to be more convenient to use the optical periodograph, but this is not necessarily true of all measurements. When dealing with discrete observations, each pair can be dealt with easily in one second.

III. THE ANALYSIS OF DRAFTING WAVES BY THE PERIODOGRAM METHOD [4]

1. *The Drafting of Cotton*

In order to make the succeeding discussion intelligible, I shall first of all describe briefly the process through which cotton passes before it is spun into yarn. The earlier processes are

designed to remove dirt and to separate the fibres from one another, and the cotton finally emerges from them in the form of a card sliver, which is an untwisted strand or rope about 1 inch in diameter. It is very soft, and although the fibres composing it are not straight, they are so loosely entangled that the sliver is easily stretched by drawing the fibres apart so that the sliver can be drawn out into a fine strand and finally twisted into yarn. The drawing or drafting is usually done in gradual stages on a number of machines, each of which consists of sets of rollers, which perform the drafting operation. The process is illustrated in Fig. 5. The sliver passes through two sets of rollers, the front pair, R_2, of which revolves at a higher speed than the back pair, R_1, and so draws the fibres over one another to make the sliver longer and finer. The ratio of the speeds of the rollers is called the draft, and is usually anything from 2 up to about 10 with ordinary rollers. The distance apart, h, of the roller nips is called the roller setting, and must obviously be adjusted to suit the length of the fibres. Since the fibres in one variety of cotton vary in length from a small fraction of an inch up to 1 or 2 inches, and since the rollers cannot be set closer than the maximum length of the fibres, there must be a number of fibres which are for a time out of the control of both sets of rollers. The laws of motion of these "floating" fibres form one of the fundamental problems of textile research.



Draft $= D = \frac{V_2}{V_1}$.
Roller Setting — h.

FIG. 5.
Drafting Rollers.

If after leaving the back rollers the short fibres always continue to move with the speed of these rollers until their front ends are gripped by the front rollers, the motion is always the same, and variations in fibre distribution in the entering sliver will be merely repeated in the drafted sliver, but will be spread out over greater lengths in proportion to the draft. But some of the floating fibres may be dragged forward by the fibres already gripped by the front rollers, and so form a thick place in the drafted sliver and a thin place between the rollers. When this thin place begins to reach the front rollers the number of floating fibres being dragged forward will decrease because there are now fewer fibres to drag them forward. Consequently a thick place will be formed between the rollers, which will in its turn reach the front rollers, and so the whole process will repeat itself. We may therefore expect the drafting to cause an oscillatory* variation in the thickness of the drafted sliver. This oscillation is called the drafting wave, and it is the application to it of periodogram and correlogram analysis, which I wish to discuss.

* For convenience, the word "oscillatory" will be used in this paper to include strictly periodic variations and also "quasi" or "almost" periodic variations, which vary in amplitude and phase. The term "strictly periodic" or where appropriate "simple harmonic" will be used for variations which repeat exactly.

2. *Periodogram Analysis of the Drafting Wave*

Fig. 6 shows photographic records of the variations in thickness of drafted slivers. The zero of these records is about half the total height of each illustration below the present base line. Both slivers were prepared by applying a draft of 3 to a card sliver, but in Fig. 6*b* the rollers were set wider than the length of the longest fibres so as to increase the number of floating fibres. Oscillations can be clearly seen in both records; especially in Fig. 6*b*, part of which bears a striking resemblance to the sun-spot series. These would, I think, now be recognized at once as disturbed oscillations, and the correlogram would be applied to their analysis. The work on the drafting wave, although only recently published openly, was, however, commenced before the appearance of Yule's paper on the sun-spot series,[14] and the periodogram method was accordingly employed. Preliminary results were very complicated, numbers of periods were discovered in every drafted sliver examined, much more than one for each drafting process. This threw considerable doubt on the simple explanation that the periods were due to the motion of the floating fibres; it could not in any case be concluded that, because the floating fibres might be expected to draft in an oscillatory manner, an observed oscillation must necessarily be caused by their motion. To prove this required reasonably accurate means of measuring the wave-length and amplitude of the drafting wave, so that the results of experiments made with different shafts, roller settings and with different cottons could be compared. The Grating Periodograph was therefore constructed, and a thorough examination of a few slivers which had been drafted once only was undertaken to see if the periodogram could provide such a means of measurement.

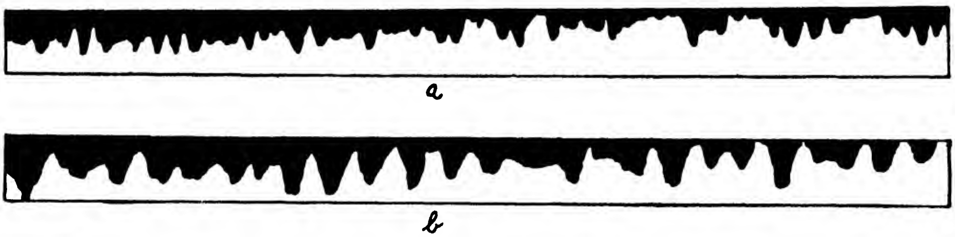


FIG. 6.

Records of Cotton Slivers. (*a*) Test D 1, D = 3, h = 3·81 cm. 280 cm. of sliver.
(*b*) Test D 2, D = 3, h = 5·08 cm. 170 cm. of sliver.

The whole curve Fig. 6(*a*) Test D.1 representing 280 cm. of sliver was first of all examined. Twenty-nine periods were observed, ranging in length from 3·6 to 38 cm. About half these were of low amplitude, and might perhaps have been insignificant. It seemed obvious, however, that Schuster's test of significance could not be applied in this case, and as an alternative test the curve was divided into four equal sections each representing 70 cm. of sliver, with the object of finding out which periods persisted throughout the whole curve. The wave-numbers of the periods observed are given in Table I, together with their amplitudes classified as high (*h*), moderate, and low (*l*), as judged by the appearance of the fringes in the periodograph. It should be noticed first of all that the resolving power of the periodograph would be insufficient to separate in a length of 70 cm. all the periods that had been observed in the full length of curve, so that if the periods persisted through the whole curve many of them should have appeared as broad bands in the analysis of the reduced lengths. No such effect was observed; nearly all the bands were of normal width for a single period. It will be seen from Table I that out of the 23 observed periods only 4 occur in all four sections, and that, if this were taken as a test of significance, 3 periods of rather low amplitude (Nos. 24, 26 and 28) would be accepted, while some of high amplitude (Nos. 15 to 18) would be rejected. But the curve was also examined in three different consecutive sections of 70 cm., the first beginning at the middle of section 1 of the table and extending to the middle of section 2 and so on. It was then seen that some of the periods that are present in all four sections of Table I were absent from these intermediate sections.

When the wave-numbers of the periods as abscissæ are plotted against a series of whole numbers as ordinates, the points, with two exceptions, lie fairly close to a straight line of slope

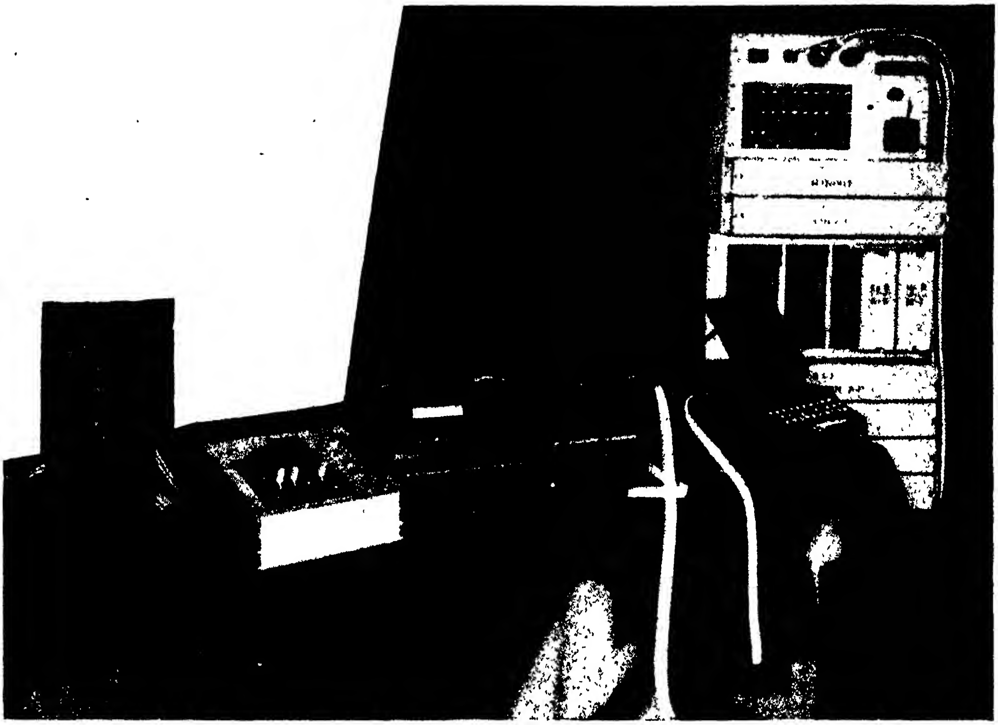


FIG. 11.

FIG. 12.

[*To face p.* 48.

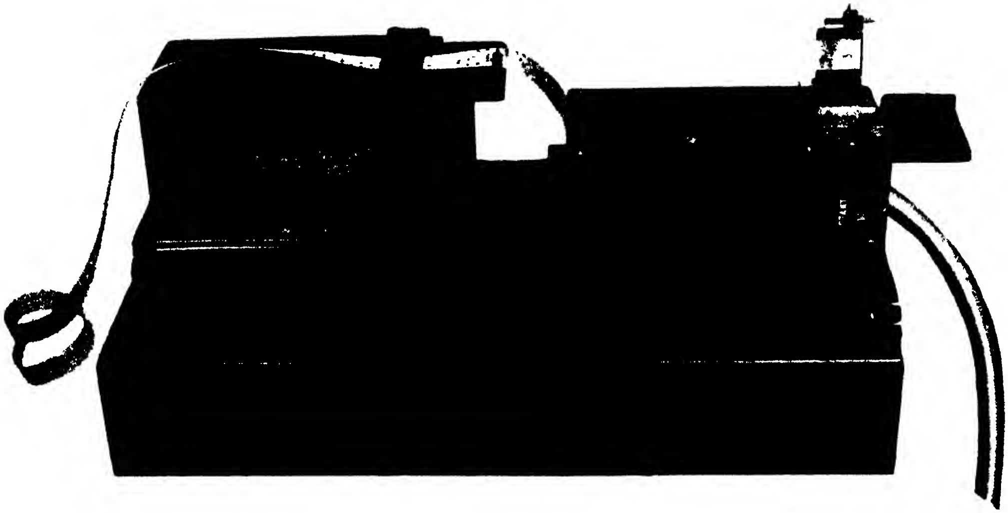


Fig. 13.

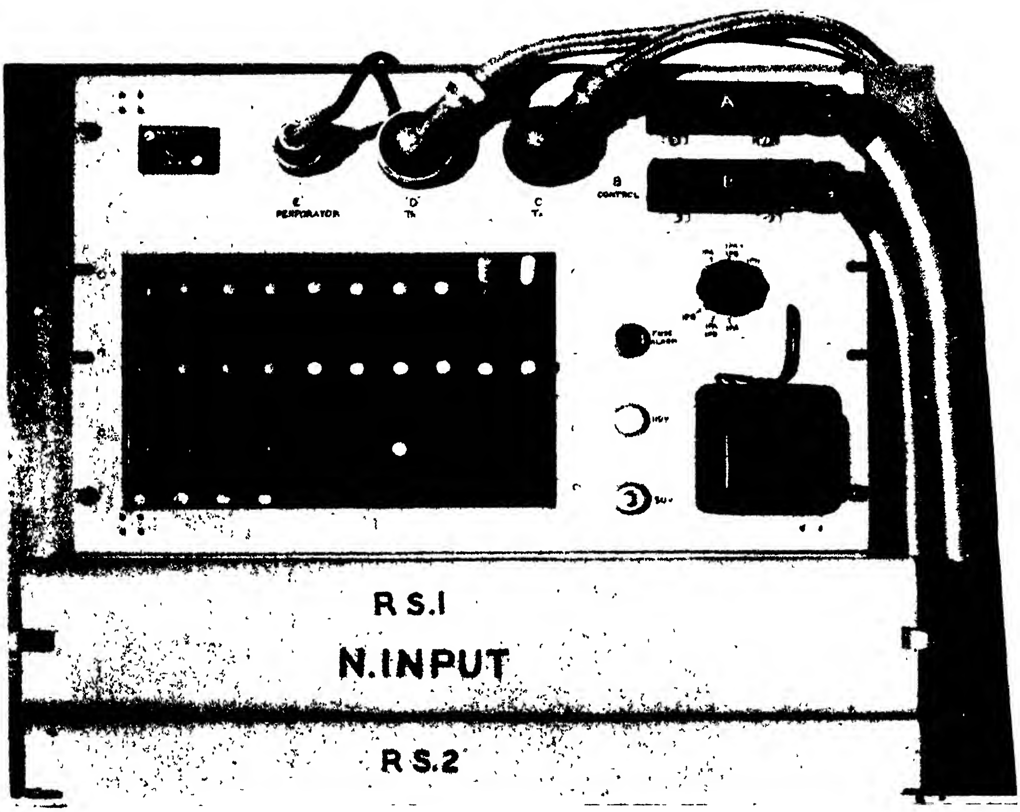


Fig. 15.

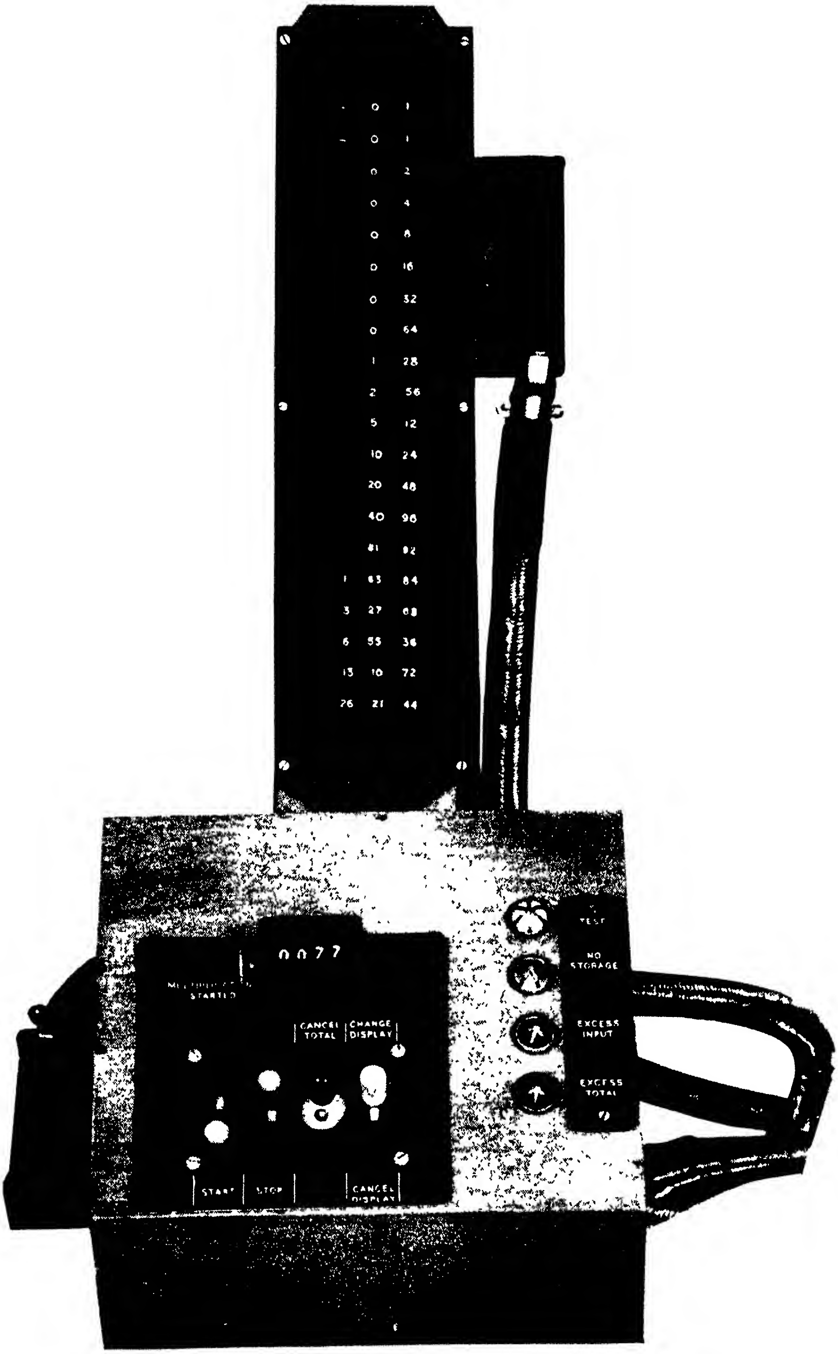

0077
STARTED
CANCEL TOTAL
CHANGE DISPLAY
START
STOP
CANCEL DISPLAY
TEST
NO STORAGE
EXCESS INPUT
EXCESS TOTAL

FIG. 14.

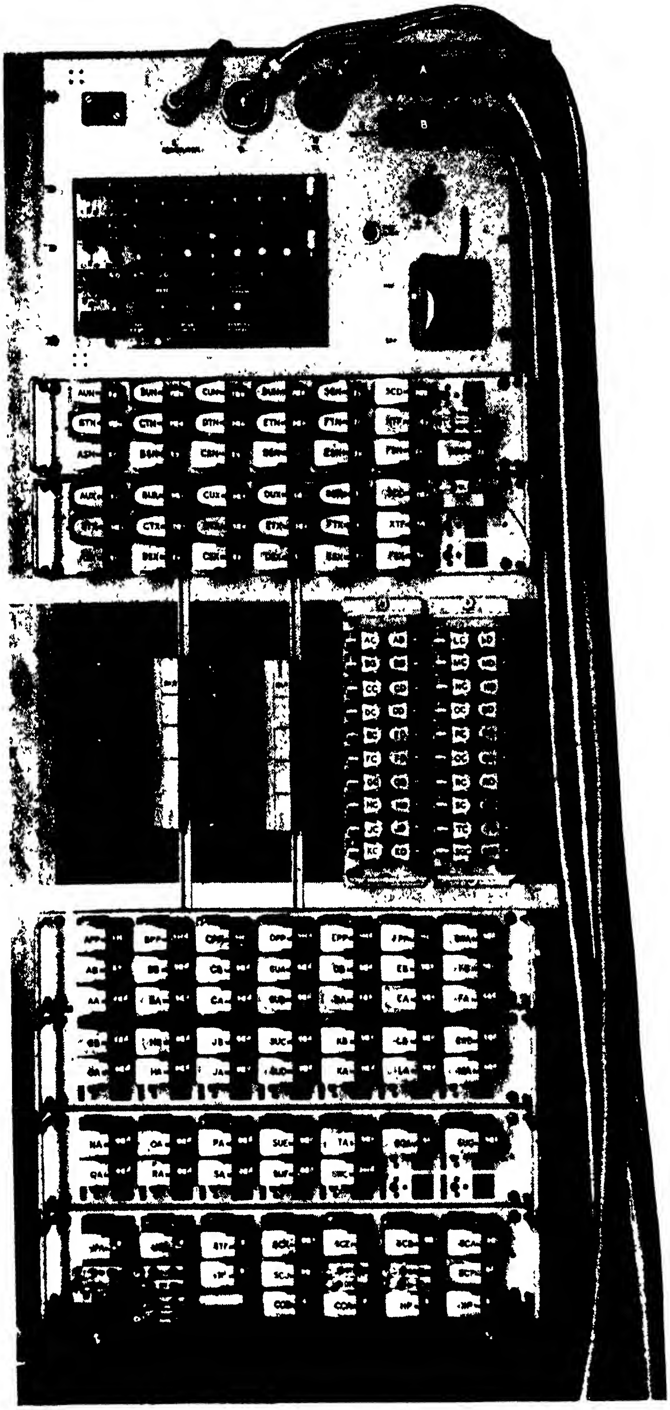


FIG. 16.

84·9 cm., which in this test passes through the origin, showing that the periods are harmonies of a fundamental period of 84·9 cm. Table I shows that the agreement of the calculated and measured wave-numbers is fairly good. The differences are, however, greater than the error of the periodograph, which for this length of curve is not greater than 0·0014 cm.$^{-1}$. But this error is only that in setting the instrument, and takes no account of the random variations in the curve, which no doubt have some effect on the measured wave numbers.

It has already been mentioned that when the whole length of this test was examined, 29 periods were observed. As shown in Fig. 7, they lie on a line of slope 120 cm., again with a few

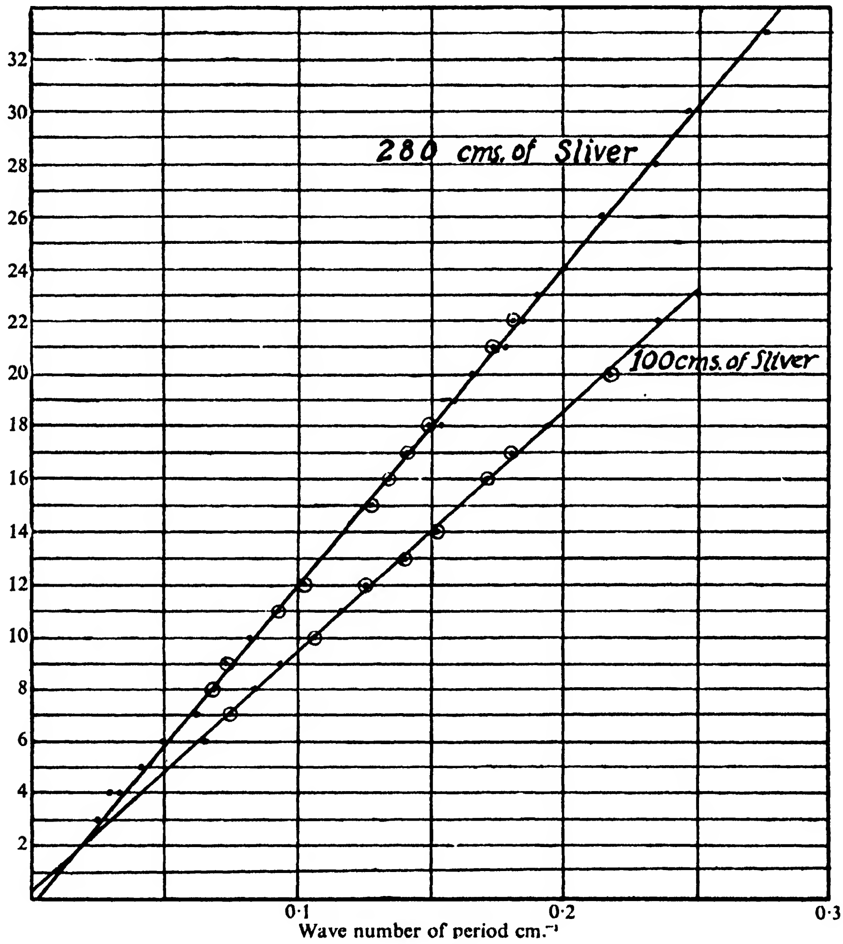


FIG. 7.
Periodogram Analysis of Drafting Wave. Test D 1.

exceptions of low amplitude, while when 100 cm. of curve were tested the slope was 91 cm. Similar results were obtained on several other slivers prepared with different drafts and roller settings. They are summarized in Table II, which includes altogether 23 periodogram analyses. With increasing length of sliver more periods were generally observed and the slope of the line was increased. The fourth column of the table gives the mean deviation of individual measured periods from the line, and so indicates the closeness of fit of the periods to the linear relation. Since no point can be more than 0·5 unit from the line, one would expect for a random distribution a mean deviation of 0·25 unit. The actual mean deviations are usually much less than this.

TABLE I

*Test D.*1. *Wave Numbers in* cm.$^{-1}$

Number of section								Harmonics of 84·9 cm.	
1		2		3		4		Wave number	Number
—	—	0·361	*l*	0·364	—	—	—	0·365	31
0·352	*l*	0·348	*l*	—	—	—	—	0·354	30
0·328	—	0·327	—	0·332	*l*	0·330	—	0·330	28
0·309	*l*	0·307	—	0·304	—	0·307	*l*	0·307	26
0·295	*l*	—	—	—	—	—	—	0 295	25
0·279	—	0·277	—	0·281	*l*	0·280	*l*	0·283	24
0·265	—	—	—	0·263	—	0·265	*l*	0·260	22
0·250	—	—	—	—	—	—	—	0·248	21
—	—	0·240	*l*	0·243	—	—	—	—	—
0·234	—	—	—	—	—	0·236	—	0·236	20
—	—	—	—	—	—	0·220	—	0·224	19
0·217	—	0·214	*h*	—	—	—	—	0·213	18
0·200	*h*	—	—	—	—	—	—	0·201	17
—	—	0·192	*h*	0·189	—	—	—	0·189	16
0·180	*h*	—	—	—	—	0·177	*h*	0·177	15
—	—	0·172	*h*	—	—	—	—	—	—
—	—	—	—	0·164	—	—	—	0·164	14
0·144	*h*	0·141	*h*	0·146	*h*	0·144	*h*	0·142	12
—	—	0·120	—	—	—	0·118	—	0·118	10
—	—	0·093	—	0·088	—	0·093	*l*	0·094	8
0·079	—	—	—	—	—	0·078	*l*	0·082	7
—	—	0·071	—	—	—	0·069	—	0·071	6
0·049	—	0·053	—	—	—	0·050	..	0·048	4

This linear relation is the only definite fact which emerges from all these periodogram analyses; the particular periods present vary in an apparently haphazard manner from one portion of the sliver to another, and the slope of the line cannot be regarded as characteristic of the drafting wave. There is thus no basis of comparison of one test with another, and the method is therefore practically useless for the purpose of measuring the effects of various drafting conditions on the amplitude and wave length of the drafting wave.

TABLE II

Test no.	Length of sliver examined, cm.	Slope, cm.	Mean deviation	Wave-length, cm.	Wave-number, cm.$^{-1}$
D1	280	120·1	0·16	5·92	0·169
—	100	91·0	0·09	--	—
—	70 (4 Sections)	84·9	—	—	—
D2	166	99·5	0·17	6·62	0·151
—	100	85·5	0·15	--	—
—	83 (2 Sections)	45·0	0·08	—	—
--	41·5 (4 Sections)	45·6	0·22	—	—
D3	81 (2 Sections)	51·2	0·12	7·58	0·132
D4	81 (2 Sections)	45·1	0·15	5·66	0·176
F1	61 (4 Sections)	58·9	0·22	—	—
—	149	105·7	0·17	7·6	0·131

3. *Periodogram of a Disturbed Series*

In Fig. 6 the drafting wave appears to be a simple period 5–7 cm. long, which varies considerably in amplitude. The next step was therefore to consider what kind of results a wave of this type might be expected to give on analysis by the periodogram method.

If we assume in the first place that the variations in amplitude are themselves periodic, they can be represented by a Fourier series:—

$$a_0 + a_1 \cos(2\pi qx + \varepsilon_1) + a_2 \cos(4\pi qx + \varepsilon_2) + \ldots$$

The only condition that the a's must satisfy is that the high order ones should be small, for these are the amplitudes of periodic variations in the amplitude shorter than the drafting wave itself, and if such amplitudes were not small, we should not describe the wave as one which simply varied in amplitude.

If the wave-number of the drafting wave is k, its equation is then

$$\begin{aligned} y &= \{a_0 + a_1 \cos(2\pi qx + \varepsilon_1) + \ldots\} \cos 2\pi kx \\ &= a_0 \cos 2\pi kx + \tfrac{1}{2}[a_1 \cos\{2\pi(k+q)x + \varepsilon_1\} + a_2 \cos\{2\pi(k+2q)x + \varepsilon_2\} + \ldots] \\ &\qquad + \tfrac{1}{2}[a_1 \cos\{2\pi(k-q)x - \varepsilon_1\} + a_2 \cos\{2\pi(k-2q)x - \varepsilon_2\} + \ldots] \quad . \quad (2) \end{aligned}$$

The simple harmonic components have wave-numbers:—

$$\ldots (k-3q), \quad (k-2q), \quad (k-q), \quad k, \quad (k+q), \quad (k+2q), \quad (k+3q) \ldots$$

and amplitudes:—

$$\ldots \tfrac{1}{2}a_3, \quad \tfrac{1}{2}a_2, \quad \tfrac{1}{2}a_1, \quad a_0, \quad \tfrac{1}{2}a_1, \quad \tfrac{1}{2}a_2, \quad \tfrac{1}{2}a_3 \ldots$$

When plotted against a series of whole numbers, the wave-numbers therefore lie on a straight line, whose slope is the wave-number of the amplitude period. The amplitudes are symmetrical about the middle of the range of periods, and tend to be greatest in the middle and least at the ends.

For a finite length of curve the amplitude variations of the disturbed period resemble a periodic variation of fundamental length comparable with the length of the curve, and the disturbed period would therefore give the kind of periodograms which have been described in the last section. The appearance and disappearance of many of the components in different portions of the curve are simply accounted for by the changes in form of the amplitude variations. It will be noticed, however, that the differences between the observed wave-numbers and that of the disturbed period represent the periodogram of the amplitude variation in the length of curve examined, and, if the amplitude variations are purely random, it seems therefore rather remarkable that the observed periods do so often recur in different sections of the curve, and that their wave-numbers do not depart more from the linear relationship. A more rigorous mathematical treatment seems to be needed.

It is still, however, necessary to compare the amplitudes of the simple periods with equation (2). These were calculated for all the periods observed in the periodograph for three of the above tests. The complete periodograms are given in Fig. 8, in which the 100-cm. length for Test D1 is the same as that for Fig. 7. The periodograms show a general tendency for the amplitudes to be greatest in the middle of the range. But the amplitudes are not symmetrical. This means that on reversing the calculation of equation (2) the simple components to be combined are unequal in amplitude, and the resultant then fluctuates in phase as well as in amplitude. In all other tests examined it could be clearly seen on the screen of the periodograph that the grouping of the periods resembled Fig. 8. The periodogram was thus useful in providing strong evidence that the drafting wave was a simple disturbed oscillation fluctuating in amplitude and phase, but it was useless for obtaining measurements of the wave-length and amplitude of the wave.

It should be added, however, that in some cotton yarns and slivers mechanical defects in the machinery introduce variations which are strictly periodic. Provided that their amplitudes are not too small, these periods produce in the periodograph bands of fringes, which stand out among the weaker fringes due to the drafting wave, so that the wave-length is easily measured and the cause of the period identified.

The inevitable splitting up of a disturbed period into an often large number of simple components is a property of periodogram analysis, which, though it is the consequence of well-known principles, has not, as far as I am aware, received sufficient emphasis in connection with the analysis of economic and meteorological time series. The simple components have constant phase and amplitude throughout the whole portion of the series analysed. If the mechanism generating the series is such as to produce harmonic oscillations of constant phase and amplitude,

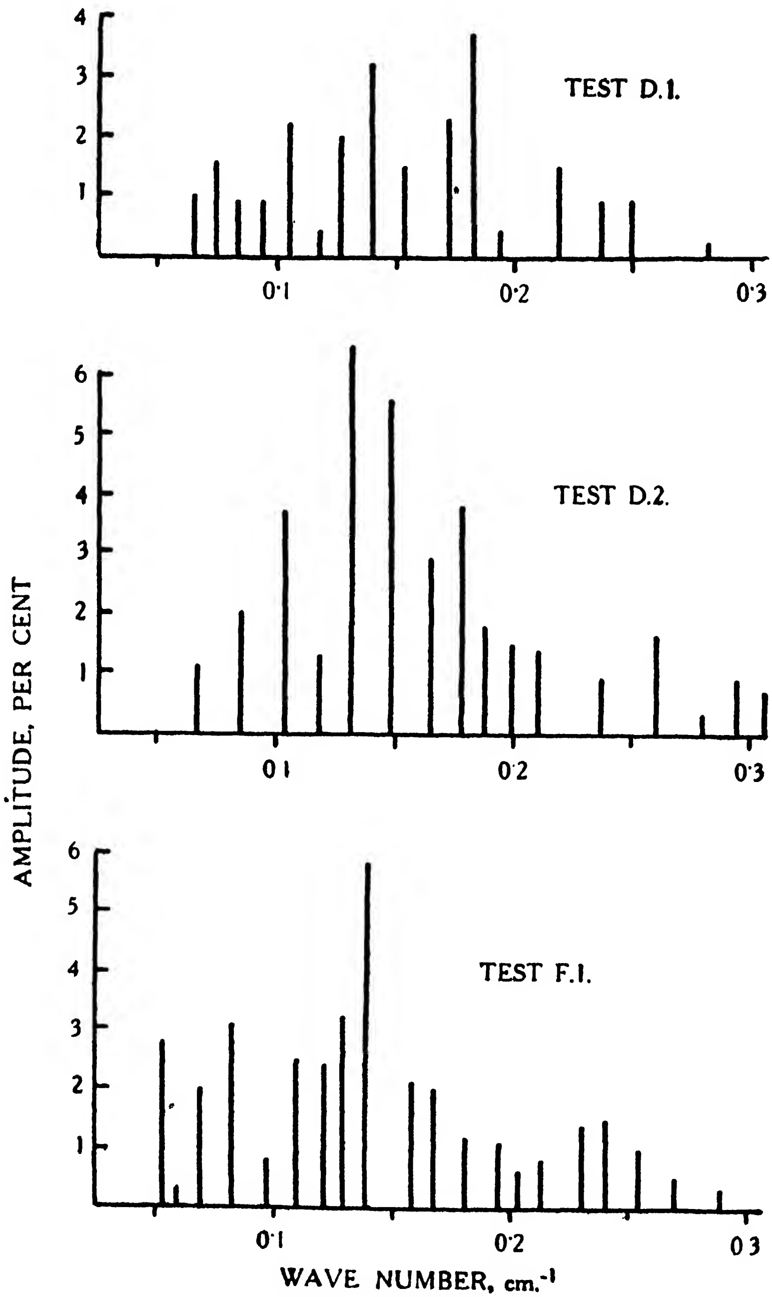


FIG. 8.

Periodograms of the Drafting Wave in Cotton Slivers. Tests D1 and D2, 100 cm. of sliver. Test F1, 149 cm. of sliver.

then the peaks of the periodogram will indicate the periods of the mechanism; but if for any reason the phase or amplitude of the oscillation varies or changes, then the periodogram will split it up into two or more simple components, whose periods may have little direct relation to the natural periods of the mechanism. When the changes of amplitude and phase are frequent and accidental, the periodogram will indicate large numbers of periods. As the length of the portion of the series analysed increases, the total variation in amplitude becomes more complex, the number of simple components increases and their amplitudes decrease. From the point of view that they are the results of accidental causes and that their amplitudes tend to decrease with increasing size of sample, these components might perhaps be regarded as being statistically insignificant. On the other hand, they are certainly significant in the sense that they are the result of a real tendency of the system to produce periodic oscillations. With a series of this type we may, as with the drafting wave, only arrive at a useful physical interpretation by putting the components together again.

In a paper read to this Society about a year ago, Kendall [8] made a comparison of the periodogram and correlogram methods as applied to an economic series and to some artificial autoregressive series, and concluded that where there is any possibility that a series is of the autoregressive type, the periodogram may not only be worthless, but extremely dangerous in suggesting periods of no reality. The conclusion drawn from the measurements on the drafting wave is similar, but not quite so drastic. The literal interpretation of the observed periods would certainly be extremely misleading, but the periodogram was useful in showing that we were almost certainly dealing with a single disturbed period.

4. *Secondary Analysis of the Drafting Wave*

An attempt was next made to measure the drafting wave by a limiting form of Schuster's method of Secondary Analysis. The whole of the grating on the Grating Periodograph was screened, except the central line. The grating was set to correspond with the approximate wave-length obtained by counting peaks on the curve, and the curve was then moved along to bring each bright fringe on the vertical ground-glass screen in turn on to a vertical line. A scale on the curve holder gave the phases of the maxima on the curve. The process is equivalent to fitting a sine curve one period at a time to the sliver curve. The maxima were numbered in order, and their distances from the beginning of the curve plotted against their numbers. The result of a typical series is shown in the upper part of Fig. 9, in which the circles represent the phases of the maxima as measured. They fall on a series of parallel straight lines. Since it is known that the amplitude is very variable, it is reasonable to assume that it is sometimes so small that some of the maxima are unobservable. On this assumption the mean slope of the lines measures the wave-length. The upper line in Fig. 9 was drawn with this mean slope, and the points brought close to it by renumbering the maxima to allow for possible unobserved ones. (The beginning of the upper line is plotted one unit higher to bring it clear of the short lines.) When the wave-length obtained from the lines differed appreciably from that to which the grating had originally been set, the grating was re-set to the new wave-length and the work repeated.

Wave-lengths and wave-numbers obtained by this method are given in the last two columns of Table II. The wave-numbers may be compared with the periodograms in Fig. 8.

From these measurements the amplitude can be approximately estimated by marking off the positions of the maxima on the curve, taking the minima to be half-way between them, and subtracting the minimum from the maximum ordinates.

This rather crude method of measurement was sufficiently accurate and reliable to allow slivers drafted under different conditions to be compared and a series of experiments to be carried out which demonstrated that the drafting wave is in fact caused by the oscillatory motion of the shorter fibres in the cotton. Nevertheless the method is not very satisfactory. While it cannot be denied that when the amplitude of the oscillation is very variable it may sometimes be so low that a peak cannot be observed, a lot of personal judgment is occasionally required, and it is by no means certain that the gaps between the lines in Fig. 9, for example, are not due to real changes of phase rather than to very low amplitudes. It is also only when the average amplitude of the wave is fairly high that the method can be used, and this puts rather severe limitations on the work. It might possibly be a useful method for oscillations less disturbed than the drafting wave.

IV. CORRELOGRAM ANALYSIS OF THE DRAFTING WAVE

Before discussing the application of the correlogram to the drafting wave, it is worth while to describe more fully the nature of the series with which we have to deal. If we ignore the variations in mass per unit length along each fibre and from fibre to fibre—variations which for the slivers used in this work contribute only a very small fraction to the total variance—the thickness or mass per unit length of a sliver at a given point is proportional to the number of fibres crossing a section of the sliver at that point. It follows at once that measurements of thickness at sections separated by distances less than the maximum length of fibre are correlated, because some fibres

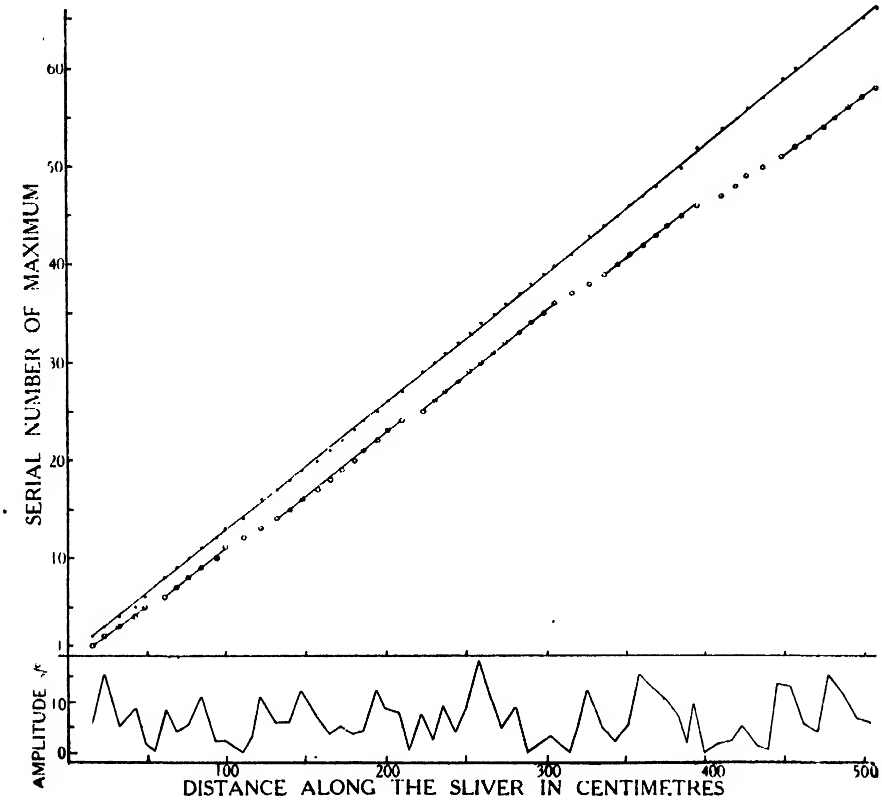


FIG. 9.

Secondary Analysis of Drafting Wave. Test F1.

are common to both sections. Spencer-Smith and Todd [12] have calculated this correlation for flax slivers; for cotton slivers their formula reduces to:—

$$r_s = \frac{\int_s^{l_m} (l - s) f(l) dl}{\int_0^{l_m} l f(l) dl} = \frac{\text{Number of fibres crossing both sections}}{\text{Number of fibres crossing one section}}$$

where $f(l)dl$ is the frequency of fibres having lengths between l and $l + dl$, and l_m is the maximum fibre length. There is a little uncertainty in applying this to actual slivers, as the fibres in the slivers are not as straight and parallel as they are when their lengths are being measured during the determination of $f(l)$.

Spencer-Smith and Todd [12] and Martindale [10] have shown that on perfect machinery with

perfect control over the fibres during drafting, the most uniform sliver that we can hope to make is one in which the fibres are distributed at random along the length of the sliver, in which the number of fibres crossing a section of the sliver, or the fibre number as it is usually called, follows a Poisson distribution with variance equal to the mean fibre number. In addition to this cause of variance, we have, in cotton slivers, the more important causes of variance, the drafting wave and variations due to mechanical defects in the machinery. The following figures give an idea of the relative importance of these variances in the rovings used for the correlogram analysis. The total coefficient of variation varied from 10 to 20 per cent., according to the drafts and roller settings employed, the coefficient of variation for the random arrangement of the fibres

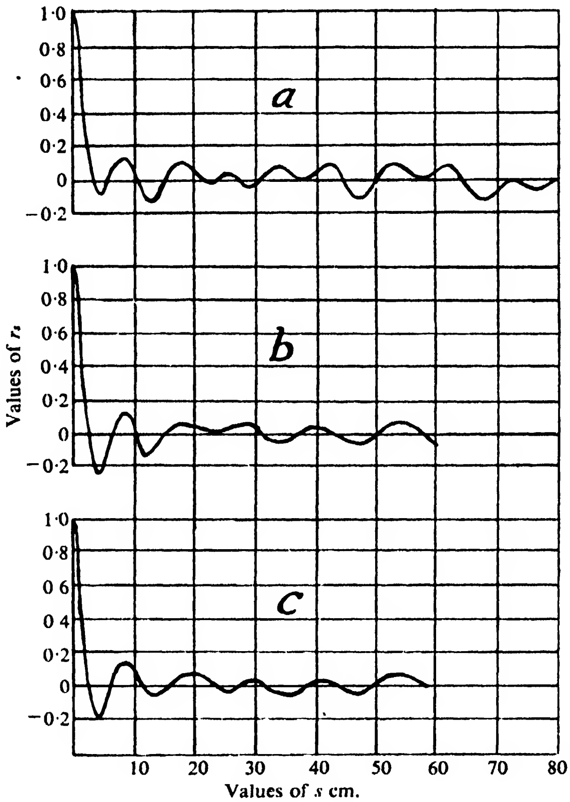


FIG. 10.

Correlograms of Drafting Wave. $D = 8{\cdot}26$, $h = 4{\cdot}05$ cm. Twist in entering roving 0·48 turns per in. Sample lengths (*a*) 340 cm. (*b*) 1700 cm. (*c*) 3400 cm.

from about 2 to 4 per cent., and that due to the variation in mass of the fibres was about 1 per cent. Mechanical defects were eliminated as far as possible by tuning up the machine on which the rovings were prepared.

The application of the correlogram to textile slivers has previously been discussed by Spencer-Smith and Todd in the paper already referred to, in which they treat it as a measure of the departure of actual flax slivers from the ideal random sliver. The deviations of their measured serial correlations from those expected for a random sliver were all significant, and suggested that some kind of periodicity is imposed on the random structure of the sliver.

Here the point of view is somewhat different. Further progress in the researches on the motion of the floating fibres demanded methods of measuring the wave-length and amplitude of the drafting wave, so that these and the laws governing their changes with draft, roller setting

and variety of cotton could be discovered and compared with theoretical studies of the fibre motion.

The correlogram analysis was therefore undertaken primarily as a better method of measuring the wave-length than that of secondary periodogram analysis, but we also hoped that something might be revealed of the type of motion of the fibres and of the nature and magnitude of the disturbances.[5]

From a statistical point of view it would be interesting to have correlograms of the slivers on which the periodogram analyses were performed; but the work had to be co-ordinated with other researches, and was accordingly carried out on rovings. These are similar to sliver, except that they are finer, and that the fibres composing them have been more or less straightened and parallelized by previous drafting. Normally rovings have also just sufficient twist inserted to allow them to be handled without damage.

A typical set of correlograms is shown in Fig. 10. In the description of the Correlation Periodogram, upon which the analyses were made, it was pointed out that in its present form the instrument gives only relative values of the correlation coefficients. In order to plot all the correlograms on the same scale, it has been assumed that the mean correlation for large values of s is zero. Each curve is the mean of those for positive and negative values of s.

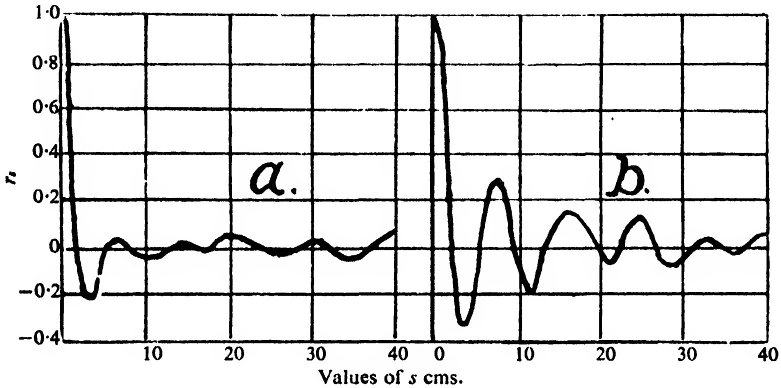


FIG. 11.

Correlograms of the Drafting Wave. $D = 8{\cdot}26$, h - $2{\cdot}03$ cm. Sample length 3400 cm. Twist in entering roving (*a*) 0·48 turns per in. (*b*) 1·4 turns per in.

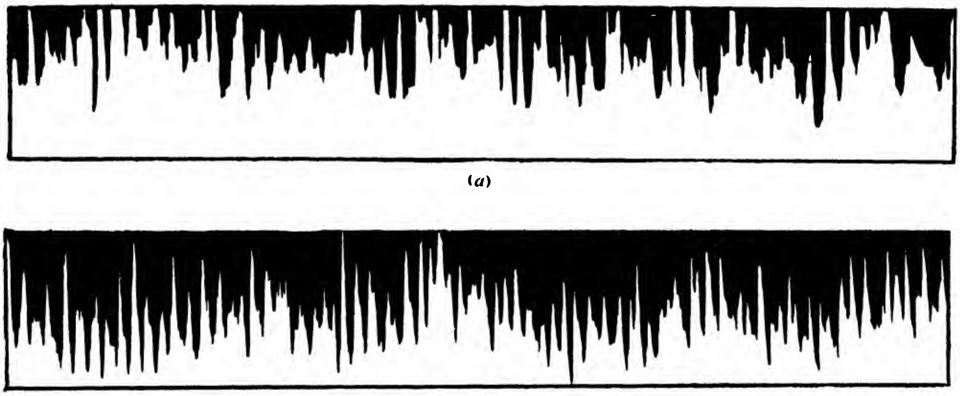
Fig. 10 (*a*) is for a sample of roving 340 cm. long (the length for the first curve in the periodograph; the total length used was this plus twice the maximum value of s). The correlogram is rapidly damped during the first period, but thereafter fluctuates between about $\pm 0{\cdot}1$. The curves for the longer samples Fig. 10(*b*) and (*c*) show, however, that this fluctuation is due merely to the finite size of the sample. The small peaks on the tail of the periodogram are therefore not statistically significant, because these decrease as the size of the sample increases. The correlation at the first minimum, the first maximum, and perhaps also the second minimum, either increases or remains almost constant.

Fig. 11(*a*) is the correlogram for a similar roving drafted with a closer roller setting; the damping is greater. This may either be due to greater damping of the drafting wave itself or to its lower amplitude relative to random variations in the roving. In Fig. 11(*b*) the roving entering the rollers had about three times the normal twist. The damping is now much less, and the oscillations persist over at least three periods before they become insignificant. The effect of twist on the wave is well shown in Fig. 12, which shows the variations in thickness of part of the rovings used for the correlograms of Figs. 10 and 11(*b*).

These and many similar correlograms showed that with normally twisted rovings the first minimum was always significant, and usually also the first maximum. It was therefore concluded that there was a definite tendency for the motion of the floating fibres to be periodic, but that either the damping is so heavy or the disturbances are so great that this tendency persists only for

a half to one and a half periods. The smaller damping of the correlograms for more highly twisted rovings suggested that the disturbances are due to inequalities in cohesion or openness of the cotton, which are less important when the fibres are more closely bound together by the twist. The twist also, by increasing the frictional forces between the fibres, probably increases the tendency towards a steady oscillation.

When, however, the correlograms are used to measure the wave-length of the drafting wave, various difficulties are encountered. At first we measured the mean distances apart of the peaks on the tails of correlograms such as Fig. 10(*a*), regarding the tail as a smoothed and averaged version of the original record. This, however, seemed rather unsound, as these peaks are statistically insignificant—a fact which was rather forcibly brought home to us when we realized that we were deliberately using rather small samples in order to preserve good peaks in the tail. The width of the central peak was also measured at definite values of the correlation coefficient, and attempts were made to fit equations to it. Such methods are, however, bound to be inaccurate because the shape of the central peak is affected by variations in the curve other than the drafting wave, and also by the correlations at short distances due to the fibres which stretch across both sections of the roving. This correlation is zero for values of s greater than the maximum



(*a*)

(*b*)

Fig. 12.

Records of Cotton Rovings. 500 cm. of roving. (*a*) Corresponding to Fig. 10, (*b*) corresponding to Fig. 11b.

fibre length, which was 2·7 cm. for the cotton used for the correlograms illustrated. Finally the distance of the first maximum from the ordinate $s = 0$ was taken as a measure of the wave-length; it was nearly always well defined, though sometimes, as, for example, Fig. 11(*a*), it was no higher than the peaks on the tail, and on some few rovings it was so ill defined as to render accurate location impossible. It is doubtful whether in such cases the wave-length has any definite meaning. Twice the value of s at the first minimum, which was always clearly defined, was also tried, but as it gave values on the average about 10 per cent. higher than the first maximum, we concluded that it was affected by some of the same factors which affect the shape of the central peak.

One feature of the measurements is the large variation of the wave-length from sample to sample of the same roving. On a number of different rovings the range of nine measurements of wave-length made on 340-cm. samples, each including about 50 periods, was anything from 15 to 30 per cent. As these variations in the position of the first maximum were accompanied by corresponding changes in the width of the initial peak and in the spacing of the peaks on the tails, we thought they might be due to real changes in the period with which the drafting tends to oscillate, caused possibly by imperfect admixture of the cotton or variations in twist; but we were able to rule out all such causes, and concluded finally that there was no evidence for changes in the fundamental period of oscillation, and that the variations in measured wave-length were

due partly to the error in measuring the position of the first maximum on the correlograms and partly to the variations in phase caused by the disturbances.

Recently Kendall [7] has shown that for the auto-correlated series generated by:—

$$u_{t+2} + au_{t+1} + bu_t = \varepsilon_{t+2} \quad \ldots \ldots \ldots \ldots (3)$$

of which the complementary function is

$$u_t = p^t \, (A \cos \theta t + B \sin \theta t) \quad \ldots \ldots \ldots \ldots (4)$$

the equation of the correlogram for an infinite series is

$$\rho_s = p^s \frac{\sin (\theta s + \psi)}{\sin \psi} \quad \ldots \ldots \ldots \ldots (5)$$

in which

$$\tan \theta = \sqrt{\frac{4b}{a^2} - 1}, \quad p = \sqrt{b} \quad \text{and} \quad \tan \psi = \frac{1 + b}{1 - b} \tan \theta.$$

There is, therefore, a phase difference between the first maximum and the peak at $s = 0$, and consequently the position of the first maximum cannot be used to obtain the length of the period unless the damping is known. Kendall therefore suggested that the period should be estimated from the peaks or upcrosses beyond the first maximum. This is not, however, very satisfactory when these peaks are not statistically significant, and since, for the reasons already explained, it does not seem possible to estimate the damping from the shape of the initial significant part of the correlogram, we are left with no sound method of measuring the length of the drafting wave.

In order to get an idea of the magnitude of this phase difference, I have calculated approximately the position of the first peak T_2 on the correlogram for three artificially generated series (Kendall [8]) according to equation (5) and compared it with the distance apart of the peaks, T_1, on the undisturbed motion (4) and with the fundamental period, T_0. The results are given in Table III.

TABLE III

Series no.	a	b	T_0	T_1	T_2	T_m	T_p
1	−1·1	0·5	9·25	8·56	8·17	8·3	8·2
2	−1·2	0·4	19·53	16·55	16·11	12·7	12·7
3	−1·1	0·8	6·92	6·79	6·64	7·0	7·9

T_m is the observed position of the first maximum on the correlogram, and T_p the period measured from the peaks other than the main one at $s = 0$. The appearance of the correlograms suggests that the damping of these series covers the range of damping for the drafting wave. It will be seen that the effect of the phase difference is not very great; in fact it is no greater than the errors of measurement for the drafting wave; the chief trouble is, of course, that it is a systematic error. We should not therefore be very far wrong in taking our measurements on the drafting wave as approximations to the wave-length of the undisturbed damped motion. The most striking features of the table are, however, the big difference between the observed and calculated periods for series 2, and that generally the differences between observed and calculated values are greater than the effect of the phase difference.

This argument, however, applies to auto-correlated series according to equation (3), which corresponds to a damped simple harmonic motion. There is, however, no evidence that the drafting wave is analogous to such a motion, and it seems, therefore, rather unsound to apply conclusions derived from the auto-correlated series to the drafting wave.

Further progress might perhaps now be made by an attempt to analyse the main peaks of the drafting wave correlograms into their components, but for the present it seems that the best we can do is to adopt the common-sense view that the position of the first maximum indicates the periods over which the time series tends to repeat itself, and that this period is connected in some way with the underlying mechanism which produced the series. The work on the drafting wave was in fact continued on this assumption; the wave-length was related empirically to the draft, the roller setting and the twist in the original roving. These relations when combined with theoretical calculations of the motion of the fibres enabled the amplitude of the wave to be calculated. The calculated amplitudes agreed reasonably well with the measured variances of drafted rovings.

In order to provide an approximate comparison of the periodogram with the correlogram, the records of Fig. 12(*a*) and (*b*) have been analysed in the Grating Periodograph. The periodograms are given in Fig. 13. The amplitudes of the periods were not calculated, but were estimated from the appearance of the fringes in the periodograph; they are therefore only rough estimates, but the periodograms are sufficient to show the grouping of the periodogram components around the wave number obtained from the first maximum of the correlogram. Fig. 13(*b*), which corresponds to the less heavily damped correlogram, contains a larger proportion of periods of high amplitude than Fig. 13(*a*).

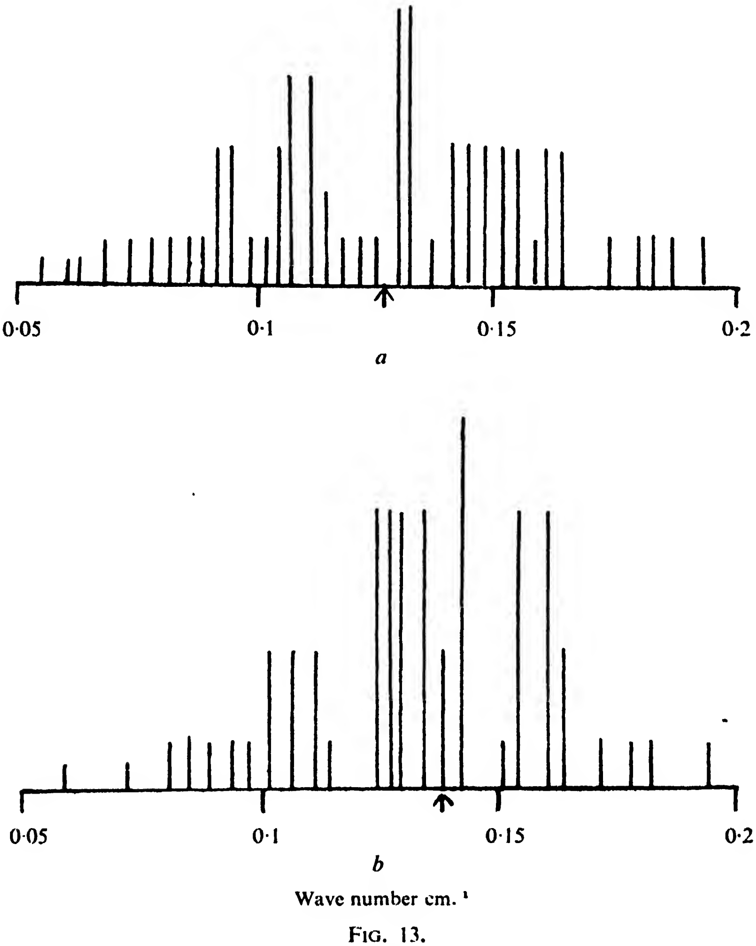


FIG. 13.

Periodograms of the Drafting Wave in Cotton Rovings. (*a*) Periodogram of Fig. 12*a*. (*b*) Periodogram of Fig. 12 (*b*). Arrows indicate wave numbers from the correlogram.

V. OTHER APPLICATIONS OF THE CORRELOGRAM

It is worth while to mention briefly two more direct applications of serial correlations to textile research. All cotton yarns are irregular, the irregularity being the resultant of the drafting waves introduced at all the drafting processes, random variations in the distribution of the fibres along the yarn, and variations due to mechanical defects in the machinery. Since a chain breaks at its weakest link, the strength of a given length of yarn is the strength of the weakest place, which is nearly always the thinnest place. The measured strength of a yarn consequently decreases

as the length of specimen broken increases, and its strength properties can be completely defined only by the complete relation between strength and specimen length. This relation has been calculated by Peirce [11] for a chain in which the links are assembled at random. Naturally the results do not agree with those observed for yarns, for in yarns neighbouring links are correlated. Obviously a knowledge of the correlogram for a yarn would allow the complete calculation to be made.

Another interesting question, to which, however, the application of the correlogram would at present be only of academic importance, is the formation of bars and other patterns when a periodic variation in the diameter of the yarn happens to be simply related to the width of the cloth. The liability to form such patterns is obviously related to the heights of the peaks and troughs of the correlogram. An example was the roving of the correlogram of Fig. 11(*b*). This formed a pronounced spiral pattern on the bobbin on which it was wound when the circumference of the bobbin was nearly an exact multiple of the period length, whereas such patterns were not formed by the rovings with more highly damped correlograms.

VI. CONCLUSION

It will be seen from this outline of the applications of periodograms and correlogram to some of the problems of textile research that these methods of analysis may serve two distinct purposes. They may, as in the two problems indicated in the last section, be used purely to describe or characterize the series for some other specific purpose, or they may be employed in an attempt to discover something of the physical causes, which produced the series, and to measure properties of the series such as periodic times, amplitudes, or damping coefficients, which may be related to the corresponding properties of the underlying mechanism. It is in this latter type of application that the greatest difficulties arise, and I should like to suggest that valuable progress might be made by a more physical approach to the problems involved. Up to now it seems that most of the work on time series has been done either on meteorological, economic or other series, for which little or nothing is known of the underlying mechanism, or on artificially generated series for which the process of generation is purely mathematical. While it is true that the simple auto-regressive scheme does correspond to a definite physical system—namely, one that executes damped simple harmonic motion—and that there seems little need for a more complicated scheme at present, there are several kinds of physical systems, in which the causes of oscillation are physically, and sometimes mathematically, very different. For example, there are control systems,[1] such as thermostats, which tend to oscillate about the temperature to which they have been set, and relaxation oscillations,[13] in which the damping is negative for small displacements, and positive for large displacements, so that the system is unstable, but nevertheless oscillates with a definite amplitude. This type of oscillation is especially interesting, as it seems likely to respond to disturbances rather differently from the others. A damped harmonic motion, for example, is maintained by the disturbances; consequently its mean amplitude is determined by the magnitude and frequency of these, and the damping of its correlogram is that of the oscillating system itself; on the other hand, the amplitude of a relaxation oscillation would probably be little affected by disturbances, which could therefore affect only the phase. There is some evidence that the drafting wave is an oscillation of this type, and that its variations in amplitude are the result of the averaging of the more or less independent waves in different longitudinal sections of the sliver.

The effect of disturbances on these and possibly other types of system could be studied mathematically and also experimentally. Models of them, with any required characteristics, could be constructed, and would be a very convenient means for generating artificial series. Such a study would lead to a better understanding of the meaning of the results of correlogram or periodogram analysis, might, by allowing the characteristics of different types of oscillation to be recognized, enable these to be interpreted more precisely, and might also suggest alternative and more suitable methods of analysis.

Finally, it is perhaps worth while calling attention to the drafting wave-time series as a practically unlimited supply of material for statistical experiments. Samples can be of almost any length, and within limits we can control the wave-length, the amplitude and the damping, and, if necessary, can add simple harmonic variations of any required amplitude and length.

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RANDOM PROCESSES IN PROBLEMS OF AIR WARFARE.

By L. B. C. CUNNINGHAM and W. R. B. HYND.

IN the course of the last two years there has been a striking increase in the interest shown in the application of correlation theory to armament studies; previously, it had been appreciated that much was lacking in the various early mathematical descriptions of weapon performance, but only recently has the remedy been found to lie in the determination of covariances and in the subsequent performance of various operations on the resulting autocorrelation functions. It is now becoming more and more widely recognized that if some military technique requires the measurement of any parameter upon which some fluctuating error is superposed, then it will in general be fruitful to study the autocorrelation of this error. We will subsequently illustrate how this is borne out in the particular cases of the hunting error of a radar set, employed by anti-aircraft gunners to determine the position of a target aircraft; or of a pilot trying to fly on a beam at a constant speed, in ground-controlled radar blind bombing; or of an air-gunner attempting, despite the wander of his aim, to keep his guns trained on a hostile aeroplane. In the computation of correlograms for these problems, we have been saved much tedious and time-consuming work by the employment of a special relay computer which will also be described later.

In war-time it has rarely been necessary to consider very thoroughly the levels of significance to which the autocorrelations were determined : the special computer has made it possible to correlate a large quantity (often all) of the available data of a particular type, and the result had to be regarded as possibly not the true autocorrelation function, but at least the best estimate which could be made in the circumstances, and from which conclusions had to be drawn.

Anti-Aircraft Gunnery : Radar Tracking

The well-known method for obtaining the spectrum of a random process by evaluating the Fourier cosine transform of its correlogram has been used in this country by Shire and Runcorn of the Radio Research and Development Establishment, and in America by Phillips, Weiss and others of the Massachusetts Institute of Technology, in studies of the radar control of anti-aircraft gunfire. The radar set involved, whether automatic or manually operated, will not give smooth information, but will have oscillatory errors superimposed upon the true values. In normal practice these errors are to some degree smoothed, but there is a limit to which this is permissible, else the lag so introduced will cause larger errors at the gun than those due to erroneous fluctuations in the information. It follows that while high-frequency errors may be almost eliminated, those with periods of the order of a few seconds remain. The predictor receives data which is accordingly somewhat unreliable, and its purpose is to make an estimate of the future position of the target aircraft, from which may be determined the necessary azimuth and elevation of the guns and fuze-setting of the shell. In this predictor stage the various harmonic components of the error are magnified by a factor which is a function of their frequency. This function differs markedly from one predictor to another, and it is thus possible to select an optimum when the error spectrum of the particular radar set is known.

In Fig. 1 is drawn the correlogram for errors in elevation of a typical anti-aircraft radar set, together with an estimate of the corresponding spectrum, while Fig. 2 shows the magnification of error graphed against frequency for a particular predictor.

Instead of describing a predictor by its characteristic curve of error magnification, we may study its response to a " Heaviside impulse function." The impulse function (also called the Dirac delta function) is defined by the equations

$$\delta(t)\begin{cases} = 0; & t \neq 0 \\ = \infty; & t = 0 \end{cases} \text{ in such a way that } \int_{-\infty}^{\infty} \delta(t)dt = 1 \quad . \quad . \quad . \quad (1.1)$$

Let the predictor response be $g(t)$ to such an impulsive input. Then its response to any inpu $f(t)$, will be given by

$$G(t) = \int_{-\infty}^{t} f(s)g(t-s)ds = \int_{0}^{\infty} f(t-s)g(s)ds \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.2)$$

showing that the output is (proportional to) a weighted average of all previous values of the input, and that $g(t)$ serves as a weighting function.

If we stop to consider the role of the predictor we may obtain a more complete understanding of the significance of this weighting function. For simplicity, we will consider only one dimension, and assume that the target is flying on a straight course at constant speed v; then its actual position at time t will be vt, while the radar set will record its position as being

$$f(t) = vt + e(t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.3)$$

From the values of this input function f, the predictor has to make two estimates: (i) an estimate,

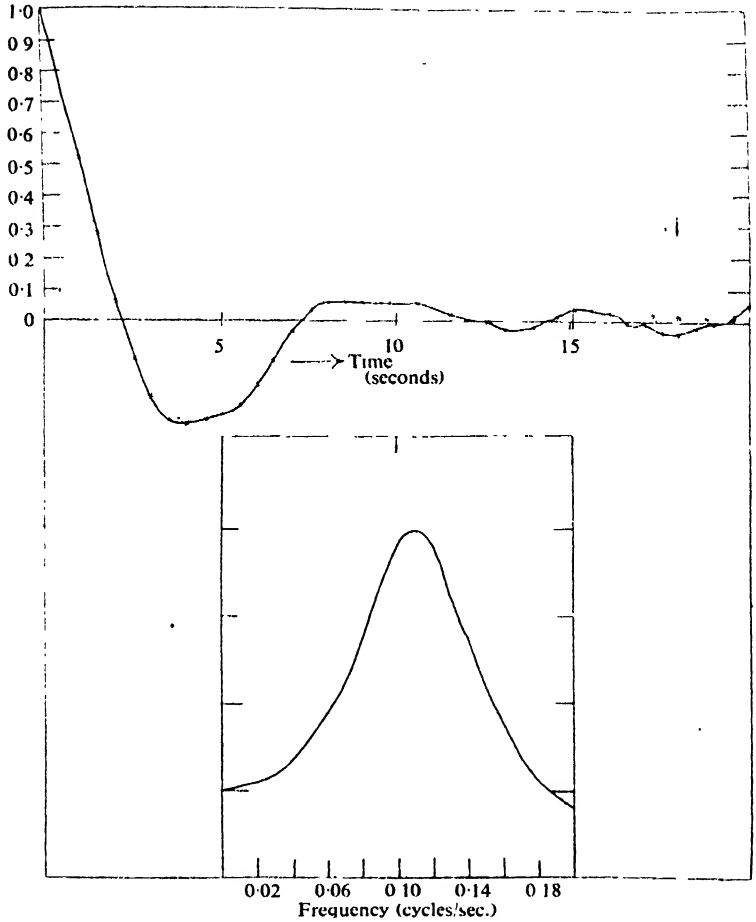

Fig. 1.

$f_1(t)$, of the true position of the aircraft, (ii) an estimate, $f_2'(t)$, of the true velocity of the aircraft, where a dash denotes differentiation with respect to time.

From these estimates the predictor deduces that the aircraft will be at a position $[f_1(t) + T_f f_2'(t)]$ at a time T_f later, where T_f is the time of flight of the anti-aircraft shell; the predictor's efficiency will be assessed by comparing this " output " with the true value $(vt + vT_f)$.

Up to the present time, at least, predictors have been constructed from linear networks, and therefore f_1 and f_2' must be weighted averages of preceding values of the position and velocity of the aircraft, so that

$$f_1(t) = \int_{-\infty}^{t} f(s) g_1(t - s) ds \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.4)$$

and

$$f_2'(t) = \int_{-\infty}^{t} f'(s)g_2(t-s)ds \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.5)$$

where $g_1(t)$ and $g_2(t)$ are the corresponding weighting functions which satisfy the conditions

$$\int_0^\infty g_1(t)dt = \int_0^\infty g_2(t)dt = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.6)$$

Now, it is a property of $\delta'(t)$, the first difference of the Dirac delta function, that

$$\int_b^c \psi(u)\delta'(u-a)du \begin{cases} = \psi'(a); & b < a < c \\ = 0 & \text{otherwise} \end{cases} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.7)$$

so that

$$\int_{-\infty}^{t} g_2(t-s)\delta'(u-s)ds \begin{cases} = g_2'(t-u); & u < t \\ = 0 & u > t \end{cases}$$

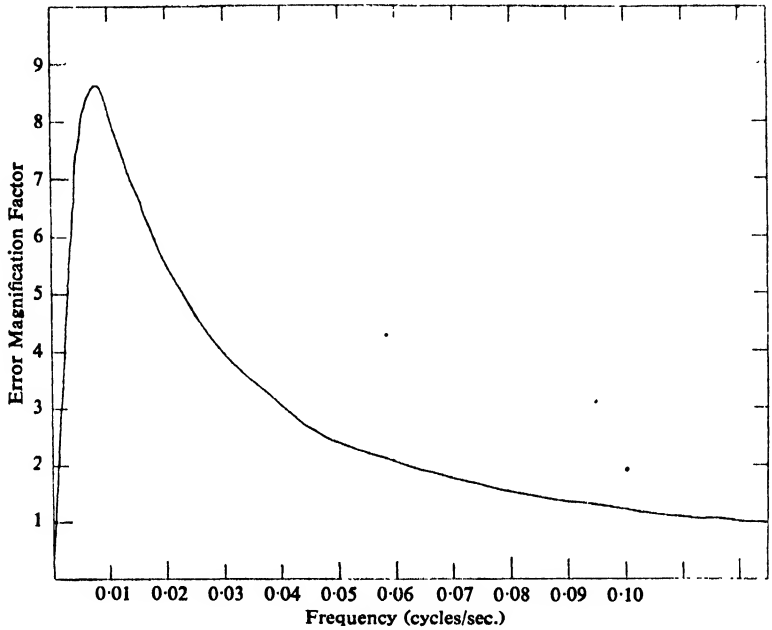

FIG. 2.

and therefore *

$$\begin{aligned} f_2'(t) &= \int_{-\infty}^{t} ds \int_{-\infty}^{\infty} du \,.\, g_2(t-s)f(u)\delta'(u-s) \\ &= \int_{-\infty}^{t} f(u)g_2'(t-u)du \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.8) \end{aligned}$$

From the results (1.4) and (1.8) the predictor output is given by

$$G(t) = f_1(t) + T_f f_2'(t) = \int_{-\infty}^{t} f(s)\{g_1(t-s) + T_f g'_2(t-s)\}ds$$

and as this must be identical with (1.2), it follows that

$$g(s) = g_1(s) + Tg_2'(s) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.9)$$

* The transformation from equation (1.5) to (1.8) might also be achieved by the careful application of the process of integration by parts, due cognizance being taken of the fact that $g(t)$ has a finite discontinuity at $t = 0$.

giving an analysis of the weighting function into two terms, the one concerned solely with smoothing, and the other with smoothing and differentiating, and the weighting function for velocity is obtained from the second term by the rule

$$g_2(s) = \int_0^s g_2'(t)dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.10)$$

where $g_2'(t)$ is the coefficient of T_f in $g(t)$.

Using equations (1.2) and (1.3), the error in the predictor output is given by

$$E = G(t) - (vt + vT_f) = \int_0^\infty [v(t-s) + e(t-s)]g(s)ds - vt - vT_f$$

$$= vt \int_0^\infty g(s)ds - vt - v\int_0^\infty sg(s)ds - vT_f + \int_0^\infty e(t-s)g(s)ds \quad . \quad . \quad . \quad (1.11)$$

When there are no errors $e(t)$ in the radar data, the predictor output error must also be zero, and by equation (1.11) this requires

$$\int_0^\infty g(s)ds = 1 \qquad \text{and} \qquad \int_0^\infty sg(s)ds = -T_f \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.12)$$

The error in predictor output is thus given in general by

$$E = \int_0^\infty e(t-s)g(s)ds \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.13)$$

It is helpful to enunciate here a very general theorem, that if $\bar{O}_1$ and $\bar{O}_2$ denote any two linear operators, the coefficients of which are functions of time, and if $X(t)$ be a random variable with variance σ_X^2 and normalized autocorrelation function ρ_X, then

$$\text{cov}\,[\bar{O}_1X(u);\ \bar{O}_2X(v)] = \sigma_X^2\bar{O}_1\bar{O}_2\rho_X(u-v) \quad . \quad . \quad . \quad . \quad . \quad (1.14)$$

Together with (1.13), this theorem readily gives the variance of E in the form

$$\sigma_E^2 = \sigma_e^2\int_0^\infty\int_0^\infty \rho_e(u-v)g(u)g(v)dudv \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.15)$$

where σ_e^2 and $\rho_e(t)$ are the variance and autocorrelation function for the error $e(t)$ in the radar data.

It is now possible by standard variational procedure, to choose $g(t)$ of such a form as to minimize σ_E^2 while always satisfying the restrictions of equations (1.12). The result is that

$$\int_0^\infty g(u)\rho_e(t-u)du = A + Bt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.16)$$

where A and B are constants to be determined by the equations (1.12).

There are practical reasons for introducing as a further restriction on $g(t)$ that its value should be zero when t is greater than a critical value T. This implies that the estimates of aircraft position and velocity will be based only on radar data supplied to the predictor during the time-interval T immediately preceding. Clearly T must not be too large, for the aircraft presents itself as a target to the gun only for a short period, and even within that time it may frequently vary its velocity or heading, so that our assumption that it flies a straight course at constant speed will be valid for only yet shorter periods. This restriction on $g(u)$ may be expressed in the mathematics by replacing the upper limits of integration in (1.12) and (1.16) by T: the optimum weighting function is thus the solution of the integral equation

$$\int_0^T g(u)\rho_e(t-u)du = A + Bt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.17)$$

where A and B are determined by the conditions

$$\int_0^T g(u)du = 1 \qquad \text{and} \qquad \int_0^T ug(u)du = -T_f \quad . \quad . \quad . \quad (1.18)\ (1.19)$$

The solution of (1.17) will now be obtained in the particular case when the errors in the radar data are of Markoff type, so that

$$\rho_e(t) = e^{-\lambda|t|} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.20)$$

Clearly

$$\int_0^T \rho_e(t-u)du = \int_0^t e^{-\lambda(t-u)}du + \int_t^T e^{\lambda(t-u)}du$$
$$= \frac{1}{\lambda}\left\{2 - e^{-\lambda t} - e^{-\lambda(T-t)}\right\}$$
$$= \frac{1}{\lambda}\left\{2 - \int_0^T \rho_e(t-u)\delta(u)du - \int_0^T \rho_e(t-u)\delta(u-T)du\right\}$$

so that

$$\int_0^T \left\{\frac{\lambda + \delta(u) + \delta(u-T)}{2}\right\} \rho_e(t-u)du = 1$$

and

$$\int_0^T u\rho_e(t-u)du = \frac{2t}{\lambda} - \frac{T}{\lambda}e^{-\lambda(T-t)} + \frac{1}{\lambda^2}\left[e^{-\lambda t} - e^{-\lambda(T-t)}\right]$$
$$= \frac{2t}{\lambda} - \frac{T}{\lambda}\int_0^T \rho_e(t-u)\delta(u-T)du + \frac{1}{\lambda^2}\int_0^T \rho_e(t-u)\left[\delta(u) - \delta(u-T)\right]du$$

so that

$$\int_0^T \frac{1}{2}\left\{\lambda u - \frac{1}{\lambda}\delta(u) + \frac{1}{\lambda}\delta(u-T) + T\delta(u-T)\right\}\rho_e(t-u)du = t$$

It follows that equation (1.17) will be satisfied by

$$g(u) = \frac{A}{2}\left\{\lambda + \delta(u) + \delta(u-T)\right\} + \frac{B}{2}\left\{\lambda u + T\delta(u-T) - \frac{1}{\lambda}\delta(u) + \frac{1}{\lambda}\delta(u-T)\right\}. \quad (1.21)$$

Substituting equation (1.21) into the conditions (1.18) and (1.19) leads to

$$1 = \int_0^T g(u)du = \frac{A}{2}\left\{\lambda T + 2\right\} + \frac{B}{2}\left\{\frac{1}{2}\lambda T^2 + T\right\}$$

and

$$-T_f = \int_0^T ug(u)du = \frac{A}{2}\left\{\frac{1}{2}\lambda T^2 + T\right\} + \frac{B}{2}\left\{\frac{1}{3}\lambda T^3 + T/\lambda + T^2\right\}$$

which may be solved to give

$$A = \frac{8\lambda^2T^2 + 24\lambda T + 24 + 12\lambda T_f(\lambda T + 2)}{\lambda^3T^3 + 8\lambda^2T^2 + 24\lambda T + 24}$$
$$B = \frac{12}{T}\cdot\frac{2\lambda T_f + \lambda T}{\lambda^2T^2 + 6\lambda T + 12}$$

so that

$$g(u) = \frac{\left[\begin{array}{l}\left\{4\lambda^2T^2 + 12\lambda T + 12 + 6\lambda T_f(\lambda T + 2)\right\}\lambda - \left\{6(2\lambda T_f + \lambda T)\left(\frac{\lambda T + 2}{T}\right)\right\}\lambda u \\ \quad + \left\{4\lambda^2T^2 + 18\lambda T + 24 + 6\frac{T_f}{T}(\lambda T + 2)^2\right\}\delta(u) \\ \quad - \left\{6\lambda T + 2\lambda^2T^2 + \frac{6T_f}{T}(\lambda T + 2)^2\right\}\delta(u-T)\end{array}\right]}{\lambda^3T^3 + 8\lambda^2T^2 + 24\lambda T + 24}. \quad (1.22)$$

Integrating the coefficient of T_f in this case shows that, according to equation (1.10), the velocity weighting function in this case is of quadratic form:

$$g_2(u)\begin{cases} = \dfrac{6}{T}\cdot\dfrac{\lambda T + 2 + \lambda^2Tu - \lambda^2u^2}{12 + 6\lambda T + \lambda^2T^2} & \text{in the range } O \leq u \leq T \\ = O \text{ for } u > T \end{cases} \quad . \quad . \quad (1.23)$$

For λT large

$$g(u) \longrightarrow \frac{1}{T^3}\left[T^2 - 6uT + 6T_f(T - 2u)\right]$$

and

$$g_2(u) \longrightarrow \frac{6}{T^3}\Big[Tu - u^2\Big]; \qquad 0 \leq u \leq T$$

—forms which are known as " Bode smoothing functions," useful when the error in the radar data approximates to uncorrelated random noise.

At the other extreme, when λT is small,

$$g(u) \longrightarrow \delta(u) + \frac{T_f}{T}\Big[\delta(u) - \delta(T)\Big]$$

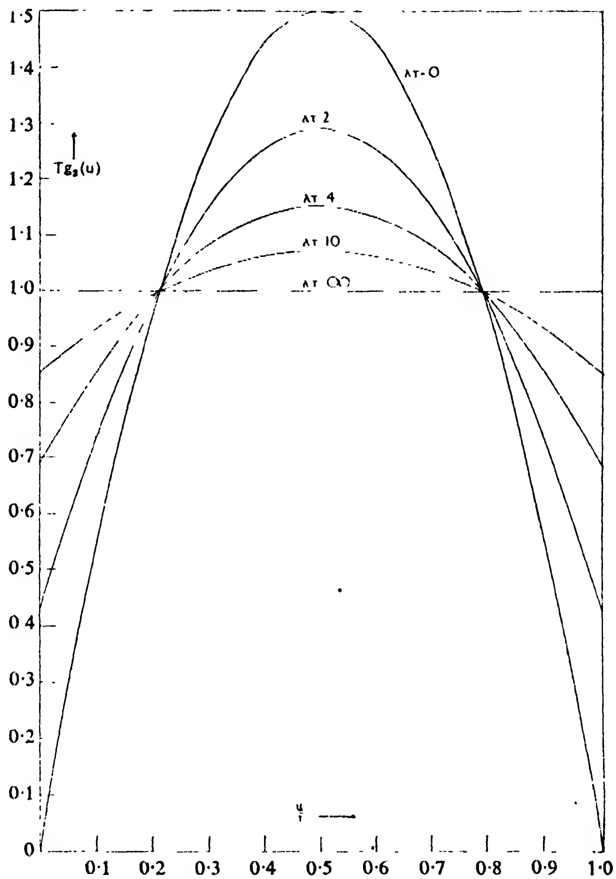

FIG. 3.

and

$$g_2(u) \longrightarrow \frac{1}{T}; \qquad 0 \leq u \leq T$$

so that in this case, when the error is highly correlated over the smoothing interval, the velocities are smoothed by taking their unweighted arithmetical average.

Graphs of the function $g_2(u)$, as given by equation (1.23), are provided in Fig. 3 for various values of λT.

Substitution of the particular forms (1.20), (1.23) and (1.24) into equation (1.15) leads to the following results for the variance of the error in the predictor output:—

In the case of Bode smoothing

$$\sigma_E^2 = \sigma_\epsilon^2\left[\left\{\frac{8}{T} - \frac{20}{T^2} + \frac{72}{T^4} - \frac{8}{T^2}\left(2 + \frac{9}{T} + \frac{9}{T^2}\right)e^{-T}\right\} + \frac{24T_f}{T^2}\left\{1 + \frac{T}{T}\right\}\left\{1 - \frac{3}{T} + \frac{12}{T^3} - \frac{3}{T}\left(1 + \frac{4}{T} + \frac{4}{T^2}\right)e^{-T}\right\}\right] \quad . \quad . \quad (1.25)$$

In the case of delta-function smoothing

$$\sigma_E^2 = \sigma_\epsilon^2\left[1 + 2\frac{T}{T}\left(1 + \frac{T}{T}\right)(1 - e^{-T})\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.26)$$

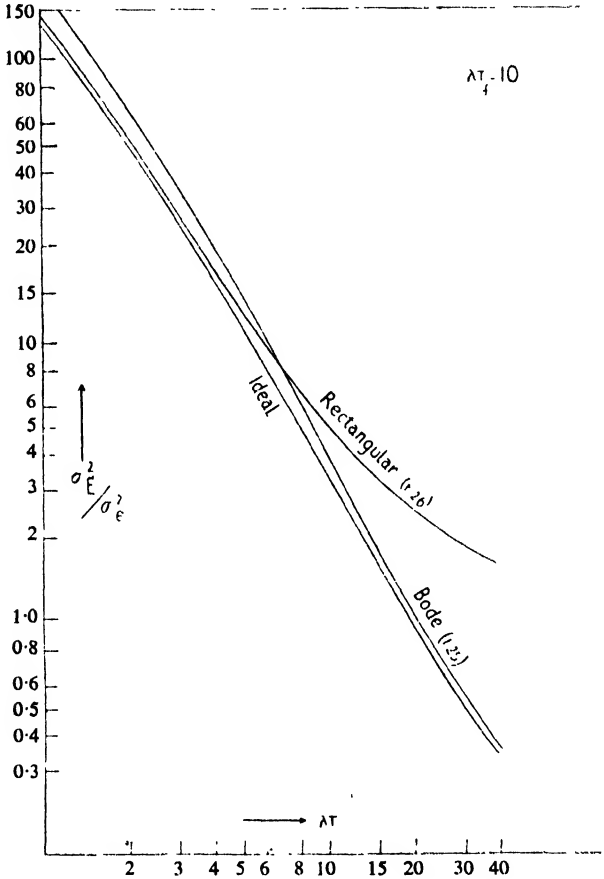

FIG. 4.

The corresponding formula in the case of ideal smoothing is rather more complex, but may be obtained from equations (1.15), (1.20) and (1.21) in the form

$$\sigma_E^2 = \sigma_\epsilon^2\left[A^2 + \frac{T}{2\lambda}(A\lambda + B)^2 + \frac{BT^2}{2}(A\lambda + B) + \frac{1}{6}\lambda B^2T^3\right] \quad . \quad . \quad . \quad (1.27)$$

where A and B are functions of T, T_f and λ previously specified. The expressions (1.25), (1.26) and (1.27) are all illustrated, for a particular value of λT_f, in Fig. 4 (logarithmic scales).

To close this discussion on the feeding of radar data to various predictor circuits, we might make two comments. It is possible, and indeed almost necessary, to introduce some smoothing stage between the radar output and the predictor input. This operation may easily be introduced

into the mathematics, for the output of these additional circuits will have a variance and autocorrelation which may be expressed using equation (1.14) in terms of the corresponding statistics for the unsmoothed original radar data, and of the response of the smoothing equipment to an impulse function. The above theory is then applicable to these new statistics, which describe the predictor input.

No mention has as yet been made here as to the duration, T, which should be chosen for the smoothing interval; indeed, in the case of formulæ (1.25) and (1.26) and the corresponding Fig. 4, it is clear that the greater the value of T the smaller is the error in prediction, and this would in general be found true when the radar errors had other autocorrelation functions. For reasons mentioned earlier, this is in disagreement with physical arguments, the discrepancy being due to our failure to introduce into the mathematics some measure of the fact that with increasing time the aircraft's position is more and more likely in practice to diverge from the value, vt, which has been assumed. Studies of this latter effect might be based on recorded tracks of aircraft over gun-defended areas, the statistical analysis of which would again require the computation of something of the nature of an autocorrelation; this part of the problem is similar to a random walk in which the steps are of variable length and there is a restriction on the angle between successive steps.

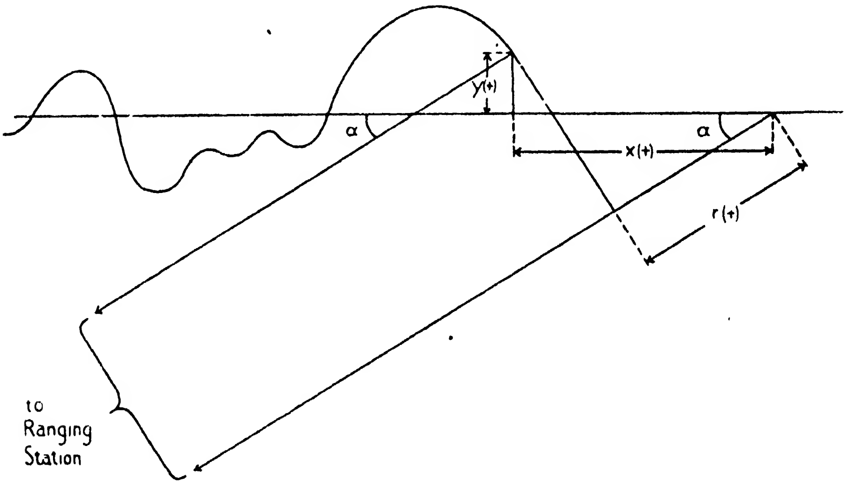

FIG. 5.

With this information analysed, it should be possible to improve upon the above analysis and to determine an optimum smoothing interval, long enough for the radar error to be effectively averaged, but short enough for only a small error to accrue from the freedom of the target aircraft to change its speed and heading. Going farther in this direction, it would probably be possible completely to eliminate the rather arbitrary restriction

$$g(u) = 0 \quad \text{for} \quad u > T$$

and to determine overall optimum weighting functions which would define both the predictor circuits and the smoothing circuits which may be introduced between the radar equipment and the guns.

Non-Visual Bombing : Beam Flying

A somewhat similar problem of radar ranging, smoothing and differentiating arises in ground-controlled " blind " bombing, and has been studied by us on behalf of the " Oboe " system used by the Pathfinder Force of Bomber Command. The broad details of such a blind bombing technique are illustrated in Fig. 5.

To assist the bomber pilot in his attempt to fly on a certain straight path which will take him over the target, he is provided with continuous radio information as to his distance from this intended course; a highly-accurate radar equipment meanwhile determines the range of the aircraft

from a ground station in friendly territory, and estimates the aircraft's forward velocity from the rate of change of this range. Knowing the time which the bombs will take to drop to target-level, it is then theoretically possible to predict the range at which they should be released, and when the aircraft reaches this range it receives instructions to this effect from the ground station.

One feature of distinction between the predictor problem and the present case is that the behaviour of the aircraft is much more restricted; its statistical description is therefore comparatively easy to determine, and has been fully included in the mathematics which follows. It is also important to note that the "jitter" inevitable in the radar ranges is constituted from much higher frequency components than is any of the other variables involved, so that radar errors may be completely filtered out and eliminated. There remain two sources of fluctuation which are of importance—fluctuations of the aircraft to one side and the other of the intended track, and fluctuations in the aircraft's forward velocity (such as might arise from small changes in engine power).

We will assume that the ground station is at a great distance from the target, so that the angle, α, between the intended course and the line joining aircraft to station, may be regarded as constant throughout the bombing run.

Let $x(t)$, $y(t)$ and $r(t)$—variables all dependent on the time t—measure the components of the distance between target and aircraft in the directions along the intended track, perpendicular to this track, and towards the ground station. We have

$$r = x \cos \alpha - y \sin \alpha \quad \text{or} \quad x = r \sec \alpha + y \tan \alpha \quad . \quad . \quad . \quad . \quad (2.1)$$

The ground station has no information on the value of $y(t)$ at any time, and so must attribute all changes in r to changes in x: it therefore makes an estimate of the aircraft's instantaneous forward velocity in accordance with the equation.

$$\begin{aligned} \dot{x}_{est} &= \sec \alpha \, . \, \dot{r} \\ &= \dot{x} - \dot{y} \tan \alpha ; \end{aligned}$$

then, realizing that this value is subject to error, the ground station may modify it by taking a weighted average value, so as to smooth out to some degree the undesirable y term, at the cost of losing instantaneity in the x term. Accordingly, the aircraft velocity is assessed by the ground station as being

$$V(t) = \int_{-\infty}^{t} \left\{ \dot{x}(u) - \dot{y}(u) \tan \alpha \right\} g(t-u) du \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.2)$$

where $g(t)$ is the weighting function introduced in the smoothing process, and satisfies

$$\int_0^{\infty} g(u) du = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.3)$$

A release signal will be sent to the bomber when its range from the target is $r(t_r)$, at time t_r, satisfying

$$\sec \alpha \, . \, r(t_r) = - T_b \, . \, V(t_r) = - T_b \int_{-\infty}^{t_r} \left\{ \dot{x}(u) - \dot{y}(u) \tan \alpha \right\} g(t_r - u) du \quad . \quad . \quad (2.4)$$

where T_b is the time of fall of the bomb.

The point of impact of the bomb on the ground will then be (X, Y), where

$$X = x(t_r) + T_b \dot{x}(t_r) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.5)$$

$$Y = y(t_r) + T_b \dot{y}(t_r) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.6)$$

Let

$$\dot{x}(t) = v + e_v(t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.7)$$

so that $e_v(t)$ is a random variable with zero mean.

Then using equations (2.5), (2.7), (2.1) and (2.4) in successive steps:—

$$\begin{aligned} X &= x(t_r) + vT_b + T_b e_v(t_r) \\ &= r(t_r) \sec \alpha + y(t_r) \tan \alpha + vT_b + T_b e_v(t_r) \\ &= T_b \left[e_v(t_r) - \int_{-\infty}^{t_r} e_v(u) g(t_r - u) du \right] + \tan \alpha \left[y(t_r) + T_b \int_{-\infty}^{t_r} \dot{y}(u) g(t_r - u) du \right] \quad . \quad . \quad (2.8) \end{aligned}$$

As the angle between the intended and achieved aircraft tracks is usually of the order of one degree or less, it is reasonable to assume that e and y are uncorrelated. If σ_e^2, σ_y^2 and $\rho_e(t)$, $\rho_y(t)$ are the variances and autocorrelation functions of these variables, the application of the general result (1.14) to equations (2.8) and (2.6) leads to the following expressions for the variances of the components of the bombing error:—

$$\sigma_X^2 = T_b^2\sigma_e^2\left[1 - 2\int_0^\infty \rho_e(u)g(u)du + \int_0^\infty\int_0^\infty \rho_e(u-v)g(u)g(v)dudv\right]$$
$$+ \tan^2\alpha\sigma_y^2\left[1 - 2T_b\int_0^\infty \dot\rho_y(u)g(u)du - T_b^2\int_0^\infty\int_0^\infty \ddot\rho_y(u-v)g(u)g(v)dudv\right] \quad . \quad . \quad (2.9)$$

$$\sigma_Y^2 = \sigma_y^2\left[1 - T_b^2\ddot\rho_y(0)\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.10)$$

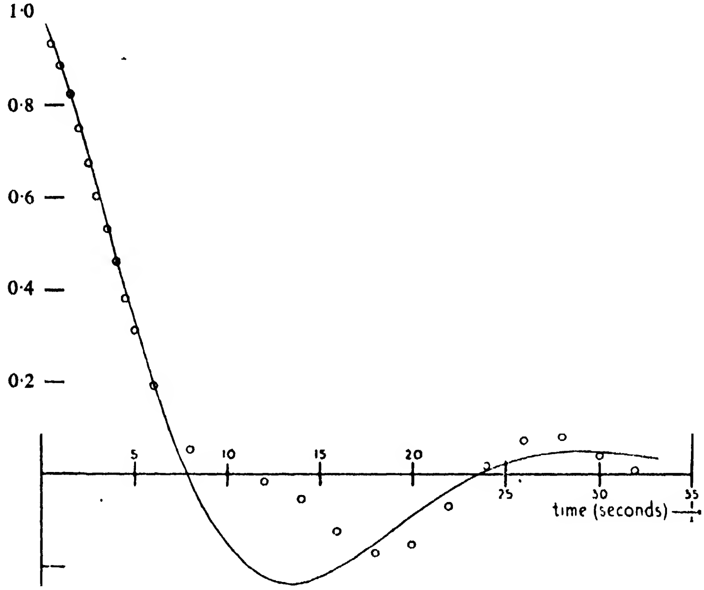

FIG. 6.

This shows that σ_Y, the R.M.S. " line " bombing error, is independent of the smoothing function, and only the R.M.S. " range " bombing error, σ_X, need be studied.

Experimental data (which was obtained on operational sorties by Pathfinder aircraft) led to the estimates

$$\sigma_e = 4{\cdot}74 \text{ m.p.h.}$$
$$\sigma_y = 30 \text{ yards} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.11)$$

Values of $\rho_e(t)$ were computed from the same data, and are graphed in Fig. 6, together with a fitted curve of the form

$$\rho_e(t) = e^{-\lambda|t|}\cos\mu t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.12)$$

which was used in the evaluation of the integrals in (2.9), and likewise values of $\rho_y(t)$ are graphed in Fig. 7, together with a fitted curve of the form

$$\rho_y(t) = (1 - ae^{-\nu t^2})e^{-\lambda t^2}\cos\mu t + ae^{-\nu t^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.13)$$

These experimental results were passed into the formula (2.9), which was then used to compare the efficiency of various simple forms for $g(u)$ such as

Case (i) $g(u) = Ke^{-Ku}$ for various values of K

Case (ii) $g(u) \begin{cases} = 1/T \text{ in the range } t < u < t + T \\ = 0 \text{ otherwise} \end{cases}$

for various values of t and T.

A typical group of results is provided in Fig. 8, in which the range bombing error, σ_x, is graphed against the angle, α, for a variety of weighting functions $g(u)$, the time of bomb-fall being fixed at a value corresponding to a release altitude of 30,000 feet.

It is clearly illustrated that for small values of α the minimum practical degree of smoothing should be adopted, but when α increases above a critical value of about 30°, the optimum changes

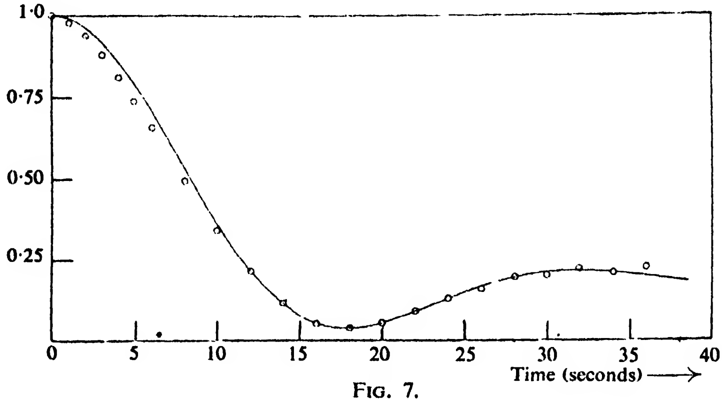

FIG. 7.

abruptly, and the more extensive the smoothing the better will be the resulting bombing: the variation of the bombing error with the angle α is marked when little smoothing is employed, but is relatively unimportant when long-term averages are used. These comments are valid whatever may be the precise form of the weighting function introduced.

Examination of Case (ii) curves shows that if the smoothing period, T, is kept constant and the

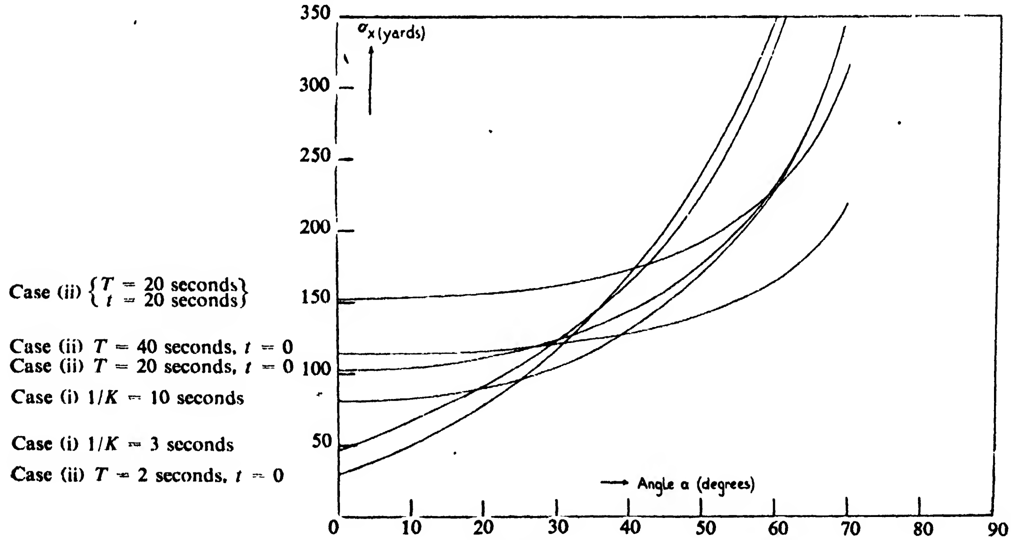

FIG. 8.

time interval, t, be varied between the end of this period and the moment of bomb release, there is no great change in the bombing error: indeed, the introduction of the quiescent interval t may improve performance; the curves for $T = 20$ seconds, t zero and for $T = 20$ seconds, $t = 20$ seconds are very similar and the latter is associated with the *lower* bombing error when α is greater than 60°: even for smaller values of α there is little harm in introducing such an interval if it should be advantageous for other reasons.

It seems appropriate to introduce at this stage some results on continuous random processes which were originally obtained in connection with beam flying, but are of much wider and more general interest.

If the pilot exercised the greatest possible skill, and if his actions are quite free from error, he will use only the ailerons of his aircraft so as to control the roll acceleration: it then follows from aerodynamical reasoning that, in this perfect case, the motion of the aircraft will be described by a fourth order differential equation

$$y^{\text{iv}} + ay''' + by'' + cy' + dy = 0 \quad \ldots \ldots \quad (2.14)$$

In practice, no pilot, however skilful, will achieve this: there will always be random errors in his efforts in controlling the motion, so that (2.14) must be replaced by

$$y^{\text{iv}} + ay''' + by'' + cy' + dy = Y(t) \quad \ldots \ldots \quad (2.15)$$

where $Y(t)$ is a random variable, with zero mean, and its successive values will in general be correlated. Equation (2.15) may be written

$$[D^4 + aD^3 + bD^2 + cD + d]y(t) = Y(t)$$

where D represents the differential operator d/dt.

Many pilots will either not possess the requisite skill, or will consider it unnecessarily refined, to adjust their distance off track by controlling their ailerons; instead, they will simply apply small rudder movements, and the aircraft's motion will then inevitably suffer from some degree of "sideslip." Under these conditions the corresponding differential equation is *approximately* of the second order only:

$$by'' + cy' + dy = Y(t) \quad \ldots \ldots \quad (2.16)$$

or

$$[bD^2 + cD + d]y(t) = Y(t) \quad \ldots \ldots \quad (2.17)$$

Further, the pilot's reactions may not be instantaneous: the control which he applies at time t may depend on information as to his distance off track which he has received at time $(t - v)$; the damping term may also contain such a lag. Equation (2.16) must then be replaced by

$$by(t) + c\dot{y}(t - u) + dy(t - v) = Y(t)$$

and the equation corresponding to (2.17) is

$$[bD^2 + cDe^{-uD} + de^{-vD}]y(t) = Y(t) \quad \ldots \ldots \quad (2.18)$$

Weighted averages may again be introduced: if the acceleration at time t depends on an average of all the information received earlier, then de^{-vD} in (2.18) should be replaced by

$$l\int_0^{\infty} \quad {}^{D}g(v)dv$$

where $g(v)$ is, as usual, the weighting function.

All these cases will be covered if we can examine the quite general formula

$$F(D) \,.\, y(t) = Y(t) \quad \ldots \ldots \quad (2.19)$$

where $Y(t)$ has variance σ_Y^2, autocorrelation function $\rho_Y(t)$, and spectrum function given by

$$S_Y(\omega) = \frac{2}{\pi}\int_0^{\infty} \rho_Y(t) \cos \omega t dt \quad \ldots \ldots \quad (2.20)$$

and if we can obtain the corresponding statistics for $y(t)$.

Now if D_t denote $\frac{d}{dt}$, and D_s denote $\frac{d}{ds}$, then

$$\left[\frac{1}{F(D_t)F(D_s)}\right] \cos (t - s)\omega = \frac{\cos (t - s)\omega}{F(i\omega)F(-i\omega)} \quad \ldots \ldots \quad (2.21)$$

The residual steady state solution of (2.19) (the complementary function having died out) is

$$y(t) = \frac{1}{F(D)} Y(t)$$

Referring once again to the result (1.14), it follows that

$$\sigma_y^2\rho_y(t-s) = \text{covar}\,[y(t),y(s)] = \frac{\sigma_Y^2}{F(D_t)F(D_s)}\rho_Y(t-s)$$

$$= \frac{\sigma_Y^2}{F(D_t)F(D_s)}\int_0^\infty S_Y(\omega)\cos[(t-s)\omega]d\omega$$

$$= \sigma_Y^2\int_0^\infty S_Y(\omega)\frac{\cos(t-s)\omega}{F(i\omega)F(-i\omega)}d\omega \qquad \text{by equation (2.21);}$$

so that

$$\sigma_y^2 = \sigma_Y^2\int_0^\infty \frac{S_Y(\omega)}{F(i\omega)F(-i\omega)}d\omega \quad . \quad . \quad . \quad (2.22)$$

$$S_y(\omega) = \frac{\sigma_Y^2}{\sigma_y^2}\cdot\frac{S_Y(\omega)}{F(i\omega)F(-i\omega)} \quad . \quad . \quad . \quad (2.23)$$

and

$$\rho_y(t) = \int_0^\infty S_y(\omega)\cos t\omega d\omega \quad . \quad . \quad . \quad (2.24)$$

For example, consider a case which has been mentioned by Dr. Bartlett:

$$\ddot{y} + \alpha\dot{y} + \beta y = \epsilon(t)$$

where $\epsilon(t)$ is an uncorrelated random variable: $S_\epsilon(\omega)$ may be represented in this case by the formula

$$S_\epsilon(\omega) = \lim_{\lambda\to\infty}\frac{2\lambda}{\pi(\lambda^2+\omega^2)}$$

the variable ϵ being thus regarded as the limiting form of a variable of Markoff type, with an auto-correlation function $e^{-\lambda|t|}$. Define also

$$I = \int_0^1 \epsilon(u)du$$

Then

$$\frac{\sigma_I^2}{\sigma_\epsilon^2} = \int_0^1\int_0^1 e^{-\lambda|u-v|}dudv = \frac{2}{\lambda}\left(1-\frac{1}{\lambda}+\frac{1}{\lambda}e^{-\lambda}\right) \longrightarrow \frac{2}{\lambda}\text{ for large }\lambda$$

and

$$\frac{\sigma_y^2}{\sigma_\epsilon^2} = \int_0^\infty \frac{2\lambda d\omega}{\pi(\lambda^2+\omega^2)[(\beta-\omega^2)^2+\alpha^2\omega^2]} \longrightarrow \frac{1}{\lambda\alpha\beta}$$

so that

$$\sigma_y^2 \sim \frac{\sigma_I^2}{2\alpha\beta} \quad . \quad . \quad . \quad (2.25)$$

and also

$$S_y(\omega) = \lim_{\lambda\to\infty}\frac{\sigma_\epsilon^2}{\sigma_y^2}\frac{2\lambda}{\pi(\lambda^2+\omega^2)}\frac{1}{[(\beta-\omega^2)^2+\alpha^2\omega^2]} = \frac{2\alpha\beta}{\pi[(\beta-\omega^2)^2+\alpha^2\omega^2]} \quad . \quad (2.26)$$

Formulae (2.25) and (2.26) are in agreement with the formulæ given by Dr. Bartlett.

Correlation theory might be usefully applied to other bombing problems: for instance, in formation bombing, each aircraft tries to keep station with respect to one preceding aircraft which has been designated as his "senior." A full understanding of the distribution of the aircraft in space, and of their velocities, will therefore require determination of the correlation between the fluctuations of one aircraft and those of another. Unfortunately, no practical data have yet become available which might be analysed in this manner. The effect of such correlations, and of the consequent correlations in bombing error for different aircraft, would be assessed by their influence on the chance that a target will receive a direct hit by at least one bomb, and the necessary mathematical formulæ will resemble those which will be presented in our next problem—the effect of the correlation between successive rounds on the efficiency of aerial gunnery.

Performance of Guns with a High Rate of Fire

When our attention is turned to studies of rapidly-firing guns, a very different correlation problem is presented. As a gunner tries to keep his gun trained on a target in unsteady conditions, the point of aim performs an erratic, quasi-orbital motion which is generally referred to as " aim wander," and is plainly a typical random process in two dimensions. Its practical effect is that of a discrete time series, picked out by the discharge, at approximately equal intervals of time, of successive rounds from the gun. The kinetic reactions to successive shots might influence the sequence, but this is usually found negligible, so that cine-gun analysis is valid.

Each round, once fired, does not of course proceed exactly along the direction of aim, but will diverge from it, as a consequence of such factors as ballistic asymmetry. This divergence (which has been termed " gun dispersion," as opposed to the aim-wander or dispersion of aim) will vary in an uncorrelated manner from one round to the next, and the population of these divergences is represented with high fidelity by a Gaussian distribution about the point of aim.

The importance of the correlation in aim-wander must be assessed by its influence on the survival chance of a target to an arbitrary burst of gunfire, compared with its chance when there is no correlation. In the simplest case,* this is the chance that every round in the burst considered should miss the target, having regard both to the aiming and gun dispersions. In other problems in this paper we have been interested in previous values of a random variable only in so far as they had an influence on the value at some later time; in the survival chance, previous values of the aiming error not only influence future values, but each also defines a momentary risk run by the target, and makes its own direct contribution to the final survival chance. No analysis of problems of such a character seems to exist in the mathematical literature. The greater the correlation, the greater will be the coherence between the bullets, and the smaller will be the equivalent number of independent uncorrelated rounds: aim-wander correlation thus reduces the lethality of the gunfire. That this reduction may be drastic is illustrated by the following example.

Aiming statistics obtained by analysis of cine-gun films exposed in R.A.F. gunnery trials were interpreted in two different ways: firstly, paying due regard to the sequence of points of aim achieved in each sample attack; secondly, considering only the overall distribution of aiming errors, and thus ignoring the correlation. Each method was used, in turn, to compute, from the same aiming data, a fighter pilot's chance of success in a typical attack with a $2\frac{1}{2}$-second burst of fire from four cannon, the mean target range during the sample population of bursts considered being 100 yards, and the pilot using a fixed reflector sight. By the former (correct) method this chance was found to be 29 per cent., but the latter method led to a gravely erroneous value of 97 per cent.

Discrepancies of this order between theory and practice cannot well be tolerated, and it is plain that a valid formula for the survival chance should be constructed. Unfortunately, the problem seems to be exceedingly intractable, and no closed formula has been discovered which is suitable for computation. The true value may nevertheless be set between limits: a lower limit is readily calculable by ignoring correlation between successive rounds, while an upper limit, obtainable rather less readily but still with no great difficulty, is offered by assuming perfect correlation, all rounds being discharged with the same point of aim, so that the rounds of a burst may be compared to the pellets scattered from a single cartridge fired in a shot-gun. This upper limit will accordingly be referred to as the " shot-gun formula." Experimental values have shown that where the simple theory of uncorrelated rounds is most erroneous, the shot-gun formula gave good agreement. We have therefore expressed the survival chance as a Maclaurin series, the leading term of which is actually the shot-gun formula, and as subsequent terms generally alternate in sign, partial sums give successive lower and upper limits.

For those readers who are interested in the results obtained rather than in the details of the methods by which they are derived, the mathematics which follows may be briefly summarized. After some preliminary remarks on the statistical functions which describe aim-wander, the desired survival chance is immediately written in a form (3.10) which is equivalent to a multiple integral: when the aiming errors are statistically symmetrical, this is exactly converted, by a Fourier transformation and inversion, into the single integral (3.16), in which the integrand contains certain operators which must be interpreted: this in turn is achieved by a series expansion, the resulting

* Some targets are more complex: a two-engined aircraft, for instance, may require a hit in each of its two engines before it will crash.

terms being simplified by an approximation which is valid for small targets (the replacement of (3.22) by (3.23)), giving finally the expression (3.25) for the survival chance.

We will consider initially the case when a burst of M rounds is fired, the time-intervals between successive rounds being all equal to the value τ. Let the error in aim for the rth round have angular components (x_r, y_r) with respect to axes which are orthogonal to one another and to the direction of attack. For such bursts of fire, directed by a gunner of a certain degree of skill under a particular set of conditions, each of these components is found in practice to belong to a normal population, so that their distribution functions are

$$P(x_r) = \frac{1}{\sigma_{xr}\sqrt{2\pi}} e^{-\frac{x_r^2}{2\sigma_{xr}^2}} \quad . \quad . \quad . \quad . \quad (3.1)$$

$$P(y_r) = \frac{1}{\sigma_{yr}\sqrt{2\pi}} e^{-\frac{y_r^2}{2\sigma_{yr}^2}} \quad . \quad . \quad . \quad . \quad (3.2)$$

Note that the conditions under which the rth round is fired may differ from the conditions obtaining at other times during the burst, so that the suffix r must be added to the standard deviations.

Further work will be expressed in terms of normalized aiming error components u_r, v_r where

$$u_r = \frac{x_r}{2\sigma_{xr}}; \qquad v_r = \frac{y_r}{2\sigma_{yr}} \quad . \quad . \quad . \quad . \quad (3.3)$$

Presuming that aim-wander is a stationary process, it then follows that these normalized components will satisfy multivariate normal laws, so that

$$P(u_1, \ldots u_r, \ldots u_M)du_1 \ldots du_M = \frac{|a_{mr}|^{\frac{1}{2}}}{\pi^{M/2}} \exp\left[-\sum_{mr} a_{mr}u_m u_r\right] du_1 \ldots du_M \quad . \quad . \quad (3.4)$$

defines the chance that the nominated sequence of normalized wander components will be observed within differential tolerance limits, at the nominated sequence of instants.

In (3.4), $a_{mr} = a_{rm}$ and the quadratic form $\Sigma a_{mr}u_m u_r$ is positive definite.

Defining

$$A_{mr} = \frac{1}{|a_{mr}|} \times \left[\text{co-factor of } a_{mr} \text{ in } |a_{mr}|\right]$$

it is well known that A_{mr} measures the correlation between u_m and u_r, so that we may write

$$A_{mr} = \rho_u(|m-r|\tau) \quad . \quad . \quad . \quad . \quad (3.5)$$

Another standard result [1] is that

$$E_a[F(u_1, \ldots u_M)] = |a_{mr}|^{\frac{1}{2}} E_A[G(\alpha_1, \ldots \alpha_M)] = |A_{mr}|^{-\frac{1}{2}} E_A[G(\alpha_1 \ldots \alpha_M)]. \quad . \quad (3.6)$$

where E_a denotes the expectation of an arbitrary function, F, of the u_r, which are distributed in accordance with (3.4); E_A denotes the expectation of a function of other variables, α_r, which are distributed in accordance with

$$P(\alpha_1, \ldots \alpha_M) = \frac{|A_{mr}|^{\frac{1}{2}}}{\pi^{M/2}} \exp\left[-\sum_{mr} A_{mr}\alpha_m\alpha_r\right];$$

and the function G is the Fourier transform of the function F, so that

$$G(\alpha_1, \ldots \alpha_M) = \frac{1}{\pi^{M/2}} \int_{-\infty}^{\infty} d\xi_1 \ldots \int_{-\infty}^{\infty} d\xi_M F(\xi_1 \ldots \xi_M) e^{2i\sum_r(\alpha_r\xi_r)}$$

The above remarks have been confined to the u components of the aiming errors, but the v are also distributed in accordance with a corresponding multivariate normal law:—

$$P(v_1, \ldots v_M)dv_1 \ldots dv_M = \frac{|b_{mr}|^{\frac{1}{2}}}{\pi^{M/2}} \exp\left[-\sum_{mr} b_{mr}v_m v_r\right] dv_1 \ldots dv_M \quad . \quad . \quad . \quad (3.7)$$

and in this case

$$B_{mr} = \frac{1}{|b_{mr}|} \times \left[\text{co-factor of } b_{mr} \text{ in } |b_{mr}|\right] = \rho_v(|m-r|\tau) \quad . \quad . \quad . \quad (3.8)$$

Combining (3.4) and (3.7) the u's and v's being uncorrelated, it is possible to write

$$P(u_1, \ldots u_M; v_1, \ldots v_M) = \frac{|a_{mr}|^{\frac{1}{2}}|b_{mr}|^{\frac{1}{2}}}{\pi^M} \exp\left[-\sum_{mr} a_{mr}u_m u_r - \sum_{mr} b_{mr}v_m v_r\right]$$

and from this it follows, just as (3.6) followed from (3.4), that

$$E_{ab}\Big[F(u_1, \dots u_M;\ v_1, \dots v_M)\Big] = |A_{mr}|^{-\frac{1}{2}}|B_{mr}|^{-\frac{1}{2}}E_{AB}\Big[G(\alpha_1, \dots \alpha_M;\ \beta_1, \dots \beta_M)\Big]$$

$$= \frac{1}{\pi^M}\int_{-\infty}^{\infty}d\alpha_1 \dots \int_{-\infty}^{\infty}d\alpha_M\int_{-\infty}^{\infty}d\beta_1 \dots \int_{-\infty}^{\infty}d\beta_M G(\alpha_1, \dots \alpha_M;\ \beta_1, \dots \beta_M)e^{-\sum\limits_{mr}(A_{mr}\alpha_m\alpha_r + B_{mr}\beta_m\beta_r)} \tag{3.9}$$

where $G(\alpha_1, \dots \alpha_M;\ \beta_1, \dots \beta_M)$ is the Fourier transform of the arbitrary function $F(u_1, \dots u_M;\ v_1, \dots v_M)$ and the significance of the notation E_{ab}, E_{AB} is obvious.

Let the rth round, fired when the normalized components of aiming error are u_r and v_r, have a chance $Q_r(u_r, v_r)$ of missing its target. Then the chance that the target will survive the burst of M rounds is given by

$$\Phi_M = E_{ab}\prod_{s=1}^{M} Q_s(u_s, v_s) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \tag{3.10}$$

Applying (3.9),

$$\Phi_M = \prod_{s=1}^{M}\Big[\frac{1}{\pi}\int_{-\infty}^{\infty}d\alpha_s\int_{-\infty}^{\infty}d\beta_s G_s(\alpha_s, \beta_s)\Big]e^{-\sum\limits_{mr}(A_{mr}\alpha_m\alpha_r + B_{mr}\beta_m\beta_r)} \quad . \quad . \quad . \tag{3.11}$$

where

$$G_s(\alpha_s, \beta_s) = \int_{-L}^{L}d\xi_s\int_{-L}^{L}d\eta_s e^{2i(\alpha_s\xi_s + \beta_s\eta_s)}Q_s(\xi_s, \eta_s)$$

L being a very large finite limit, such that the chance of erring by so much with either component is negligible.

Inverting this transformation leads to

$$\prod_{s=1}^{M} Q_s(u_s, v_s) = \prod_{s=1}^{M}\Big[\frac{1}{\pi}\int_{-\infty}^{\infty}d\alpha_s\int_{-\infty}^{\infty}d\beta_s G_s(\alpha_s, \beta_s)e^{-2i(\alpha_s u_s + \beta_s v_s)}\Big]$$

so that if f be an arbitrary function satisfying certain broad and obvious conditions,

$$f\Big[-\frac{1}{4}\sum_{mr}\Big\{(1 - A_{mr})\frac{\partial^2}{\partial u_m\partial u_r} + (1 - B_{mr})\frac{\partial^2}{\partial v_m\partial v_r}\Big\}\Big]\prod_{s=1}^{M} Q_s(u_s, v_s)$$

$$= \prod_{s=1}^{M}\Big[\frac{1}{\pi}\int_{-\infty}^{\infty}d\alpha_s\int_{-\infty}^{\infty}d\beta_s G_s(\alpha_s, \beta_s)e^{2i(\alpha_s u_s + \beta_s v_s)}f\Big[\sum_{mr}\Big\{(1 - A_{mr})\alpha_m\alpha_r + (1 - B_{mr})\beta_m\beta_r\Big\}\Big]\Big] \tag{3.12}$$

As a particular case of (3.12), the inverse Fourier transform of

$$e^{\sum\limits_{mr}\{(1 - A_{mr})\alpha_m\alpha_r + (1 - B_{mr})\beta_m\beta_r\}}\prod_{s=1}^{M} G_s(\alpha_s, \beta_s)$$

is

$$e^{-\frac{1}{4}\sum\limits_{mr}\left\{(1 - A_{mr})\frac{\partial^2}{\partial u_m\partial u_r} + (1 - B_{mr})\frac{\partial^2}{\partial v_m\partial v_r}\right\}}\prod_{s=1}^{M} Q_s(u_s, v_s) \quad . \quad . \quad . \quad . \quad . \tag{3.13}$$

When the u and v components are each completely correlated, so that $A_{mr} = B_{mr} = 1$, let E_0 denote the expectation either of a function of the u's and v's or of a function of the α's and β's. Then (3.9) becomes in this special case

$$E_0 .\ F(u_1, \dots u_M;\quad v_1, \dots v_M) = E_0 .\ G(\alpha_1, \dots \alpha_M;\quad \beta_1, \dots \beta_M)$$

so that, using (3.13)

$$E_0\Big[\exp\Big\{-\frac{1}{4}\sum_{mr}\Big[(1 - A_{mr})\frac{\partial^2}{\partial u_m\partial u_r} + (1 - B_{mr})\frac{\partial^2}{\partial v_m\partial v_r}\Big]\Big\}\prod_{s=1}^{M} Q_s(u_s, v_s)\Big]$$

$$= E_0\Big[\exp\Big[\sum_{mr}\Big\{(1 - A_{mr})\alpha_m\alpha_r + (1 - B_{mr})\beta_m\beta_r\Big\}\Big]\prod_{s=1}^{M} G_s(\alpha_s, \beta_s)\Big]$$

$$= \prod_{s=1}^{M}\Big[\frac{1}{\pi}\int_{-\infty}^{\infty}d\alpha_s\int_{-\infty}^{\infty}d\beta_s G_s(\alpha_s, \beta_s)\Big]\exp\Big[\sum_{mr}\Big\{(1 - A_{mr})\alpha_m\alpha_r + (1 - B_{mr})\beta_m\beta_r\Big\} - \sum_{mr}(\alpha_m\alpha_r + \beta_m\beta_r)\Big]$$

$= \Phi_M$ in accordance with (3.11) . . . . (3.14)

Also

$$E_0 .\ F(u_1, \dots u_M;\ v_1, \dots v_M) = \frac{1}{\pi}\int_{-\infty}^{\infty}du\int_{-\infty}^{\infty}dv F(u, u, \dots u;\ v, v, \dots v)e^{-u^2 - v^2}$$

The ultimate formal expression for the survival chance is therefore

$$\Phi_M = \frac{1}{\pi}\int_{-\infty}^{\infty} du \int_{-\infty}^{\infty} dv e^{-u^2 - v^2 - \frac{1}{4}\sum_{mr}\left[(1-A_{mr})\frac{\partial^2}{\partial u_m \partial u_r} + (1-B_{mr})\frac{\partial^2}{\partial v_m \partial v_r}\right]} \prod_{s=1}^{M} Q_s(u_s, v_s) \quad . \quad . \quad (3.15)$$

in the integrand of which all the u_r are to be equated to u, and all the v_r to v, after the differential operators in the exponent have been fully interpreted, and before the integrations with respect to u and v are carried out.

If the aim-wander be statistically symmetrical, then

$$\rho_u(t) = \rho_v(t) = \rho(t)$$

and σ_{xr}, σ_{yr} are equal to a common value, $\frac{\sigma_{ar}}{\sqrt{2}}$, so that σ_{ar} is the standard deviation of the radial error in aim under the conditions associated with the rth round.

The polar co-ordinates of the point of aim will then be $(\sigma_{ar} w_r, \gamma_r)$ where

$$w_r = (u_r^2 + v_r^2)^{\frac{1}{2}} = \text{the normalized radial aiming error}$$

$$\gamma_r = \tan^{-1}\frac{v_r}{u_r}.$$

The expression (3.15) may then be somewhat simplified, for $Q_s(u_s, v_s)$ will become a function of the single variable $(\lambda_s w_s)$ where

and σ_{gr} is the standard deviation of the gun dispersion.

The operator

$$\left[(1-A_{mr})\frac{\partial^2}{\partial u_m \partial u_r} + (1-B_{mr})\frac{\partial^2}{\partial v_m \partial v_r}\right]$$

then becomes

$$\left[1-\rho(|m-r|\tau)\right]\cos(\gamma_r - \gamma_m)\frac{d}{dw_m}\frac{d}{dw_r}$$

and is to operate, according to (3.14), under the conditions implied by the operator E_0, so that

$$\gamma_r = \gamma_m \qquad \text{and} \qquad w_r = w_m$$

giving further simplification to

$$\left[1-\rho(|m-r|\tau)\right]\left(\frac{d}{dw}\right)_m\left(\frac{d}{dw}\right)_r$$

Expression (3.15) therefore takes the form

$$\Phi_M = 2\int_0 w dw e^{-w^2 - \frac{1}{4}\sum_{mr}\left[1-\rho(|m-r|\tau)\right]\left(\frac{d}{dw}\right)_m\left(\frac{d}{dw}\right)_r} \prod_{s=1}^{M} Q_s(\lambda_s w)$$

$$= 2\int_0^{\infty} w dw e^{-w^2 - \frac{1}{4}\sum_{mr}\lambda_m\lambda_r\left[1-\rho(|m-r|\tau)\right]D_m D_r} \prod_{s=1}^{M} Q_s(\lambda_s w) \quad . \quad . \quad . \quad . \quad (3.16)$$

where D_m, for instance, operates only on Q_m, turning it into Q_m', without regard to the argument of the function.

We have obtained no more satisfactory method of interpreting the operators in the exponent of (3.16) than by a McLaurin series expansion. Writing

$$g_{mr} = \tfrac{1}{4}\lambda_m\lambda_r\{1 - \rho(|m-r|\tau)\}$$

and noting that $\rho(t)$, and therefore g_{mr}, are even functions of time, then

$$g_{mr} = \tfrac{1}{2}\tau^2\ddot{g}_{mr}(0) + \tfrac{1}{24}\tau^4 g^{iv}_{mr}(0) + O(\tau^6)$$

where

$$\ddot{g}_{mr}(0) = -\tfrac{1}{4}\lambda_m\lambda_r(r-m)^2\ddot{\rho}(0)$$

$$g^{iv}_{mr}(0) = -\tfrac{1}{4}\lambda_m\lambda_r(r-m)^4\rho^{iv}(0)$$

Hence

$$\exp\Big[-\sum_{mr} g_{mr}D_mD_r\Big] = 1 - \tfrac{1}{2}\tau^2\sum_{mr}\ddot{g}_{mr}(0)D_mD_r - \tfrac{1}{24}\tau^4\sum_{mr}\overset{\text{iv}}{g}_{mr}(0)D_mD_r$$
$$+\tfrac{1}{8}\tau^4\Big(\sum_{mr}\ddot{g}_{mr}(0)D_mD_r\Big)^2 + O(\tau^6). \quad . \quad (3.17)$$

Let $q_s(\lambda w) = \log Q_s(\lambda_s w)$, and denote $D_s(\log Q_s)$ by $q_s'(\lambda_s w)$, $D_s^2(\log Q_s)$ by $q_s''(\lambda_s w)$; then

$$\sum_{mr}\ddot{g}_{mr}(0)D_mD_r \cdot \prod_{s=1}^{M} Q_s(\lambda_s w) = \prod_{s=1}^{M} Q_s(\lambda_s w)\Big[\sum_{mr}\ddot{g}^2{}_{mr}(0)q_m'(\lambda_m w)q_r'(\lambda_r w)\Big] \quad . \quad . \quad (3.18)$$

$$\sum_{mr}\overset{\text{iv}}{g}_{mr}(0)D_mD_r\prod_{s=1}^{M} Q_s(\lambda_s w) = \prod_{s=1}^{M} Q_s(\lambda_s w)\Big[\sum_{mr}\overset{\text{iv}}{g}_{mr}(0)q_m'(\lambda_m w)q_r'(\lambda_r w)\Big] \quad . \quad . \quad (3.19)$$

$$\Big[\sum_{mr}\ddot{g}_{mr}(0)D_mD_r\Big]^2\prod_{s=1}^{M} Q_s(\lambda_s w) = \prod_{s=1}^{M} Q_s(\lambda_s w)\Big[2\sum_{mr} g_{mr}(0)q_m''(\lambda_m w)q_r''(\lambda_r w)$$
$$+4\sum_{mnr}\ddot{g}_{mn}(0)g_{mr}(0)q_m''(\lambda_m w)q_n'(\lambda_n w)q_r'(\lambda_r w) + \Big\{\sum_{mr}\dot{g}_{mr}(0)q_m'(\lambda_m w)q_r'(\lambda_r w)\Big\}^2\Big]. \quad . \quad (3.20)$$

Substituting (3.17), (3.18), (3.19) and (3.20) into (3.16), and replacing the summations by integrations, and assuming that λ_r, σ_{ar} and σ_{gr} are all constant in time and equal to the values λ, σ_a and σ_g, then the survival chance against a burst of fire of duration T is obtained in the form

$$\Phi(T) = 2\int_0^\infty w\,dw\,e^{-w^2+\nu\int_0^T q(\lambda w,t)dt}\Big[1+\frac{1}{8}\nu^2\lambda^2\ddot{\rho}(0)\int_0^T\int_0^T (t_1-t_2)^2q'(\lambda w,t_1)q'(\lambda w,t_2)\,dt_1dt_2$$
$$+\frac{1}{64}\nu^2\lambda^4\ddot{\rho}^2(0)\int_0^T\int_0^T (t_1-t_2)^4q''(\lambda w,t_1)q''(\lambda w,t_2)dt_1dt_2$$
$$+\frac{1}{32}\nu^3\lambda^4\rho^2(0)\int_0^T\int_0^T\int_0^T (t_1-t_2)^2(t_1-t_3)^2q''(\lambda w,t_1)q'(\lambda w,t_2)q'(\lambda w,t_3)dt_1dt_2dt_3$$
$$+\frac{1}{128}\nu^4\lambda^4\rho^2(0)\Big[\int_0^T\int_0^T (t_1-t_2)^2q'(\lambda w,t_1)q'(\lambda w,t_2)dt_1dt_2\Big]^2$$
$$+\frac{1}{96}\nu^2\lambda^2\overset{\text{iv}}{\rho}(0)\int_0^T\int_0^T (t_1-t_2)^4q'(\lambda w,t_1)q'(\lambda w,t_2)dt_1dt_2\Big] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.21)$$

where ν is the rate of fire, so that

$$M = \nu T \qquad \text{and} \qquad \nu = \frac{1}{\tau}$$

It should be remarked that any small discrepancy which may exist between the results of integration and summation serves the interests of realism by making allowance for irregularity in the time intervals between successive rounds.

If the target be circular, of angular radius R, then

$$Q(\lambda w,t) = 2\int_{R(t)}^\infty e^{-x^2-\lambda^2w^2}I_0(2x\lambda w)x\,dx \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.22)$$

where I_0 is the Bessel function of imaginary argument of order zero, but a highly accurate approximation to (3.22) in the range $\frac{R}{\sigma_g} < 1$ is provided by

$$Q(\lambda w,t) = \exp\Big[-\frac{R^2}{\sigma_g^2}e^{-\lambda^2w^2}\Big] = \exp\Big[-ze^{-r^2}\Big] \quad . \quad . \quad . \quad . \quad . \quad (3.23)$$

where

$$r = \lambda w \qquad \text{and} \qquad z = \frac{\Omega}{\pi\sigma_g^2}$$

Ω being the solid angle subtended by the target at the gun. This angle, and therefore z, will vary with the firing range and therefore with time. We shall write

$$Z = \nu\int_0^T z(t)dt$$

Adopting (3.23), it follows that

$$q(\lambda w,t) = -z(t)e^{-r^2}$$
$$q'(\lambda w,t) = 2z(t)re^{-r^2}$$

and

$$q''(\lambda w,t) = 2z(t)(1-2r^2)e^{-r^2}$$

so that (3.21) becomes

$$\begin{aligned}\Phi(T) = \frac{2}{\lambda^2}\int_0^\infty rdre^{-\frac{r^2}{\lambda^2}-Ze^{-r^2}}\Big[1 &+ \tfrac{1}{2}\ddot{\rho}(0)\lambda^2\nu^2r^2e^{-2r^2}\int_0^T\int_0^T(t_1-t_2)^2z(t_1)z(t_2)dt_1dt_2\\ &+ \tfrac{1}{16}\nu^2\lambda^4\ddot{\rho}^2(0)(1-2r^2)^2e^{-2r^2}\int_0^T\int_0^T(t_1-t_2)^4z(t_1)z(t_2)dt_1dt_2\\ &+ \tfrac{1}{4}\nu^3\lambda^4\ddot{\rho}^2(0)(1-2r^2)r^2e^{-3r^2}\int_0^T\int_0^T\int_0^T(t_1-t_2)^2(t_1-t_3)^2z(t_1)z(t_2)z(t_3)dt_1dt_2dt_3\\ &+ \tfrac{1}{8}\nu^4\lambda^4\ddot{\rho}^2(0)r^4e^{-4r^2}\Big[\int_0^T\int_0^T(t_1-t_2)^2z(t_1)z(t_2)dt_1dt_2\Big]^2\\ &+ \tfrac{1}{24}\nu^2\lambda^2\overset{\text{iv}}{\rho}(0)r^2e^{-2r^2}\int_0^T\int_0^T(t_1-t_2)^4z(t_1)z(t_2)dt_1dt_2\Big] \quad (3.24)\end{aligned}$$

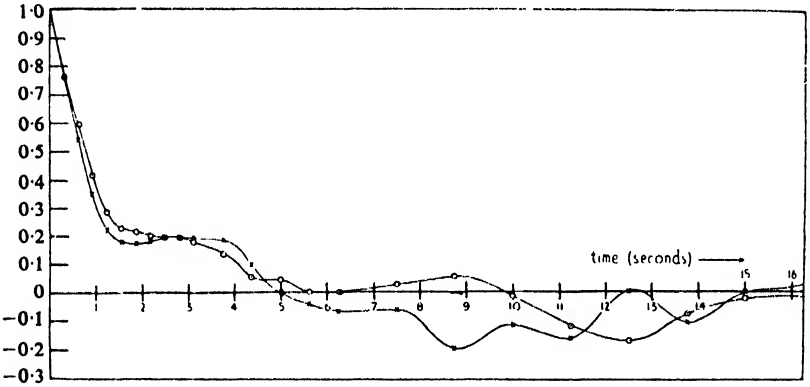

FIG. 9.

The internal integrals in this result, with respect to time, may clearly be determined as soon as the change of air-range with time has been prescribed. As these integrals do not involve r, they behave thereafter like constants. The integrals with respect to r are all of the form

$$I(m,n) = \int_0^\infty r^{2m+1}e^{-\left(\frac{1}{\lambda^2}+n\right)r^2 - Ze^{-r^2}}dr$$

where m and n are integers.

It may be shown that

$$I(m,n) = -\left[\frac{\partial}{\partial\left(\frac{1}{\lambda^2}\right)}\right]^m\left[Z^{-\frac{1}{\lambda^2}-n}\gamma\left(\frac{1}{\lambda^2}+n,Z\right)\right] \quad . \quad . \quad . \quad . \quad . \quad (3.25)$$

where $\gamma\ (x,y)$ is the Incomplete Gamma Function.

In the particular case when the air-range is kept constant, equation (3.24) integrates to the form

$$\Phi(T) = C_0(\lambda,Z) + C_1(\lambda,Z)\ddot{\rho}(0)T^2 + \frac{1}{30}C_1(\lambda,Z)\overset{\text{iv}}{\rho}(0)T^4 + C_2(\lambda,Z)\ddot{\rho}^2(0)T^4 + \text{higher terms} \quad (3.26)$$

where $C_0(\lambda,Z)$, $C_1(\lambda,Z)$ and $C_2(\lambda,Z)$ are expressible in terms of the $I(m,n)$ functions and have been tabulated.

Examples of typical gunnery autocorrelation functions are given in Fig. 9, where experimental

values of $\rho(t)$ are graphed against time: one of the two curves has been obtained from the values of the elevation component of the aiming error when a certain modern method is in use for controlling the tracking of the guns in a bomber turret, the error being recorded by a gyro cine-assessor: the other curve is based on the corresponding data for the traverse component.

The equation (3.26), and similar equations involving variations in air-range, have been used as a basis for a variety of calculations. As an example, Fig. 10 shows the variation of the lethality, measured by $-\log \Phi$, with the aiming and gun dispersions, when a one-second burst of fire is directed from a single cannon against a target of area 5 square feet at a range of 200 yards, the rate of fire and autocorrelation function being typical for such conditions: the values of $-\log \Phi$ are plotted against σ_g, and each curve corresponds to a particular value of σ_a. The graph indicates that if the aim-wander parameters remain unchanged, then it may be beneficial, sometimes markedly, to *increase* the dispersion of the bullets by increasing the gun dispersion, for by so doing the undesirable correlation between the bullets is reduced.

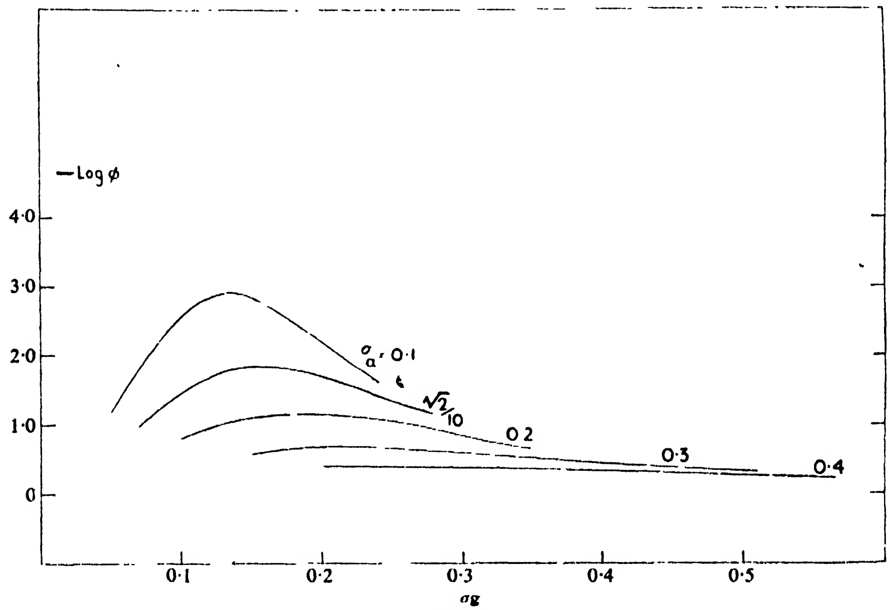

FIG. 10.

Machine used to Compute Autocorrelation Functions

In the various studies which we have undertaken, it has been necessary to evaluate an enormous number of covariances, and, as mentioned earlier, the labour entailed has been tremendously reduced by a special relay computer. For the initial development of a machine of this type, statisticians are indebted to Shire and Runcorn, who built a small model using punched tapes, relays and uniselectors, so that they could obtain autocorrelation functions from data which had been grouped to integer values in the range -24 to $+24$. When the capabilities of this prototype were appreciated, a larger model was designed and constructed to our requirements by Weir and Barnes, at the Telecommunications Research Establishment of the Ministry of Aircraft Production, the range being increased from ± 24 to ± 63, the uniselectors being eliminated so that only ordinary relays are used in the mechanism, thus achieving a more robust design and greater ease in maintenance, and various other refinements in detail being introduced.

The resulting machine is constructed from standard G.P.O. equipment, and comprises six main parts—a keyboard perforator, two transmitters, a lamp display, control box and a computer rack—all illustrated in Fig. 11. Experimental data, in the form of sequences of numbers, are recorded in code as perforations on each of two tapes, which are then read automatically by each

of two transmitters. The machine then carries out the basic process of multiplying corresponding pairs of numbers from the two tapes and of summing the products, the final total being shown on the lamp display, and remaining there until cancelled by the operator.

Originally, the numbers were typed in code on the parchment paper tape used in G.P.O. transmitters, but it was found that a tougher material was required to stand up to constant usage, so that now all tapes are made of Textuff, a cellulose-impregnated cotton. The typing is done by a keyboard perforator (see Fig. 12), which, for each operation of a key, can punch up to five holes across the tape. Each number is always coded by operating two keys—one for the tens of the number, and one for the sign and units of the number. The first key is selected from the first row of the perforator, on which the keys are numbered 0, 10, 20 . . . 60; the second key is selected from the second row if the number is positive, and from the third row if negative—both these rows being marked 0, 1, 2 . . . 9. Thus + 29 is obtained by operating the keys 20 and + 9; − 15 by keys 10 and − 5; + 3 by keys 0 (in tens row) and + 3. The five holes, which are used in coding tens, represent the values 2, 4, 8, 16 and 32, in that order across the tape, so that the 20 key causes the punching of holes 4 and 16. In the case of the units, the first four holes have values 1, 2, 4 and 8, while the last hole gives the sign, being punched if positive, so that + 9 is coded by holes in the first, fourth and fifth positions across the tape. Zero must always be punched positively, since a negative zero gives no perforation and would be read by the transmitter as the end of the data on the tape, automatically stopping the computer. Besides the coding holes, the perforator punches guide holes for the tape-feed mechanism.

The coding system described above does not make maximum use of the two rows of five holes each. Coding in powers of 2, and allowing one hole for the sign, it should be possible to code all numbers in the range ± 511, but the system adopted permits a very simple conversion from base 10 to base 2, together with a simple keyboard perforator. Even with the present coding, numbers not exceeding ± 77 could be punched on the tape, but the relays can only accept those between ± 63. Conversion to the scale of two makes the data peculiarly well adapted for relay computation, for then only two digits—0 or 1—are possible, and these may be made to correspond to the only possible states—open or closed—of a relay. Other relay methods of computation, not based on the scale of two, are generally less simple and less economical.

The system of coding is illustrated in the following diagram.

(*a*) Positive Units

1	2	4	guide	8	sign	
			•		o	+0
o			•		o	+1 = 1
	o		•		o	+2 = 2
o	o		•		o	+3 = 1 + 2
		o	•		o	+4 = 4
o		o	•		o	+5 = 1 + 4
	o	o	•		o	+6 = 2 + 4
o	o	o	•		o	+7 = 1 + 2 + 4
			•	o	o	+8 = 8
o			•	o	o	+9 = 1 + 8

(*b*) Negative Units

1	2	4	guide	8	sign	
			•			0
o			•			−1
	o		•			2
o	o		•			−3
		o	•			−4
o		o	•			−5
	o	o	•			−6
o	o	o	•			−7
			•	o		−8
o			•	o		−9

(*c*) Tens

2	4	8	guide	16	32	
o		o	•			10 = 2 + 8
	o		•	o		20 = 4 + 16
o	o	o	•	o		30 = 2 + 4 + 8 + 16
		o	•		o	40 = 8 + 32
o			•	o	o	50 = 2 + 16 + 32
	o	o	•	o	o	60 = 4 + 8 + 16 + 32

(*d*) Typical Combinations

+29		o		•	o		20 tens
	o			•	o	o	+ 9 units
−15	o		o	•			10 tens
	o		o	•			− 5 units
+ 3				•			0 tens
	o	o		•		o	+ 3 units
+47			o	•		o	40 tens
	o	o	o	•		o	+ 7 units
+ 0				•			0 tens
				•		o	+ 0 units

The perforated tapes are fed through two transmitters, *X* and *N*, from right to left, one row (1/10 in.) at a time, by a small star wheel which engages the guide holes (see Fig. 13). Originally designed for use on the Multiplex Telegraph System, these transmitters set up, on five wires, vol-

tages corresponding to the various combinations of holes perforated in the tape, by means of five selecting rods, mechanically connected to five tongues. The tongues move between two contacts, so that when the pins are at the limit of their upward thrust, the tongues rest against the "make" contacts. Thus when a tape is passing through the transmitter, only pins opposite a coding hole can complete their upward travel, enabling their tongues to make contact, and those pins arrested in their motion by the tape maintain their tongues in the break position.

The multiplication of each pair of numbers and addition of the product to the total takes about 1 second. If the two coded series of data on the tapes be denoted by $x_1, x_2, \ldots x_K, \ldots$ and $y_1, y_2, \ldots y_K, \ldots$, and if $\sum_K x_K y_K$ is required, then the tapes should be set in the transmitters so that when the first transmitter reads x_K from one tape, the second will simultaneously read y_K from the other. When the electrical computer has accepted these two numbers, the tapes automatically proceed, and while x_{K+1} and y_{K+1} are being read off, x_K and y_K are multiplied together, and added to the sum of products (which is always zero at the beginning of the operation). This sequence continues until the end of the tape is reached, or until the operator stops the process. In particular, when autocorrelation coefficients are being determined, then $y_K = x_{K+N}$, where N may be given any desired value: the x and y tapes are then actually identical, but as they are passed through the transmitters, one is displaced by N values relative to the other.

Fig. 14 shows the lamp display—a series of powers of 2, from which the required product is obtained by totalling the illuminated values. In the same photograph appears the control-box, used in all normal operations. The " Start " key, on it, closes both the transmitter and multiplier circuits, and when returned to normal resets the whole equipment, cancelling the total and all storage except that on the display relays. The " Stop " key enables the operator to stop the transmitters while still retaining the total and storage—this is used to examine the total at an intermediate point on the tape, if required. On this installation the " Cancel Total " key has been blocked up, since it served no purpose that could not be performed equally well by the other keys. Its function is to enable the final product to exceed $\pm$ 524,287, by operating the " Stop " key before it is reached, displaying the total, recording it, and then cancelling everything with the " Cancel Total "—obviously just as easily done by restoring the " Start " key to its original position. The amount so extracted is added to the final answer. The " Display " key in the " Cancel " position clears the display relays, and cuts off the lamps; in the " Change " position operates the display relays to correspond with the latest total, and in the upright position retains the reading of the last key shift.

Of the four lamps shown on the control box, " Excess Input " and " Excess Storage " need no explanation. " No Storage " is a signal that the multiplying relays have exhausted all the tens and units stored in the addition relays, lighting intermittently in normal sequence, and continuously when the end of the tape is reached, or if the tape sticks owing to worn feed holes. The " Test " lamp operates when either the " Test " key, " Change-over " keys or " Display " key in the Test Panel are being used, and warns the operator that the machine is not available for normal use. A counter on the control-box records the number of products which have been evaluated and added together.

The Test Panel is illustrated in Fig. 15, and its various keys ought never to be used unless the " Test " key is in operation. On it, two " Change-over " keys, C.O., will cut out the data from the transmitters X and N, substituting data from the " Numerical Input " keys, designated " X input " and " N input."

It is fairly easy to determine whether a fault lies in the transmitter or computer racks by replacing the data from the transmitters by that from the " Numerical Input " keys. In addition to the use of the Test Panel as a fault-finding device, the " Input " keys perform a useful function in finding the sum of a series of data. By using the $+1$ key on the X-input and inserting the tape containing the series in the N transmitter, each integer will be added to the next, and the total shown on the display board.

The " Test " key also brings into operation two keys starting the transmitting and multiplying circuits independently, a " Display " key corresponding to that on the operator's box, a " Reset " key, and an " Impulse Test " key. The " Impulse Test " key allows manual control of the relays by the rotary switch (top right of photograph) which gives two impulse cycles per revolution.

Fig. 16 shows the computer rack with relays uncovered. The letters on these relays give some indication of their function—thus in *AUN*, the first of the relays, *UN* denotes that *units* are stored here from the N transmitter or input, while the first letter gives the numerical value, in this case *A*

corresponding to $2^0 = 1$. Similarly *B* corresponds to 2^1, *C* to 2^2, *D* to 2^3, and so on. In the same way, in *BTN*, *TN* denotes that *tens* are stored here from the *N* transmitter or input, and the numerical value is again indicated by the first letter *B*, corresponding to 2^1. When *NTF* operates, the contents of the two rows of relays are added together, and the sum appears in the *SN* group (*ASN*, *BSN* . . . *FSN*). The sign of the data is taken by relay *SGN*. A similar set of relays carries the data from the *X*-transmitter. For example, the number + 37 will be fed into the *N* relays as 30 stored in the tens relays *ETN*, *DTN*, *CTN* and *BTN*, and + 7 stored in the units relays, *CUN*, *BUN*, *AUN* and *SGN*. When *NFT* operates, the two will add as follows :—

f	*e*	*d*	*c*	*b*	*a*	
	1	1	1	1	0	Tens
			1	1	1	Units
1	0	0	1	0	1	Total

in accordance with the addition laws in radix 2:—

$$
\begin{aligned}
0 + 0 &= 0 \\
0 + 1 &= 1 \\
1 + 1 &= 10 \\
1 + 1 + 1 &= 11
\end{aligned}
$$

so that + 37 is stored in the " *S* " relays, *FSN*, *CSN*, *ASN* and *SGN*.

This demonstrates very well the simplicity offered by this system of converting numbers from base 10 to base 2. A sacrifice has, of course, been entailed in that maximum use is not made of the ten coding holes, which could have provided a range of ± 511, as mentioned earlier.

The numbers so introduced into the *SX* and *SN* relays are now to be multiplied together. The sign of their product is first determined by relay *SGS*, which is open if *SGN* and *SGX* are both open or both closed, and closed if these two are in different states. Then, if *ASX* be closed, *APP*, the first of the " Partial Product Selection " relays (*APP*, *BPP* . . . *FPP*), is also automatically operated, and the latter then closes those of *AB*, *BB* . . . *LB*, which correspond to relays closed in the *SN* group. Thus if *ASN*, *BSN* and *DSN* are closed, the *AB*, *BB* and *DB* will close.

Up to this stage the previous total of products has been retained in the "*A* " relays (*AA*, *BA*, . . . *TA*), and the sum or difference (according as *SGS* is open or closed) of the data in the "*A* " and " *B* " groups is now built up in the " *C* " set (*AC*, *BC*, . . . *TC*): this new total is then transferred back to the "*A* " group, the " *B* " relays are cleared, *APP* and *ASX* are released, and the next *SX* relay closes its corresponding *PP* relay. If this be *BPP*, then a similar sequence is carried out, except that this selection relay does not cause the closure of those " *B* " relays in exact correspondence with the *SN* set, but those " *B* " relays which are one power higher in the binary scale. In other words, data in the *SN* group, when passed by *BPP* into the " *B* " relays, are staggered one place. Similarly, each of the other *PP* relays, closed in turn by the *SX* group, selects an appropriate amount of stagger, and the product of the number in the *SN* and *SX* sets is built up and added to the "*A* " relays, according to the following scheme :—

k	*j*	*h*	*g*	*f*	*e*	*d*	*c*	*b*	*a*	
				1	0	1	1	1	1	47 in *SN* group.
						1	0	1	1	11 in *SX* group.
				1	0	1	1	1	1	— no stagger, first partial product selected by *APP*.
			1	0	1	1	1	1		— one place stagger, second partial product selected by *BPP*.
		0	0	0	0	0	0			*CPP* not operated.
	1	0	1	1	1	1				— three place stagger, third partial product selected by *DPP*.
1	0	0	0	0	0	0	1	0	1	Total contribution to sum of products, equal to 517.

Except at those intermediate stages in the multiplication which involve the adding of a partial product to the previous total, the data contained in the "*A* " and " *C* " relay sets are equal to one another and to the most recent sum of products. By operation of the " Change Display " key, this latest total can be transferred from the " *C* " set to the " *D* " relays, and on the display board these lamps light which correspond to the digits whose " *D* " relays are operated.

During constant usage, the machine has shown itself to be extremely reliable. Although slower than an electronic machine, it has the advantage of being much less expensive and much more robustly constructed.

The evaluation of correlation coefficients is not the only computation which may be readily performed by this relay computer: it will also give an accurate estimate of the value of

$$\int^{b} f(x)g(x)dx$$

if numbers be perforated on the two tapes which are proportional respectively to the values of the functions $f(x)$ and $g(x)$ at close and regular intervals of their argument. The determination of the spectrum of a random variable as the Fourier cosine transform of its correlogram is therefore particularly easy—the correlogram is represented on one tape, while a series of tapes is maintained permanently available which represent cosine functions of various frequencies: thanks to the periodicity of the cosine functions, these latter tapes may be closed endless bands. To test the efficiency of the machine in this application, the function

$$e^{-\lambda|t|} \cos \mu t; \quad \lambda = 0{\cdot}00876; \quad \mu = 0{\cdot}022848$$

was represented by a tape of 70 consecutive values, and the cosine transform of the function then estimated. The values so obtained are shown in the following table, together with values of the theoretical result

$$S(\omega) = \frac{2}{\pi}\int_0^\infty e^{-\lambda t} \cos \mu t \cos \omega t dt = \frac{2}{\pi} \frac{\lambda(\lambda^2 + \mu^2 + \omega^2)}{(\lambda^2 - \mu^2 + \omega^2)^2 + 4\lambda^2\mu^2}.$$

ω	0·039270	0·034148	0·025335	0·023800	0·023100	0·022440	0·020668
$S(\omega)$ estimated	8·841	14·205	34·912	37·128	37·495	37·544	35·871
$S(\omega)$ theoretical	8·758	14·479	34·788	37·150	37·581	37·569	35·634

ω	0·017453	0·015708	0·013090	0·006545	0·003272	0·000000
$S(\omega)$ estimated	28·372	23·838	18·179	11·369	9·866	9·337
$S(\omega)$ theoretical	27·985	23·616	18·254	11·105	9·736	9·314

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Discussion on the Papers

Mr. M. G. Kendall: I move a vote of thanks to the authors of the three papers with great pleasure. One of the primary objects of re-instituting this Section was to provide an opportunity for workers in various fields to compare notes, and the papers we have had to-night form an ideal illustration of the value of such an opportunity. I should like to congratulate the four contributors and the Section on these papers. I should also like to add a word of congratulation to myself, because I claim the distinction of having worked out more of these serial correlations by hand than any other living worker, and I am glad to hear that that period of hard labour is now at an end. I hope that the machines about which we have heard may be made generally available for private workers. It is beyond the scope of any individual or the ordinary institution to acquire them.

On the machines themselves I have only one question to ask, and that relates to the optical device described by Mr. Foster. It is necessary in some classes of work to work out serial correlations with some accuracy to an order as high, perhaps, as the 50th, for a series which may run into a thousand terms; and I am not sure how far such series can be photographically reproduced accurately enough to enable calculations of such an order to be made. In other words, what is the instrumental error as compared with the sampling error?

I have one comment to make on Mr. Foster's paper. I was gratified to find that he had confirmed, quite independently, the conclusions to which I came in dealing with economic series. He says that his conclusions are not quite so drastic as mine, but I do not think there is much between us. I found in an economic series that periodogram analysis showed a large number of periods for consideration, and concluded that the analysis is extremely misleading. Mr. Foster

would agree that if one tries to identify a separate period with each of the trial periods thrown up, they are not identifiable; and says that if there are a number of periods of that kind, it is indicative of some one disturbed period in the series. I do not differ from that, except that I would like the point settled by some experiments as follows: if someone would put through a periodogram analysis a few random series of moderate length, say 200 or 300 terms, and show what sort of periodogram was obtained from that result, it would be extremely interesting. I should not be surprised if there were quite a number of periods thrown up as significant in the ordinary meaning of the term. If not, then I think Mr. Foster is quite right, and that the existence of a number of these periods probably does indicate one single disturbed oscillation of the series.

On the paper of Dr. Cunningham and Mr. Hynd I have one comment. I wonder if their technique is applicable to naval gunnery. When a ship is rolling there is some sort of approximate harmonic motion imposed on the range-finding apparatus, and it would be interesting to see whether there is any harmonic difference shown up in the correlogram, or whether there is the same kind of correlogram for guns which are more firmly fixed.

I have left Dr. Bartlett to the last because I have more to say with regard to his paper. First of all, there are two comments on the question of standard errors. Firstly, the standard errors which he gives depend not on the observed correlations, but on the parent correlations. If the observed correlations are 100 times too large, as they may very well be, then the standard error given by the formula may be quite wide of the mark. It may be two or three times the true value. Dr. Bartlett has foreseen that difficulty, and suggests using an autoregressive scheme and fitting the first two serials which are more likely to be near the mark. Secondly, in most work that I have undertaken on correlogram analysis one is interested, not in the significance of any particular coefficient, but in the character of the series as a whole. In the correlograms in the paper the question is not whether the actual values are too large or too small, but whether there is any significance in the undulations as a whole. The indications are, as far as one can judge from experiment, that when definite sinusoidal movements appear in the correlogram they arise from some property of the series, and not from the nature of the analytical processes: but one would like some test on the point.

I should like to have said a good deal about the continuous random process, because I think there is some danger here of the mathematician running away with us to a slight extent. I have never been able to imagine any sort of continuous randomness; it seems to me that it is essentially an idea of discontinuity. It may be that the discontinuity in the sort of series we observe is in the first or second order differentials, that it is the impulses and accelerations which change discontinuously, not the actual values of the series themselves. But I think that in economic series one must provide, even if the series itself is continuous in the sense that it proceeds through time, for random disturbances imposed on the series from outside. Yule has said, as I have, that the type of series we employ is not by any means the last word on the subject. It is admittedly only an approximation, and one must at some stage try to find a scheme which does provide for this more continuous process. To some extent I would have expected the generalization to proceed not so much from a consideration of the continuous random process as from that of shocks of finite extent which do not occur at regular intervals of time, but at irregular intervals, the randomness lying in the time intervals between the successive disturbances.

Perhaps I can add one remark on that. The scheme introduced by Yule has the advantage that it permits of the construction of experimental series. I cannot think of any method of constructing a continuous random series, but one of the difficulties is to make sure, when analysing a series, that one knows its properties beforehand. In the Yule scheme one knows what one should find, and if it is not there, one can conclude that the methods of analysis are wrong.

Personally, I feel that this work which we have had put before us to-night has taken the subject a great deal further. We obviously still have a long way to go, but the signs are hopeful; and I move the vote of thanks with great pleasure.

Dr. H. E. Daniels: I am particularly pleased to have been asked to second the vote of thanks this evening. It was my privilege to work with Dr. Cunningham and Mr. Hynd for over three years and watch the development not only of the fruitful methods described in their paper to-night, but of other fundamental research initiated by Dr. Cunningham at the Air Ministry which I hope we shall hear about in the future. Mr. Foster's paper is also of special interest to me, as his pioneering work on cotton-spinning has been known to us at the Wool Industries Research Association for many years, and it is good to know that his results and the ingenious machines he has devised for periodic analysis are at last being made available to workers in other fields.

The problem of the target survival chance discussed in the latter part of Dr. Cunningham and Mr. Hynd's paper is an interesting and fundamental one. It was outlined in a simpler form (without gun dispersion) by Prof. Pearson in the discussion on Mr. Kendall's paper to the Society last year, and Mr. Foster hints at it in a very different context in his reference to the distribution of yarn strength, which is determined by the behaviour of the weakest places in given lengths of yarn along which the strength is autocorrelated.

The approach by Cunningham and Hynd is very general, granted the assumption of normal

residuals, as it places no restriction on the behaviour of the autocorrelations except that they should not be too far from unity, a condition which is usually satisfied in practice in their problem of short bursts. But in the case of yarn strength, and possibly in certain other gunnery problems too, the intervals may be long enough for the condition of high correlation not to be met, and the Cunningham–Hynd series expansion is then not very tractable.

In such cases an alternative approach might be tried along the lines of recent work on the theory of Brownian motion, if one is prepared to assume that the time series is generated by a simple autoregressive process. When there is no superposed error one has to solve the appropriate Fokker–Planck equation with a reflecting boundary, a familiar though not yet completely solved problem in that subject. The theory can also be extended to include superposed random error; for example, in the simple case of a one-dimensional Markoff process,

$$\dot{x} + \mu x = I_t$$

if the survival chance in an interval dt is $1 - \psi(x,t)dt$, the Fokker–Planck equation extends to

$$\frac{\partial P}{\partial t} + \psi(x,t)P = \mu\frac{\partial}{\partial x}(xP) + \tfrac{1}{2}\sigma^2\frac{\partial^2 P}{\partial x^2}$$

where $\sigma^2 dt$ is the variance of the increment of x in dt, and the required total survival chance after time t is $\int_{\infty}^{\infty} P dx$. A similar formula in radial co-ordinates would apply to the gunnery problem. I have not been able to solve explicitly even this simple form of the equation, let alone the more complicated equations for higher-order processes, but numerical solution by relaxation methods might provide useful information in the region of moderate correlations not adequately dealt with by the Cunningham–Hynd series expansion.

We are fortunate in having three new machines described to us for the calculation of serial correlations. For data provided in numerical form, the relay computor is in my opinion undoubtedly the best of the three, but when the data are presented in the form of continuous records on a chart, the machines described by Mr. Foster seem more convenient, especially for textile work where high accuracy is not demanded. The Martindale optical instrument is the most rapid once the transparencies are prepared, but until a machine is available to perform automatically this at present rather messy operation, I prefer the integrating wheel. As it is relatively inexpensive to construct, would it not be a worth while improvement to arrange two integrating wheels to calculate $S_1 = \Sigma(x_1 + x_2)^2$ and $S_2 = \Sigma(x_1 - x_2)^2$ simultaneously? The intra-class correlation $r = \frac{S_1 - S_2}{S_1 + S_2}$ could then be conveniently obtained from each "run through." If the practice is adopted of calculating correlations by dividing each covariance by a single variance computed from the whole of the original data, one is occasionally embarrassed by correlations which apparently exceed unity.

Mr. Foster's researches on irregularity in cotton-spinning inspired us at the Wool Industries Research Association to try out similar investigations on wool. There had been an impression that wool was "better behaved" in spinning than cotton, on account of its longer fibres, but our correlograms turned out to be depressingly similar to Mr. Foster's, with periods even more in evidence, and Spencer-Smith's parallel work on linen tells much the same story. This is not really surprising, since, as Mr. Foster will no doubt agree, the important factor affecting the amplitude of the drafting wave is not so much the average length of the fibres as their relative variability.

I should like to ask Mr. Foster whether he has found any evidence in cotton series of what might be called a directional effect. From the theory of drafting outlined in his paper, one might expect the thickness to increase relatively slowly up to a point, and then to diminish rapidly as the tuft is pulled through the front rollers, producing a kind of saw-toothed appearance in the thickness curve. That such an effect may exist in wool is suggested by a tendency to skewness in the form of the frequency distribution of first differences, though the evidence is admittedly inconclusive in the absence of a suitable test of significance. It is perhaps worth observing that a directional effect of this nature is produced in an autoregressive series when the distribution of the residuals is skew.

I have left little time to comment on Dr. Bartlett's valuable paper, which has clarified for me much of the hitherto puzzling behaviour of autocorrelations in short series. In particular, his standard error and correlation formulae for sample autocorrelations, crude as he admits them to be, are of considerable practical assistance in the interpretation of sample correlograms. There remains, of course, the question as to how far one is entitled to test individual autocorrelations for significance without making due allowance for selection, and perhaps Mr. Kendall had qualms about it when he suggested testing the correlogram as a whole for evidence of oscillatory movements. The fact that, as Dr. Bartlett points out, autocorrelations in short series are themselves highly correlated may minimize the importance of this, since there are effectively fewer inde-

pendent correlations to select from. To do the job completely would presumably involve the solution of a "survival chance" problem even more formidable than the Cunningham–Hynd one!

I have great pleasure in seconding the vote of thanks to the authors of these three stimulating papers.

THE CHAIRMAN then read a message from Mr. G. Udny Yule in which he expressed his regret at not being able to attend the meeting. He considered that the Research Section was maintaining a high standard with these three valuable papers, and wished to send his good wishes to the Section for a flourishing future.

DR. M. S. BARTLETT then read a summary of the following written contribution from PROFESSOR P. J. DANIELL, who expressed his regret at being prevented by illness from attending the meeting.

My absence from this symposium is a grief to me. The subject is very important and interesting, and I send the following notes.

The work done in America has been based on a fundamental study by N. Wiener of integrals in an infinite number of dimensions corresponding to the values of the fluctuating quantity at various instants. This work is not behind that of the Russian school in time or importance.

In erratic fluctuations there will be no sharply marked frequencies, and formula (17) of Bartlett's paper can be expressed in the form

$$\rho(s) = \frac{1}{\pi}\int_{\infty}^{\infty} \cos \omega s \, \phi(s\omega)d\omega \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where $\phi(\omega)$ is the spectral intensity.

If y is the resultant of a stationary time-series of chaotic instantaneous impulse functions (Dirac's δ function), the spectral intensity is constant. If x is the response to these impulses by means of a "linear mechanism," then in the usual operational notation

$$f(p)x = f(i\omega)x = y, \; i = \sqrt{(-1)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In this case $\phi(\omega)$ for x will be proportional to

$$[f(i\omega)]^{-2} = [f(p)f(-p)]^{-1}, \; p = i\omega.$$

The mechanism is presumed to be stable with decay and then the roots, α, of $f(p) = 0$ have only negative real parts.

By contour integration from (1) it appears that the autocorrelation ρ is a time-function which for positive s is such that its Laplace transform is proportional to

$$\Sigma \frac{1}{f'(\alpha)f(-\alpha)(p-\alpha)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

summed for all roots α of $f(p) = 0$.

This agrees with Bartlett's formula (31), but it shows not only the differential equation satisfied by $\rho(s)$, it also gives the particular solution needed. Moreover, in many cases, as with pure time-lag problems, $f(p)$ corresponds to no finite differential equation.

For convenience we standardize by multiplying by such a constant that $\rho(0) = 1$.

The Laplace transform of the impulse function is 1. If $1/f(p)$ is the transform of the displacement, x, caused in the mechanism by the impulse function at time 0, then the spectral intensity is inversely proportional to $[f(i\omega)]^2$ and $\rho(s)$ can be found from (3) or (1).

The Laplace transform point of view has the advantage of combining mechanisms in cascade by ordinary multiplication.

There are also extensive examples from mechanical and electrical problems.

Example 1. *Simple damping.*

$$f(p) = p + k$$
$$\phi(\omega) = \text{const.} \times (\omega^2 + k^2)^{-1}.$$

In equation (3) there is only one root, $-k$, and $\rho(s)$ is therefore proportional to the function whose transform is $1/(p + k)$.

Hence

$$\rho(s) = \exp. (-ks).$$

Example 2. *Electrical tuned circuit.*

Put
$$2k = R/L, \; \omega_1^2 = 1/(LC).$$
Then
$$f(p) = \text{const.} \times (p + 2k + \omega_1^2/p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

If

$$k = \omega_1 \cos\lambda,\ w_0 = w_1 \sin\lambda,$$

the roots of $f(p) = 0$ are at

$$\alpha = -w_1 \exp.(\pm i\lambda),$$

and for each of these $f(-\alpha) = 4k$. Hence the transform of $\rho(s)$ is inversely proportional to $f(p)$ and

$$\rho(s) = \exp.(-ks)\sin(\lambda - \omega_0 s)/\sin\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Formula (30) of Bartlett's paper arises similarly from taking

$$f(p) = p^2 + \alpha p + \beta.$$

In this case $f(-p)$ does not have the same value at the two zeroes of $f(p)$, so that $\rho(s)$ is not quite so simple.

Conversely, given $\rho(s)$, and therefore $\phi(\omega)$, it is possible to invent a mechanism—that is to say, an $f(p)$, which would yield $\rho(s)$ under certain mathematical conditions. One can refer to Titchmarsh * using

$$\log\phi(\omega) = \log f(i\omega) + \log f(-i\omega).$$

[*Note*.—In example 2, if the circuit is over-damped we put $k = \omega_1\cosh\lambda$, $\omega_0 = \omega_1\sinh\lambda$ and replace sin by sinh in equation (5).]

On the question of errors of estimates my contribution is slight in comparison with the work of Bartlett and Cunningham. My interest was aroused by a discussion as to whether in an empirical attack the autocorrelation or the frequency spectrum would be more subject to error. By elementary algebra it can be shown that the methods are equivalent both in results and in accuracy if the proper formula of translation is used.

Let $x(t)$ be known at N instants of time separated by intervals h so that

$$\text{total time } T = Nh.$$

Let

$$\omega = \frac{2\pi}{T} \times \text{integer from } -\tfrac{1}{2}(N-1) \text{ to } \tfrac{1}{2}(N-1),$$

assuming N to be odd. Then

$$\phi(\omega) = h\left[\tfrac{1}{2}v(0) + \sum_{r=1}^{N-1}\left(1 - \frac{r}{N}\right)v(rh)\cos\omega rh\right] \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where $\phi(\omega)$ is the proper estimate of spectral intensity and $v(\tau)$ is the estimated covariance of $x(t)$, $x(t+\tau)$. This formula is an algebraic identity for any empirical set. Taking an infinite population of such sets

$$\phi(\omega) = h\left[\tfrac{1}{2} + \sum_{r=1}^{N-1}\left(1 - \frac{r}{N}\right)\rho(rh)\cos\omega rh\right].$$

Errors in estimates of v and of ϕ will exactly correspond.

It happens, however, that the covariance of $v(r_1h)$, $v(r_2h)$ is of the same order as the variance of either, so that the variance of $\phi(\omega)$ is considerable, and does not decrease appreciably as N (or rather T) increases. Thus $\phi(\omega)$ appears to be more subject to error than $v(\tau)$. However, the covariance of $\phi(\omega_1)$, $\phi(\omega_2)$ is small, so that the variance of a mean $\phi(\omega)$ taken over a broad band of frequencies can be made as small as that of $v(\tau)$. This suggests that in some cases an alternative method of estimation would be to run the data in suitable form through a mechanism, including perhaps 10 or 20 vibrators damped to produce broad tuning and to determine the mean energy in each. If we knew the operator functions for these vibrating elements we could deduce the original spectral intensity and obtain $\rho(s)$ from the Fourier transform.

Since the problem concerns correlation between many (in fact an infinite number of) variables, it is best to follow the usual custom in theoretical studies of correlation which is to assume normal distributions of variables. The fundamental lemma is then that for variables y_1, y_2, y_3, y_4 the same or different,

$$\text{cov.}\,(y_1y_2,\ y_3y_4) = E(y_1y_3)E(y_2y_4) + E(y_1y_4)F(y_2y_3) \quad . \quad . \quad . \quad . \quad . \quad (7)$$

It follows that

$$\text{cov.}\,\{v(s),\ v(s+t)\} = \frac{h}{T-s}\Sigma\lambda(u)[\rho(u)\rho(u+t) + \rho(u-s)\rho(u+s+t)] \quad . \quad . \quad (8)$$

in which u is a multiple of h and $\lambda(u)$ equals 1 when $-t \leqslant u \leqslant 0$, $\lambda(u)$ equals 0 when $u \geqslant T-s-t$ and when $u \leqslant -(T-s)$, and in between $\lambda(u)$ varies linearly.

If T is sufficiently great so that $\rho(s)$ is small for values of s of the same order as T, then as an approximation we have Bartlett's formula (6) in the form

$$\text{cov.}\,\{v(s),\ v(s+t)\} = \frac{h}{T-s}\sum_{-\infty}^{+\infty}[\rho(u)\rho(u+t) + \rho(u-s)\rho(u+s+t)].$$

This he kindly attributes to me, but I imagine it is well known.

* E. C. Titchmarsh, *Fourier Integrals*, (1937), A131.

If h is small the summation can be replaced by integration, but in no case should the factor $T - s$ be replaced by T. This factor corresponds to the *a priori* deduction that relative errors of estimates must increase as s approaches T.

The expectation of $\nu(0)$ is 1, but it has as much variance as any $\nu(\tau)$.

On the subject of the variance of the estimated correlation, as distinct from covariance, I have nothing to add to the more profound work of Bartlett and Cunningham.

If we use the lemma (7) and assume h small enough to allow us to replace summations by integrations,

$$\text{var.}\,\{\phi(\omega)\} = C^2(\omega,T) + \frac{1}{\omega^2 T^2}S^2(\omega,T) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and if $\omega_1 - \omega_2 = 2\nu$ is fairly small, $\omega_1 + \omega_2 = 2\omega$

$$\text{cov.}\,\{\phi(\omega_1),\phi(\omega_2)\} = \frac{1}{\omega^2 T^2}[S^2(\omega,T) + (\omega dS/d\omega)^2] \quad . \quad . \quad . \quad . \quad . \quad (10)$$

In these formulae

$$\bar{\phi}(\omega) = C(\omega,T) = \int_0^T \left(1 - \frac{t}{T}\right)\rho(t)\cos\omega t dt \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

$$S(\omega,T) = \int_0^T \rho(t)\,\text{sir}\,\omega t dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

If $\rho(T)$ is small we can replace $C(w,T)$, $S(\omega,T)$ by

$$\bar{\phi}(\omega) = C(\omega) = \int_0^\infty \rho(t)\cos\omega t dt,\ S(\omega) = \int_0^T \rho(t)\sin\omega t dt.$$

From (9) we see that the variance of $\phi(\omega)$ is always greater than the square of its theoretical mean value however great T may be, but from (10) the covariance of neighbouring $\phi(\omega)$ is small of order T^{-2} (where ω is not small).

If, beside possible fluctuations, $\rho(s)$ decreases on the whole like exp. $(-\ ks)$, and if we take a band of frequencies such that

$$\Delta\omega = 2\pi n/T$$

then the variance of the mean $\phi(\omega)$ over the band divided by the square of the theoretical mean is of the form

$$\frac{1}{n} + \frac{0}{k^2T^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where 0 varies with ω, but is generally of the order of 1. Hence we should choose a band-width of frequencies

$$\Delta f = mk \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

with m between 1 and kT.

Dr. Harold Jeffreys said that he had no experience in detecting empirical periodicities in geophysical data. He had a good deal of experience of failing to find evidence for them. In frequencies of earthquakes, for instance, the uncertainties found by the usual multinomial theory were much too low because earthquakes at a given place did not occur independently- they might occur in batches up to thousands over an interval of a few months. A single series of shocks in one place seemed to follow a simple law of chance of the form $dt/(t + \alpha)$ with no superposed periodicity.

He was a little surprised that nobody had mentioned that the Schuster criterion was identical with χ^2 for two degrees of freedom; although he believed Schuster was first, Turner had a useful set of two-figure tables for harmonic analysis.

In estimation problems he used inverse probability or maximum likelihood, but in geophysics it was usually found that the calculation became prohibitively long, designs could not be balanced, and the normal law never held. Therefore the maximum likelihood method was usually employed for the main features, but corrections were applied for minor effects. Preliminary examination was needed to find which were the major and which the minor effects, and this might be the longest part of the work.

In the variation of latitude problem there were measures of the direction cosines of the earth's axis at intervals of 0·1 year over 50 years. The free motion was presumed to be maintained by irregular disturbances as for Yule's pendulum. The recurrence relations were

$$l_n = \alpha l_{n-1} - \beta m_{n-1} + \sigma$$

$$m_n = \beta l_n - 1 + \alpha m_{n-1} + \sigma$$

where

$$\alpha = e^{-kT}\cos\gamma T,\ \beta = e^{-kT}\sin\gamma T$$

If this was all, the determination of α, β, and hence k, T by least-squares would be straightforward. Dyson tried it, and found that this method led to an estimate of damping which was quite inconsistent with the actual persistence of the amplitude and phase of the free movement. The trouble was that the observed quantities were not the l, m of the equations, and that the error of observation σ' was not negligible. In fact it was larger than σ. One way of separating them was to use an equation for observations ρ intervals apart—$\rho = 24$ worked fairly well. When he studied the variation of the amplitude by harmonic analysis he found that it had changed by a few large jerks, not by many small ones, as if the hits had been vigorous but infrequent. (There was no physical explanation for this at present.) There was actually an interval of about 15 years when the motion seemed to show no irregularities but observational error, and he obtained what he believed to be the best solution by using this period alone and taking σ zero. This reduced the problem to the other extreme case, which was not difficult.

The other problem was concerned with the mass of the moon as determined from the motion of Eros in the 1930–31 approach. These perturbations were not quite harmonic, but they were proportional to a known function of the time, $f(t)$. The serial correlation came in because the positions of the comparison stars had errors, part of the errors affecting all the stars in a region. Sir H. Spencer Jones had dealt with the problem, but Dr. Jeffreys thought he had made some improvements. He determined the mean residual at each time when $f(t)$ was zero, using residuals over about five days about those times, and interpolated to get an allowance for star errors as nearly as possible independent of $f(t)$. Then separate estimates of the coefficient for each interval from one zero to the next were made, and a general estimate was obtained by combining the results by least-squares. The variation of these estimates among themselves provided an estimate of uncertainty. The errors of the datum values would be expected to produce a correlation of $-\frac{1}{6}$ between successive estimates, but this was small enough to be taken into account by a small correction.

Mr. J. E. Moyal said that he had been interested for several years in continuous time series or, more generally, random functions of time of the type mentioned by Dr. Bartlett, because he was interested in the application of statistical methods to physical theory; in physics it was the continuous type of random process that prevailed rather than the discrete. The notion of a random variable is generally sufficient in the equilibrium problems of statistical mechanics; the extension to the notion of random function of time becomes necessary in fluctuation problems—*e.g.*, Brownian motion, electrical fluctuation; in the theory of non-uniform states; in the statistical theory of turbulent motion in fluids. He was particularly interested in Dr. Bartlett's approach to the formidable problem of estimation in this type of continuous process; as Dr. Bartlett pointed out, this problem is by no means of academic interest only, since it is possible to devise physical instruments which will give a measure of various types of time averages, and a solution of the estimation problem is necessary to discriminate between them and choose the most efficient instrument in the measurement of a given quantity. An example is the use of an electronic analyser to measure the spectrum of autocorrelation functions in electrical fluctuations and turbulent motion. This is required to give the mean square of the Fourier components of the random process; the output meter used could be made to give readings of time averages of square, absolute value or peak amplitude; the solution of the estimation problem leads to the obvious choice of the instrument giving the time average of the square as the most efficient. Dr. Bartlett mentioned that the introduction of an uncorrelated impulse function in his equation 26 was not rigorous because it was not then possible to define the derivative of the velocity. It is possible to render Dr. Bartlett's expression rigorous (*a*) by using the theory of the conservation of momentum and energy as in the classical theory of impulsive motion, (*b*) by supposing the random impulses do possess correlations lasting over a period which is short compared to the period of the velocity autocorrelation (*i.e.*, such that the impulse autocorrelation tends to zero within a period during which the velocity autocorrelation is still nearing unity).

Major J. M. Hammersley said that Dr. Bartlett had discussed the "effective" number of degrees of freedom appropriate to the estimates of variance of the autocorrelations of a time series. He mentioned the use of the factors

$1/\Sigma\rho_r$ in connection with the mean,

$1/\Sigma\rho_r^2$ in connection with variance and covariance.

A recent official publication, prepared by Major Bayley and the speaker, enlarged upon this aspect of autocorrelation theory. Before giving a summarized version of the results of this publication, it was necessary to sketch in the nature of the problem which confronted them.

In trials of anti-aircraft equipment, the instruments under trial produced a continuous set of data. For example, when a radar was tracking a moving target it necessarily fed to the predictor a continuous record of the estimated co-ordinates of the target; and these co-ordinates were continuous functions of time. For practical reasons, however, these data were only recorded by cameras for analysis of the trial at discrete points of time. Usually the interval of time between

successive camera recordings was constant for any one set of recordings—*e.g.*, the errors of the radar might be photographed by a cine-camera running at 16 frames/sec., or by a single-shot camera operated by a timing unit, at intervals of, say, 2 seconds. The performance of the equipment was then assessed by calculating various statistics (such as arithmetic mean, standard deviation, etc.) from these sets of recorded data.

As a rough statement it might be said that the closer together in time were the recordings, the more was the information yielded about the behaviour of the equipment. On the other hand, the greater was the amount of computation necessary to produce the required result. A compromise was therefore required between economy of computing time, photographic materials, etc., on the one hand, and amount of information in the result on the other hand. Bearing this in mind, the problem was "What was the best time interval to choose between successive observations?"

Their approach to the problem was as follows: It was well known that the standard error formulæ for the unbiased estimates of the mean and variance of a set of n independent observations were respectively

$$\text{var. } (\bar{x}) = \sigma^2/n, \qquad \text{var, } (s^2) = 2\sigma^4/(n-1).$$

(The latter formula assuming a mesokurtic distribution of the x's.) These formulæ did not, of course, hold when the readings were correlated in time; but they might then *define* two numbers n_b^* and n_v^* such that

$$\text{var. } (x) = \sigma^2/n_b^*, \qquad \text{var. } (s^2) = 2\sigma^4/(n_v^* - 1).$$

These quantities n^* could be expressed in terms of n and the autocorrelation coefficients of the series. Their formula for n_b^* corresponded to that given by Mr. Yule in *J.R.S.S.*, CVIII (1945); while that for n_v^* did not appear to have been given previously.

In the formula for var. (s^2), s^2 was defined as the unbiased estimate of variance. As such it differed slightly from the ordinary definition for independent observations, being in fact

$$\frac{n_b^*(n-1)}{n(n_b^* - 1)}$$

times the usual definition.

If they supposed that the observations were evenly spaced in time along a continuous series, the number of observations, n, would be inversely proportional to the time interval between them; and the labour of computation would be directly proportional to n. They might then define the efficiency of the computation by the percentage ratios

$$E_b = 100\, n_b^*/n, \qquad E_v = 100\, n_v^*/n.$$

They had prepared methods and tables for evaluating n^*. Hence they had been able to assess the efficiency of the computation. On comparing this with the computation labour required, they could arrive at an optimum time interval which would give a workable compromise between efficiency and effort.

In preparing their tables they had assumed that the autocorrelation coefficients could be expressed in the functional form

$$\rho(s) = ae^{-\mu s} \cos \lambda s$$

It might be mentioned that in first solving the problem for dispersion they confined their investigations to the standard error of the mean square error about the parametric mean. For this case they introduced the quantity n_d^* which differs slightly from n_v^*.

MAJOR G. V. BAYLEY said that in following Major Hammersley he would confine his remarks to the practical application of the methods and tables which they had prepared.

Several occasions arose where much was already known about an equipment, but where further trials involving the measurements of the mean and dispersion of errors were necessary. He would discuss firstly the case where some sample correlograms relating to the equipment were already available. The first step was to estimate, in effect, the constants in the formula:

$$\rho(s) = e^{-\mu s} \cos \lambda s$$

which would produce a reasonable "fit" for the observed values of r_s. Other formulæ for ρ were sometimes more appropriate.

Knowing the average length of a series, they then calculated n and n^* from the tables for a selection of time intervals. Finally an "optimum" time interval between observations could be chosen for subsequent trials. This would depend on whether high reliability was required—*i.e.*, large n^*—or high efficiency—*i.e.*, a large ratio of n^*: n—or whether a virtual independence of observations was required with n^* approximately equal to n.

Frequently, however, a sample correlogram was not available. But often a more or less reliable value of the period of the correlogram was known. They therefore examined n^* and E for series with inherent autocorrelation of specified types. They reached a number of tentative conclusions:

Firstly, as the time interval was reduced, n of course increased, but n^* varied in a very different manner, reaching maximum and minimum points. Finally a stage was reached where it was

pointless to reduce the time interval further. Here large increases in n produced relatively small increases in n^*.

Secondly, n and n^* might differ appreciably. It was usually impracticable to quote either a standard error or n^* for the estimated statistics. Nevertheless they felt that to quote n alone might be entirely misleading.

Thirdly, n_b^*, for the mean, differed from n_d^*, for dispersion. n_b^* might be several times greater than n, but n_d^* was never greater than n. The order of these differences depended on the damping of the correlogram and the relation of the time interval to the "period" of the correlogram. These conclusions on the reliability of the mean might be compared with some similar observations by Mr. Kendall in *J.R.S.S.*, CVIII, p. 96, paragraph 11.

Finally they were able to recommend values of the time interval in terms of the correlogram period, which would give high efficiency. They also indicated which values should be avoided.

A report had been issued setting out these methods in detail, and they would be glad to submit it in condensed form as a written addendum to this discussion†.

They had mentioned this elementary but fundamental problem in autocorrelation, not only to quote another instance of its practical application in gunnery, but also because they found that this device, the conception of n^*, enabled them to represent their difficulties in a not very easy subject, to those who had little time to study statistics in detail.

In conclusion, there was one other point to which he should like to refer. Dr. Cunningham and Mr. Hynd mentioned in their paper that there might be a wide field of application for an instrument which would evaluate, between limits, the integral of the product of two functions. An obvious example which occurred to the speaker, was the calculation of the large number of joint life annuities and other functions, required by actuaries. There the age separation of the two lives was analogous to lags in the autocorrelation coefficient. Might he, therefore, emphasize the great value of some figures, or even an opinion, indicating the accuracy of such instruments, not only in their present application but in other spheres of research?

DR. HARTLEY wished to say a few words about the comparative efficiency of the calculating machines mentioned by the speakers. A description had been given of two machines *specially* designed for the calculation of serial correlations, and it might be of interest to compare their efficiency with that obtained on standard commercial machines currently produced and serviced. Under certain conditions the equipment most suited to the calculation of autocorrelations, if carried out on a large scale, consisted of a Hollerith Tabulator, a Reproducer and Sorter. The method and the plugging would be very special, and could not be given here.

To fix the idea they would consider a definite example of a serial correlation calculated from 50, two-figure observations. The Post Office relay-machine would cope with this accuracy, its working speed being about 1 second per product formed. It would therefore produce a serial correlation of this kind in about 50 seconds. The Hollerith equipment would form and print in some 80 seconds 12 sums of products representing 12 serial correlations. This worked out at a theoretical speed of about 7 seconds per serial correlation. Allowing for some contingencies, it would appear that the Hollerith installation would be about 6 times as fast as the Post Office relay-machine.

The comparison was not a very fair one, because the Hollerith equipment mentioned would represent a larger installation. Nor was it easy to make any comparison of cost, because the Post Office machine was a single machine, specially made for the purpose, and the Hollerith equipment could not be purchased, it had to be hired. Nevertheless he would ask the last speaker whether his department had considered the possibility of using this standard Hollerith equipment. For even if the total amount of work in the department did not warrant an installation of this sort, there were possibilities of enlisting its service on an *ad hoc* hire basis.

With regard to the optical machine described by Mr. Foster, the comparison was reversed. Its time was faster than that of the Hollerith Tabulator. On the other hand, the accuracy of the optical instrument was limited. The time to draw the curve would appear to be larger than that taken to punch numerical values of its ordinates on to Hollerith cards, but if, as happens in Mr. Foster's work, the graph is plotted directly by the recording instrument, this time would be virtually eliminated, and in these circumstances the optical instrument appears to be particularly efficient. Nevertheless, he would suggest that Mr. Foster had been a little unfair in stating that the time taken by his optical instrument was negligible compared to that needed in arithmetical calculation of serial correlations. His time, 5 seconds, was not very much shorter than that of 7 seconds, the theoretical time taken by the Hollerith. For the purpose for which Mr. Foster's instrument had been designed it seemed to be admirable, and it would be difficult to find anything suited better for this particular purpose.

MR. STONE said he ought perhaps to make it clear at the outset that, through nobody's fault, he only became aware that he should be invited to speak at this meeting about twenty-four hours before. He hoped, therefore, that he would be forgiven if his remarks were addressed to the sub-

† This addendum will be printed in Part II of this Journal.

ject generally rather than to specific points raised in the very interesting papers which had been read that afternoon.

There seemed little doubt that the type of analysis which had been the subject of these papers was destined to exercise a profound effect on those sciences in which it was claimed that cycles had been found empirically, but that these cycles were ones for which no satisfactory explanation could be obtained. It might well be that the natural sciences would be more affected than the social sciences, since in economics theories involving exact periodicities had for some time, he thought, been on the decline. The picture presented by an autoregressive scheme with appropriate coefficients was one of a system with a propensity to oscillate regularly, but which was constantly being disturbed by random shocks. This seemed to be a good representation of economic systems in the large, and, looking back, it was perhaps surprising that the search for exact periodicities had been so intensive in this field.

Another aspect of the present representation was that dynamic models of economic systems must be stated in stochastic terms, and the success of prediction from such models must depend on the relative importance of the disturbances compared with the regular influences at work. A recognition of this problem had led to much interesting work on systems of stochastic simultaneous equations with which the name of Haavelmo was particularly associated. This work threw light on the dangers of single equation systems for the calculation of structural coefficients where, as was usually the case in economics, the single equation formed part of a set of stochastic simultaneous equations. It seemed to the speaker that this field of work arose from a similar view of the phenomena under investigation to that adopted in the autoregressive approach, and was a matter which might perhaps fruitfully be discussed by the Society on another occasion.

There was a final point he would like to make at the risk of stressing the obvious. To say that the variables of a system were subject to random shocks as well as to the regular oscillatory tendencies inherent in the system was not the same as to say that those variables if subjected to factorial analysis would show large specific factors. The reason was, of course, that the disturbances might be incorporated in the common factors. The speaker had recently completed a factorial analysis of the components of the national income and expenditure in the United States of America over the short period 1922–38 (Barger's data), and it appeared that over 97 per cent. of the variance of these components could be explained by three factors which might be identified with the national income, the rate of change of the national income and time. He was not suggesting that this simple analysis would necessarily be equally successful in explaining variation if it were possible to analyse data covering a longer period. But the analysis did perhaps indicate the close-knit character of variation in economic systems where the variables might be supposed to be influenced by random disturbances as well as by a regular mechanism of change.

THE CHAIRMAN regretted that time made it necessary to close a most interesting discussion which had ranged over a wider variety of applications of the subject introduced to the meeting than he could ever remember happening on any other occasion. He would invite the openers to reply to the discussion, but imagined that they would probably prefer to reply through the medium of the *Journal*.

The following written contributions were received after the meeting:—

F/LT. W. R. BUCKLAND: May I take this opportunity to thank Dr. Cunningham, and his staff at A.W.A.S., for this paper and for the pleasant contacts during the war on the subject of OBOE bombing. A possible derivation of the term is interesting: OBOE—Hautbois—high wood—"Mosquito," an all-wood air-framed aircraft, with its high speed and altitude characteristics. The figure of 30,000 feet for the release altitude which appears in the results at Fig. 8, when coupled with the optical nature of the radar beam, completes the picture in this respect. An essential feature of accurate bombing, or target-marking, is an uninterrupted undeviating run over the target. This yields a relatively more simple statistical position to the defences. However, the relatively high speeds of the "Mosquito" gave it an excellent opportunity to cheat the defences in the target area, and thereby tended to minimize the effect of the $y(t)$ component. In fact, we are told at 2·11 that σ_y is 30 yards only (for experimental data from P.F.F. aircraft).

With regard to σ_v, it is of interest that, even though more extensive smoothing does mean some loss of instantaneity in the estimate of the forward velocity of the aircraft, the value of σ_v given at 4·74 m.p.h. is only of the order of $1\frac{1}{2}$ per cent. of the forward velocity, and probably not greatly significant in view of the better bombing which results for values of α greater than 30°.

In considering Fig. 8; with a value of α at 67 – 70°, and using two of the Case (ii) curves where the smoothing period is constant $T = 20$ seconds), although, as the paper points out, the bombing error for $t = 20$ seconds is lower than for $t = 0$ seconds, it is clear that the theoretical bombing error (σ_x) is of the order of 300 ± 25 yards. From this it is but a short mental step to appreciate the enormous advance in aerial warfare which is known as "The Battle of the Ruhr," 1943. Aircraft could operate and successfully bomb targets which had hitherto been protected by that cousin of London's fog, RUHR haze, as well as by the more usual natural protection in the form of cloud.

The climax of the use of the OBOE system was in the successful bombing of the myriad tactical targets prior to, and after, June 6th, 1944. On the eve of the D-Day assault heavy coastal defences were subjected to a pounding through 8–9/10ths of cloud. So effective was this, that the concentrated fire upon the landing beaches never materialized and the defences failed to carry out their allotted task in the enemy's plans. For this type of target it was necessary to have a very high degree of accurate geographical location (to 1/100th part of a minute), based principally upon the work of the photographic interpreters on the various Intelligence Staffs.

DR. J. L. SPENCER-SMITH: Mr. Foster's work on cotton slivers and yarns has been very similar to ours at the Linen Research Institute on flax slivers, although there are certain differences. We may find disturbed periodic variations as opposed to oscillatory variations such as the drafting wave. For these the periodogram works well enough as a means of determining the period length, but our object was also to measure the amplitude of the variation. For this the periodogram was of little use because the amount of phase disturbance was not the same in all slivers. We were therefore led to the serial correlogram method, and here we met the added difficulty that owing to the length of flax-fibre strands, some of which may be 30 inches long, there is a very pronounced secular trend. Also for physical reasons the concept of a damped harmonic motion maintained by disturbances was untenable. It was to meet this type of series that I made the analysis to which Dr. Bartlett referred, and I agree with him that the concept of independent disturbances of amplitude, phase, and trend has only a limited application, although it may be a useful preliminary approach to a series when little is known about the physical processes which generate the series. It seems important, however, that such a series can produce a correlogram which does not differ significantly from those of the series of Mr. Kendall and Dr. Bartlett. I think this supports Mr. Foster's emphasis on the need for a physical approach to every time series, because the serial correlogram of any unknown series may be reproduced by several different physical processes. Thus for example we can reproduce the serial correlogram of practically any economic series on the basis of a textile sliver, although I do not suppose that the processes are the same. The reason for this is that serial correlogram does not specify the time series completely. Some other data are required: the frequency distribution of the whole series may suffice, but I believe we may also have to consider both the form and the variation of the frequency distribution of short runs of consecutive terms along the series.

If the same correlogram can be produced by different physical processes the question arises: Will a significance test calculated for one process hold for the serial correlogram resulting from another process? In other words, is there any general significance test for the serial correlogram?

DR. BARTLETT, in reply: As I mentioned at the end of the meeting, I only partly agree with Mr. M. G. Kendall's remarks on my own paper. I agree that much remains unsolved in the theory of time-series—for example, the problem of goodness of fit of the correlogram; I also agree that we must be careful not to allow some of this mathematical theory, which has its most exact application in physical problems, to run away with us when we are applying it in the less exact sciences. On the other hand, I think it important to realize the generality of some of the theory I have described. The phrase "continuous time-series" is perhaps misleading, for the only essential continuity required is that the series exists continuously through time, and the shocks to which it may be subject are in fact of just the kind which, if I understand him correctly, Mr. Kendall is suggesting should be considered. Of course one obvious condition if a *statistical* analysis of any kind is to be possible is that no shock shall be of a unique and "epoch-making" character, such as, for economic series, the outbreak of war or of a general strike. Dr. Jeffreys mentions a problem where he was lucky enough to have a length of series with no shocks at all, but I suspect that the times when we can be certain that such a situation exists will be very limited in number. With regard to the construction of experimental continuous series, I pointed out that such series, with predictable theoretical properties, have been recorded in physical problems, and such experimental records can be used for theoretical studies. The Cowles Commission for Research in Economics have, I think, constructed and studied experimental series of this kind. I seem to recollect that Mr. Kendall, in his spoken remarks, raised also the question of superposed error, and queried the value of measuring it by observational differences. This reference of mine to what is done in practice (see last paragraph of section 6 preceding the discussion of Wolfer's sunspot numbers) was not intended to be one of approval; I indicated that the effect should more properly be estimated by its depression of the correlation coefficients.

While I do not altogether agree with Professor P. J. Daniell's statement that "it is best . . . to assume normal distributions of variables", I found his contribution most interesting, especially in its emphasis on the close relation, *even for the sample*, between correlogram and periodogram analysis, so that if the proper interpretation of either analysis is made, there should be no question of getting results by one method not implied by the other.

The problem raised by Cunningham and Hynd of the survival chance from a number of shots with correlated errors is a generalization of the problem of a salvo of rounds with a common

aiming error; this case, which they call the " shot-gun " case, is important in itself, and received a lot of study during the war, but the more general problem, for which it provides the upper limit, I have not seen discussed elsewhere in any detail before.

Dr. Spencer-Smith's query about correlograms is related to the unsolved problem of goodness of fit already referred to. For a stationary series a correlogram does specify the character of the series completely if the process is normal, but not otherwise. But I have noted that for a wide class of time-series the sampling errors of the correlogram are, to the first order, dependent only on the correlogram and not on other features of the distribution, to which any test of goodness of fit should therefore be insensitive. That is something, but not very much, for we cannot hope to estimate the true correlogram at all accurately unless we can specify it by one or two unknown constants, and this means specifying the underlying mechanism of the series.

Finally, besides recording my personal thanks to the various speakers in the discussion, I would like to make two comments myself on the formulæ in my own paper.

(i) I have referred in connection with equation (25) of my paper to Campbell's theorem, but the latter is usually restricted to random impulses of the simplest type characterized by the number occurring in any finite time interval being a Poisson distribution. Equation (25) is more general, being associated with the homogeneous random process, which may be regarded as a linear superposition of impulses belonging to such a simple type.

(ii) I have confined myself to single time-series, but there is no theoretical difficulty in discussing by similar methods the joint distributional properties of more than one series, *e.g.* of two coupled series which, even when only of the first order, can exhibit similar oscillatory properties to the single series of the second order discussed in my paper.

Such an extension will help to link the present work with the work on simultaneous stochastic equations referred to by Mr. Stone. I have worked out some of the corresponding correlational and sampling formulæ, but in view of the length already of the printed contributions to this symposium, I do not propose to record them here in detail.

MR. FOSTER, in reply: I should like to comment on Drs. Daniel's and Hartley's summing up of the various methods for calculating correlograms. The advantages of the optical method are (*a*) speed and (*b*) that it will deal directly with traces from a recording instrument. The work on cotton slivers, which involved some hundreds of correlograms, would have been practically impossible by any of the other methods. (The planimeter integrator, for example, was specially designed for this work, but was found to be much too slow and laborious).

When the data are in numerical form (*b*) becomes a disadvantage; but, if considerable numbers of correlograms are required, the speed of the method is such an advantage that it would be worth while to make a special machine to plot the data in the required form. This could either be photographic as suggested in the paper, or, better, could punch or cut the observations into opaque cards.

When, however, only small numbers of correlograms are needed, the relay computer or the planimeter integrator have obvious advantages for dealing with numerical observations, and Dr. Hartley's Holbrith method, for those who have access to the equipment, combines these advantages with a very high speed of calculation. My comparison of the optical method with arithmetical calculation referred to calculation on the ordinary keyboard machines.

It is difficult to give a precise reply to Mr. Kendall's question about the calculation of high order correlation coefficients by the optical method, because the method has not been pushed to the limit of its accuracy, and also because it has so far only been used on the traces from recording instruments. With some refinements in design I think it should be capable of calculating correlation coefficients to within 0.01 on a long series of discrete observations. In the work on cotton slivers instrumental errors were negligible compared with the variations from sample to sample of the same sliver. This would, I think, apply also to most other material to which correlogram analysis is likely to be applied.

DR. CUNNINGHAM and MR. HYND, in reply: In our paper we have tried to cover both the theoretical foundations of stochastic work, and also all the practical applications to military science which have come to our notice, and which have been carried through to a successful conclusion. So far as we are aware, therefore, the methods of autocorrelation analysis have not been applied to naval gunnery. There is, however, still much research of this nature to be done in the field of armament, both in improving the efficiency of the weapons of the present and in designing those of the future.

In the problem of survival chances against a rapidly firing gun, we had given some consideration to an approach involving an adaptation of the Fokker-Planck Differential equation, comparable with the suggestion of Dr. Daniels, but difficulties were encountered, and the method which we have presented in our paper proved more fruitful. Considerable progress has also recently been made in obtaining inequalities which will determine, within reasonable limits, the survival chance for rather long bursts of gunfire, when the convergence of the series (3·25) is not satisfactory.

Being a digital machine, the relay computer is in one sense perfectly accurate. It is, however,

necessary to group the data to the nearest integer in the range $\pm$ 63; in general, this has a negligible effect on the correlogram, but if the grouping be very drastic, it is possible to introduce corrections analogous to Sheppard's corrections, which are valid when certain plausible assumptions are satisfied, and which have been worked out by Dr. Daniels while he was working with us at the Ministry of Aircraft Production.

In comparing the relay computer with the Hollerith, we have formed the impression that the data take much longer to be prepared on the Hollerith cards than on the punched tapes which we use; on the other hand, actual points on the correlogram are evaluated more rapidly by the Hollerith, once it has reached that stage in its processes. In consequence, the relay computer is the speedier as long as the number of points desired on the correlogram is small (perhaps less than 1 : 5) compared with the number of data in the sequence to be correlated, while the Hollerith is the better under other conditions.

Sequential Sampling Formulae for a Binomial Population.

By J. P. Burman, B.A.

Introduction.

In this paper we shall be concerned with the schemes of sequential sampling, which have been developed by A. Wald[1] in America and G. A. Barnard[2] in his recent paper (see p. 1 of this *Supplement*). The primary object is to develop exact and workable formulae for the operating characteristic and average sample size of open schemes, and to give simplifications for small fractions defective.

Section 1. 1.

The sampling procedure described by Barnard is as follows:—

Start with a score H_2.
For each non-defect sampled, add 1.
For each defect, subtract b.
Accept the batch when the score reaches $2H = H_1 + H_2$.
Reject the batch when the score falls to zero or below.

It is assumed that the fractions defective which the scheme is designed to distinguish are reasonably small, so that the appropriate value of b is large (usually at least 10) and may without inconvenience be taken as an integer. Naturally we may then suppose H_1, H_2 integers without further restriction. In practice the batch size will be finite, but it is supposed so large compared with the number normally sampled that it may be treated as infinite and the rectifying effect of sampling may be ignored.

Section 1. 2.

Let the score at any moment be x and u_x be the probability of the score reaching $2H$ without previously dropping to 0—that is, the chance of acceptance of the batch. If during sampling the score again reaches x, the chance of acceptance is again u_x, for the past history of the score is clearly irrelevant.

Now u_x = (chance that next observation is a non-defect) $\times$ (chance of acceptance when score is $(x + 1)$).
+ (chance that next observation is a defect) $\times$ (chance of acceptance when score is $(x - b)$).

Thus
$$u_x = qu_{x+1} + pu_{x-b}\ (x = 1, 2, 3 \ldots (2H - 1)) \quad \ldots\ldots \quad \text{(i)}$$
where p = fraction defective, $q = 1 - p$.

This is true as long as no critical score has yet been reached; hence the restrictions on x. We also have:
$$u_0 = u_{-1} = u_{-2} \ldots = u_{-b+1} = 0 \quad \ldots\ldots\ldots \quad \text{(ii)}$$
$$u_{2H} = 1 \quad \ldots\ldots\ldots \quad \text{(iii)}$$
The acceptance score is exactly $2H$. That on rejection varies from 0 to $(-b + 1)$.

Now
$$u_{x+1} = \frac{1}{q} u_x - \frac{p}{q} u_{x-b}.$$
Hence
$$u_2 = \frac{1}{q} \quad {}_1$$
$$u_3 = \frac{1}{q} u_2 = \frac{1}{q^2} u_1 \text{ and so on up to: } u_{b+1} = \frac{1}{q^b} u_1.$$

The term u_{x-b} then begins to operate and the formula becomes more complicated. It is necessary to guess a solution.

Consider the function:

$$F(x) = \frac{1}{q^x} - (x - b - 1)\frac{p}{q^{x-b}} + \binom{x - 2b - 1}{2}\frac{p^2}{q^{x-2b}} \; . \; . \; . \; (x > 0).$$

The general term $= (-1)^K \binom{x - Kb - 1}{K} \frac{p^K}{q^{x-Kb}}$

where $\binom{x - Kb - 1}{K} = (x - Kb - 1)(x - Kb - 2) \; . \; . \; . \; (x - Kb - K)/K\,!$

The series continues as long as $x - Kb - K > 0$

$$K < \frac{x}{b+1}.$$

$$F(x) - qF(x+1) = \sum_{K=0}^{K_1} (-1)^K \binom{x - Kb - 1}{K} \frac{p^K}{q^{x-Kb}} - \sum_{K=0}^{K_1} (-1)^K \binom{x - Kb}{K} \frac{p^K}{q^{x-Kb}}.$$

If $F(x+1)$ contains one more term than $F(x)$ then the latter may have a term formally added to it which has a zero factor. $K_1 =$ integral part of $\frac{x+1}{b+1}$.

$$\therefore \quad F(x) - qF(x+1) = \sum_{K=1}^{K_1} (-1)^{K+1} \binom{x - Kb - 1}{K - 1} \frac{p^K}{q^{x-Kb}}$$

since the first term disappears.

$$= p\left\{\frac{1}{q^{x-b}} - (x - 2b - 1)\frac{p}{q^{x-2b}} + \; . \; . \; . \right\} = pF(x - b).$$

since when
$$K_1(b + 1) < x + 1$$
$$(K_1 - 1)(b + 1) < x - b$$
$$\therefore \quad F(x) = qF(x+1) + pF(x - b).$$

So far F has only been defined for $x > 0$.
Let $F(x) = 0$ when $x \leqslant 0$.
Then $F(x)$ satisfies conditions (i) and (ii) for u_x.
$\frac{F(x)}{F(2H)}$ also satisfies (iii). Hence it is equal to u_x.
From which we obtain the operating characteristic or initial chance of acceptance

$$u_{H_2} = \frac{F(H_2)}{F(2H)} = \frac{F(H_2)}{F(H_1 + H_2)} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(iv)}$$

since initially $x = H_2$.
$F(x)$ is most conveniently written as:

$$q^{-x}\left\{1 - (x - b - 1)pq^b + \binom{x - 2b - 1}{2}(pq^b)^2 \; . \; . \; . \; . \right\}$$

Section 2. 1.

Now let $v_x =$ probability of rejection when score is x.
If it is assumed that $p + q = 1$, of course, $v_x = 1 - u_x$, but a solution will be required with p and q as independent constants. Conditions are:

(i) $v_x = qv_{x+1} + pv_{x-b} \qquad (1 \leqslant x \leqslant 2H - 1)$
(ii) $v_0 = v_{-1} = \; . \; . \; . = v_{-b-1} = 1.$
(iii) $v_{2H} = 0.$

Now
$$v_2 = \frac{1}{q}v_1 - \frac{p}{q}$$
$$v_3 = \frac{1}{q^2}v_1 - \frac{p}{q^2} - \frac{p}{q}$$
$$. \; . \; . \; . \; . \; . \; . \; . \; . \; . \; .$$
$$v_{b+1} = \frac{1}{q^b}v_1 - \frac{p}{q^b} - \frac{p}{q^{b-1}} \; . \; . \; . - \frac{p}{q}$$

and again it is necessary to guess the answer.

Try $$v_x = qF(x)v_1 - p\{F(x-1) + F(x-2) \ldots + F(x-b)\}$$
for $x > 0$.

This is a linear function of the F's and therefore satisfies condition (i) for $x > b$. Moreover, it is so chosen as to satisfy (i) for $1 \leqslant x \leqslant b$. We can make it satisfy (ii) by definition, since the trial solution has only been defined for $x > 0$.

(iii) will be true if $0 = v_{2H} = qF(2H)v_1 - pG(2H)$

where $$G(x) = F(x-1) + F(x-2) \ldots + F(x-b).$$

Then $$v_1 = \frac{p\,G(2H)}{q\,F(2H)}$$

$$v_x = p\left\{\frac{F(x)}{F(2H)}G(2H) - G(x)\right\} \quad \ldots \ldots \ldots \quad \text{(v)}$$

Section 2. 2.

Now let u_{xy}, v_{xy} be the probabilities of acceptance and rejection in exactly y more steps when the present score is x.

u_{xy} satisfies
(i) $u_{xy} = qu_{x+1,y-1} + pu_{x-b,y-1}$
(ii) $u_{x0} = 0 \; (1 \leqslant x \leqslant 2H-1)$
(iii) $u_{xy} = 0 \; (-b+1 \leqslant x \leqslant 0, \text{ all } y)$
(iv) $u_{2H,y} = 1 \; (y = 0)$
$= 0 \; (y > 0)$.

(i) is true for $\begin{cases} 1 \leqslant x \leqslant 2H-1 \\ y > 0 \end{cases}$.

Let $u_x(t) = \sum_{y=0}^{\infty} u_{xy}t^y$, which is the generating function of the sample size for acceptance.

Multiplying (i) by t^y and summing from $y = 1$ to infinity,

$$u_x(t) = u_x(0) + qtu_{x+1}(t) + ptu_{x-b}(t)$$

and since $u_x(0) = 0 \; (1 \leqslant x \leqslant 2H-1)$ by (ii),

(i)′ $u_x(t) = qtu_{x+1}(t) + ptu_{x-b}(t)$.

The boundary conditions become:

(ii)′ $u_0(t) = u_{-1}(t) \ldots = u_{-b+1}(t) = 0$.
(iii)′ $u_{2H}(t) = 1$.

Thus $u_x(t)$ obeys the same three conditions as u_x with the replacement of p and q by pt and qt. The same argument relates $v_x(t)$ and v_x, but the solutions to be used must treat p and q as independent and *not* use $p + q = 1$.

Hence $$u_x(t) = \frac{F(x, t)}{F(2H, t)} \quad \text{from (iv)}$$

$$v_x(t) = pt\left\{\frac{F(x, t)}{F(2H, t)}G(2H, t) - G(x, t)\right\} \quad \text{from (v)}$$

where $$F(x, t) = (qt)^{-x}\left\{1 - (x-b-1)(pq^bt^{b+1}) + \binom{x-2b-1}{2}(pq^bt^{b+1})^2 \ldots\right\}$$

and $$G(x, t) = \sum_{z=1}^{b} F(x-z, t).$$

Finally $$u_{H_2}(t) + v_{H_2}(t) = \frac{F(H_2, t)}{F(2H, t)}\left\{1 + ptG(2H, t)\right\} - ptG(H_2, t) \quad \ldots \quad \text{(vi)}$$

is the generating function of sample size, since initially $x = H_2$.

Section 2. 3.

The average sample size $A(p)$ (say) is obtained from differentiating (vi) with respect to t and putting $t = 1$.

$$A(p) = p\,\frac{F(H_2)}{F(2H)}\Big\{G(2H) + G_1(2H)\Big\} - p\,\Big\{G(H_2) + G_1(H_2)\Big\}$$
$$+ \left\{\frac{F_1(H_2)}{F(2H)} - \frac{F(H_2)\,.\,F_1(2H)}{(F(2H))^2}\right\}\Big\{1 + pG(2H)\Big\} \quad . \quad . \quad . \quad . \quad . \quad \text{(vii)}$$

where $\quad F_1(x) = \left[\frac{d}{dt}F(x,t)\right]_{t=1} = q^{-x}\sum_{K}(x - Kb - K)\binom{x - Kb - 1}{K}(-1)^{K-1}(pq^b)^K$

and similarly G_1 is defined.

Now it follows from the recurrence relation (i)′ satisfied by $F(x, t)$ that

$$G(x, t) = pt\,.\,G(x - b, t) + qt\,.\,G(x + 1, t)$$
$$= pt\,.\,G(x - b, t) + qt\,.\,G(x, t) + qt\{F(x, t) - F(x - b, t)\}$$
$$\therefore \quad G(x, t) = \frac{pt}{1 - qt}G(x - b, t) + \frac{qt}{1 - qt}\Big\{F(x, t) - F(x - b, t)\Big\} \quad \text{for } x > b.$$

Repeated application of this relation leads to the following:

$$G(x, t) = \frac{qt}{1 - qt}F(x, t) + \frac{(t - 1)qt}{(1 - qt)^2}\left\{F(x - b, t) + \frac{pt}{1 - qt}F(x - 2b, t) + \ . \ . \ .\right.$$
$$\left. . \ . \ . + \left(\frac{pt}{1 - qt}\right)^{K-1}F(x - Kb, t)\right\} - \frac{(pt)^K}{(1 - qt)^{K+1}} \quad \text{where } 0 < x - Kb \leqslant b.$$

Hence $$G(x) = \frac{q}{p}F(x) - \frac{1}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(viii)}$$

Multiplying $G(x, t)$ by pt, differentiating twice with respect to t and putting $t = 1$ in each case,

$$p\Big\{G(x) + G_1(x)\Big\} = qF_1(x) + \frac{q}{p}(1 + p)F(x)$$
$$+ \frac{q}{p}\Big\{F(x - b) + F(x - 2b)\ .\ .\ .\ .\ .\Big\} - \frac{(K + 1)}{p} \quad . \quad . \quad \text{(ix)}$$

$$p\Big\{2G_1(x) + G_2(x)\Big\} = qF_2(x) + \frac{2q}{p}(1 + p)F_1(x) + \frac{2q}{p^2}F(x)$$
$$+ \frac{2q}{p^2}\Big\{2F(x - b) + 3F(x - 2b) + \ .\ .\ .\ + (K + 1)F(x - Kb)\Big\}$$
$$+ \frac{2q}{p}\Big\{F_1(x - b) + F_1(x - 2b) + \ .\ .\ .\ + F_1(x - Kb)\Big\} - \frac{(K + 1)(K + 2q)}{p^2} \quad . \quad . \quad \text{(x)}$$

Substituting (viii) and (ix) in (vii) leads to:

$$A(p) = \frac{1}{p}\frac{F(H_2)}{F(2H)}\Big\{q(F(2H - b) + F(2H - 2b)\ .\ .\ .\ + F(2H - K'b)) - (K' + 1)\Big\}$$
$$- \frac{1}{p}\Big\{q(F(H_2 - b) + F(H_2 - 2b)\ .\ .\ .\ + F(H_2 - Kb)) - (K + 1)\Big\} \quad . \quad . \quad \text{(xi)}$$

where $0 < 2H - K'b \leqslant b$
$\quad\quad\ \ 0 < H_2 - Kb \leqslant b.$

Section 3.

By differentiating the generating function (vi) twice with respect to t and putting $t = 1$, we get the second factorial moment of the sample size distribution $\mu'_{(2)}$. The variance $\mu_2 = \mu'_{(2)} + \mu'_1 - \mu'^2_1$ where $\mu'_1 = A(p)$, the average sample size.

Substituting (viii) (ix) and (x) in $\mu'_{(2)}$ it is found finally that:

$$\mu'_{(2)} = \frac{2}{p}\left\{\frac{F_1(H_2)}{F(2H)} - \frac{F(H_2)\,.\,F_1(2H)}{(F(2H))^2}\right\}\Big\{q(F(2H - b) + F(2H - 2b)\ .\ .\ .$$
$$+ F(2H - K'b)) - (K' + 1)\Big\} + \frac{1}{p^2}\frac{F(H_2)}{F(2H)}Q(2H) - \frac{1}{p^2}Q(H_2) \quad . \quad . \quad \text{(xii)}$$

where

$$Q(x) = 2q\{2F(x - b) + 3F(x - 2b) + \ .\ .\ .\ \}$$
$$+ 2pq\{F_1(x - b) + F_1(x - 2b) + \ .\ .\ .\ \} - (K + 1)(K + 2q)$$

and $0 < x - Kb \leqslant b$ as before.

Section 4.

For many industrial applications the working range covers only very small values of p, and u_{H_2} is negligible except in this range. This is the case when H_1, H_2, and b are large. We write $H_1 = R_1 b$, $H_2 = R_2 b$, $pb = X$ and let $b \to \infty$, keeping R_1, R_2, fixed so as to find the limiting properties of this type of scheme.

Put
$$1 - p = e^{-p} + O(p^2) = e^{-p} + O(b^{-2})$$
$$\binom{H_2 - Kb - 1}{K} = \frac{b^K(R_2 - K)^K}{K!} + O(b^{K-1})$$

with a similar approximation for $(H_1 + H_2)$.
Thus $F(H_2)$ has the limiting form $F_{R_2}(X)$.

Where
$$F_{R_2}(X) = e^{R_2 X}\left(1 - (R_2 - 1)Xe^{-X} + \frac{(R_2 - 2)^2}{2!}X^2e^{-2X} \; . \; . \; . \; . \; .\right)$$

u_{H_2} has the limiting form $u(R_1, R_2, X) = \dfrac{F_{R_2}(X)}{F_{R_1+R_2}(X)}$

$\dfrac{A(p)}{b}$ has the limiting form $A(R_1, R_2, X)$

$$= \frac{1}{X}\frac{F_{R_2}(X)}{F_{R_1+R_2}(X)}\left\{F_{R_1+R_2-1}(X) + F_{R_1+R_2-2}(X) + \; . \; . \; . \; . + F_{R_1+R_2-K'}(X) - (K'+1)\right\}$$
$$- \frac{1}{X}\left\{F_{R_2-1}(X) + F_{R_2-2}(X) + \; . \; . \; . + F_{R_2-K}(X) - (K+1)\right\}$$

Summary.

Formulae are obtained relating to a sequential scheme with starting score H_2, winning score $(H_1 + H_2)$, and penalty (for a failure) b. For fraction defective p,

Probability of acceptance $u_{H_2} = \dfrac{F(H_2)}{F(H_1 + H_2)}$

where
$$F(H) = q^{-H}\left\{1 - (H - b - 1)pq^b + \binom{H - 2b - 1}{2}(pq^b)^2 - \binom{H - 3b - 1}{3}(pq^b)^3 \; . \; . \; .\right\}$$

the series terminating when the binomial coefficient vanishes or the largest factor in its numerator ceases to be positive. Formula (xi) gives the average sample size $A(p)$ and (xii) the variance of the sample size distribution.

The limiting form for u_{H_2} where b tends to infinity, $R_i = \dfrac{H_i}{b}$ is constant ($i = 1, 2$) and $X = pb$ is:

$$u(R_1, R_2, X) = e^{-R_1 X} \; . \; \frac{1 - (R_2 - 1)Xe^{-X} + \dfrac{(R_2 - 2)^2}{2!}X^2e^{-2X} - \; . \; . \; . \; . \; .}{1 - (R_1 + R_2 - 1)Xe^{-X} + \dfrac{(R_1 + R_2 - 2)^2}{2!}X^2e^{-2X} -}$$

$A(p)$ also has a limiting form if measured in multiples of b. From these formulae (either in their limiting forms or not) the corresponding curves of u_{H_2} and $A(p)$ plotted against p may be drawn. These are known respectively as the " Operating Characteristic " and the " Average Sample Size Curve," and they give a fairly complete idea of how the scheme will work in practice, enabling it to be compared with other sampling inspection schemes. The calculations for the variance of the sample size are rather more complicated, but not entirely impracticable.

Tables of percentage points for the operating characteristic and of average sample sizes for a considerable range of open and closed schemes will shortly be published by Mr. F. J. Anscombe.[3] The fundamental tables of the F function are available from Dr. H. O. Hartley of the Scientific Computing Service.

Acknowledgments.

The author wishes to thank Mr. G. A. Barnard for drawing his attention to the problem, and Mr. F. J. Anscombe for advice and encouragement throughout, and in particular for the idea of considering the limiting forms when b is large.

All this work was done while the author was at the Ministry of Supply, and thanks are due to the Chief Scientific Officer for permission to publish.

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[1] Wald, A., " Sequential Tests of Statistical Hypotheses," *Ann. Math. Stat.*, **17** (1945).
[2] Barnard, G. A., " Sequential Tests in Industrial Statistics," Supplement to *J. Roy. Stat. Soc.* (1946).
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Some Properties of Closed Sequential Schemes

By C. M. Stockman and P. Armitage

In a recent paper, Barnard [1] has outlined the theory and development of sequential sampling. In a sequential sampling scheme the sample size is a random variable, but it may sometimes be desirable to fix an upper limit to the sample size. Such a scheme may be called a closed sequential scheme. We shall consider Wald sequential schemes closed by the condition that if no decision has been reached by the time a sample of a certain size has been taken, the batch will be accepted if the score is then greater than some number, z, and rejected if the score is less than or equal to z (using the method of scoring introduced by Barnard). Our notation for the quantities defining a scheme is the same as that used by Barnard, except that his H and H' are replaced by H_1 and H_2, so that the scoring procedure becomes:

Start the score at H_2.
Add 1 mark for a good item.
Subtract b marks for a bad item.
Accept if the score reaches $H_1 + H_2$.
Reject if the score reaches or falls below zero.

This paper gives a method of obtaining the operating characteristic for any closed scheme. The limiting case as $b \rightarrow \infty$ and $R_1 = H_1/b$ and $R_2 = H_2/b$ are constant is dealt with, and a method is given of evaluating the average sample size as a multiple of b, for any fraction defective as a multiple of $1/b$. This limiting form is more convenient to use both from the point of view of numerical calculation, and also because it lends itself more readily to theoretical investigation.

Anscombe [2] has discussed the application of sequential sampling schemes, and has tabulated the operating characteristic and average sample size curves for various closed schemes which were obtained by the methods of this paper. He has also discussed the use of the limiting form by substituting a finite value for b.

1. *The lattice diagram.*

For a description and explanation of the lattice diagram, the reader should consult Barnard's paper. Some of the more important dimensions of the lattice diagram for a Wald scheme are given in para. 4. 1.

We shall first prove two simple but important properties of the lattice diagram:

(i) If the sampling is random, the probability of reaching (x,y)

$$= Np^y(1-p)^x, \qquad (1)$$

where N is the number of paths to (x, y) which are not interrupted by the acceptance and rejection boundaries. We shall call N the number of admissible paths (or simply the number of paths).

For all orders of the x non-defects and y defects are equally likely,

$\therefore$ Probability of reaching $(x, y) = N \times$ (Probability of reaching (x, y) by any one path).

Now the probability of reaching (x, y) by getting x non-defects followed by y defects $= p^y(1-p)^x$, where p is the fraction defective.

$\therefore$ Probability of reaching $(x, y) = Np^y(1-p)^x$.

(ii) The number of paths from

(f, g) to $(f + x, g + y)$ if no paths are interrupted by the boundaries $= {}^{x+y}C_x$. (2)

For the number of paths is equal to the number of ways of choosing the positions of the x non-defects in the sample of $(x + y)$, or the positions of the x unit displacements parallel to Ox in a total of $(x + y)$ displacements, $= {}^{x+y}C_x$.

If a sequential scheme is closed at a certain sample size, the two boundaries on the lattice diagram are joined by a diagonal line, some of the points on which will be assigned to the acceptance

boundary and some to the rejection boundary. The object of the following method is to find the number of admissible paths to the acceptance points, both on the original acceptance boundary and on the diagonal cut-off, and thence by (1) the probability of reaching each of these points.

2. *Exact method.*

An inner boundary is drawn on the lattice diagram—*i.e.*, one which keeps at a distance of 1 unit from the acceptance and rejection boundaries and coincides with the axes before the boundaries start, as shown in Fig. 1.

The area contained by this inner boundary is composed of a preliminary part (M_1 and M_2 in Fig. 1), followed by a repetitive part (A and B in Fig. 1). In any Wald sequential scheme the repetitive part of this area can be divided into two blocks of the shapes shown in Fig. 2, which will be called A and B respectively. The lengths of the horizontal and diagonal sides of these blocks

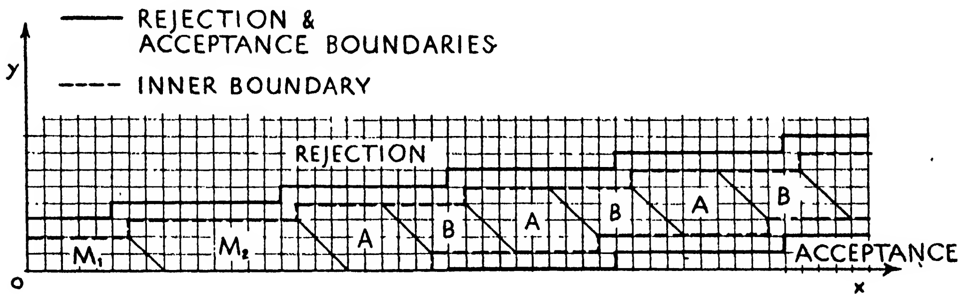

FIG. 1.

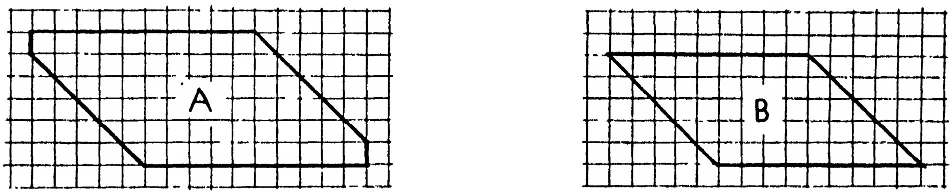

FIG. 2.

depend on H_1, H_2 and b; and it is shown in para. 4. 1 that when $(H_1 + H_2)/(b + 1)$ is integral the block B vanishes, making the solution simpler. Now, with each block is associated a matrix, the (i, j)th element of which is the number of paths from the i-th point of the L.H. diagonal side to the j-th point of the R.H. diagonal side (counting from the top), all of the elements being binomial coefficients, according to (2) above. Further, the matrices may be multiplied together so that the (i, j)th element of the product of two adjacent matrices, $S = (s_{ij})$ and $T = (t_{ij})$, is the number of paths from the i-th point of the L.H.S. of S to the j-th point of the R.H.S. of T.

For this number of paths

$$= \sum_k s_{ik} t_{kj} \text{ where } k \text{ refers to the } k\text{-th point of the R.H.S. of } S \text{ or the L.H.S. of } T,$$

$$= q_{ij}, \text{ where } (q_{ij}) = ST.$$

The area before the repetitive blocks start—*i.e.*, on the left of the first A—can also be divided into blocks of a similar kind, so that the elements of the corresponding matrices are binomial coefficients as before. The blocks will not in general be of the same size or shape as A and B, and their number will depend on H_1, H_2, and b. The first of these " irregular " matrices will, of course, be a row matrix, the elements of which give the numbers of paths from the origin to the points on the R.H.S. of the first block.

For convenience, the symbols A, B, M, Y, etc., will be used for both the blocks and the corresponding matrices.

Example.

Fig. 1 represents the scheme having $H_1 = H_2 = 25$, $b = 10$. The repeating blocks are A and B, and the irregular blocks are M_1 and M_2.

Defining $M = M_1M_2$ and $Y = AB$, the elements of the row matrix MY^n will be the number of admissible paths from the origin to the points on the diagonal line representing a sample of $19 + 11n$.

$$\text{We have } M_1 = ({}^8C_2 \quad {}^8C_1 \quad 1) = (28 \quad 8 \quad 1)$$

$$M_2 = \begin{pmatrix} {}^{11}C_1 & 1 & 0 & 0 \\ {}^{11}C_2 & {}^{11}C_1 & 1 & 0 \\ {}^{11}C_3 & {}^{11}C_2 & {}^{11}C_1 & 1 \end{pmatrix} = \begin{pmatrix} 11 & 1 & 0 & 0 \\ 55 & 11 & 1 & 0 \\ 165 & 55 & 11 & 1 \end{pmatrix}$$

$$A = \begin{pmatrix} {}^6C_1 & 1 & 0 & 0 \\ {}^6C_2 & {}^6C_1 & 1 & 0 \\ {}^6C_3 & {}^6C_2 & {}^6C_1 & 1 \\ {}^6C_4 & {}^6C_3 & {}^6C_2 & {}^6C_1 \end{pmatrix} = \begin{pmatrix} 6 & 1 & 0 & 0 \\ 15 & 6 & 1 & 0 \\ 20 & 15 & 6 & 1 \\ 15 & 20 & 15 & 6 \end{pmatrix}$$

$$B = \begin{pmatrix} 1 & 0 & 0 & 0 \\ {}^5C_1 & 1 & 0 & 0 \\ {}^5C_2 & {}^5C_1 & 1 & 0 \\ {}^5C_3 & {}^5C_2 & {}^5C_1 & 1 \end{pmatrix} = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 5 & 1 & 0 & 0 \\ 10 & 5 & 1 & 0 \\ 10 & 10 & 5 & 1 \end{pmatrix}$$

whence $$M = (913 \quad 171 \quad 19 \quad 1)$$

and $$Y = \begin{pmatrix} 11 & 1 & 0 & 0 \\ 55 & 11 & 1 & 0 \\ 165 & 55 & 11 & 1 \\ 325 & 155 & 45 & 6 \end{pmatrix}$$

$$\therefore \quad MY = (22908 \quad 3994 \quad 425 \quad 25)$$

$$MY^2 = (549908 \quad 94092 \quad 9794 \quad 575) \text{ and so on.}$$

The numbers of admissible paths to the acceptance points on the lower boundary, $(H_1, 0)$, $(H_1 + b, 1)$, $(H_1 + 2b, 2)$ etc. are the last elements of the row matrices M, MY, MY^2, etc., and so from (1) the probabilities of reaching these points for any fraction defective can be calculated.

It will be convenient to make the diagonal cut-off coincide with the R.H.S. of one of the blocks A or B. In the first case the maximum sample size will be of the form $H_1 + n(b + 1)$, and the numbers of paths to the points on this cut-off will be given by the elements of the matrix MY^nA. In the second case we shall use the elements of the matrix MY^n (as in the Example above), but the maximum sample size cannot be expressed so simply as before. Again the probabilities of reaching these points may be calculated from (1).

The total probability of acceptance for any fraction defective can thus be found, and the operating characteristic may be obtained. The result will of course depend on which points of the cut-off we assign to the acceptance boundary.

The calculation of the average sample size would involve finding the probabilities of rejection at a large number of rejection points; although this could be done by a matrix method, it would involve a great deal of work. It is suggested, therefore, that the limiting form given in para. 3. 2. be used as an approximation.

3. *Limiting form as* $b \to \infty$.

3. 1. *Operating characteristic.*

Suppose that $b \to \infty$ in such a way that $H_1/b = R_1$ and $H_2/b = R_2$, where R_1 and R_2 are constants. Then it clearly makes no difference in the limit whether the lattice diagram represents $H_1/b = R_1$ and $H_2/b = R_2$ or $H_1/(b + 1) = R_1$ and $H_2/(b + 1) = R_2$. We shall consider the latter case, for, as has been pointed out in para. 2, and is proved in para. 4. 1, the block B then vanishes if $R_1 + R_2$ is integral, and we need consider only A as repetitive.

Fig. 3 shows the diagram for $b = 10$, $H_1 = H_2 = 22$. In the general case for $R_1 = R_2 = 2$, we have :

$$M = ({}^{b+1}C_2 \quad {}^{b+1}C_1 \quad 1)$$

$$Y = A = \begin{pmatrix} {}^{b+1}C_1 & 1 & 0 \\ {}^{b+1}C_2 & {}^{b+1}C_1 & 1 \\ {}^{b+1}C_3 & {}^{b+1}C_2 & {}^{b+1}C_1 \end{pmatrix}$$

and $MY^n = (a_{1,n}b^{n+2} + O(b^{n+1}),\ a_{2,n}b^{n+1} + O(b^n),\ a_{3,n}b^n + O(b^{n-1}))$.

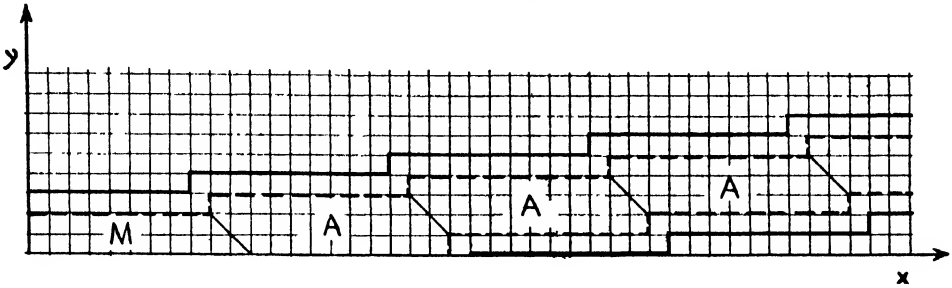

FIG. 3.

Now $a_{1,n+1} = a_{1,n} + a_{2,n}/2! + a_{3,n}/3!$, and similarly $a_{2,n+1}$, $a_{3,n+1}$ may be expressed as linear functions of $a_{1,n}$, $a_{2,n}$ and $a_{3,n}$; *i.e.*, the coefficient of the highest power of b in any element depends only on the coefficient of the highest power in elements previously obtained.

We may thus write

$$M \sim (b^2/2! \quad b \quad 1) \quad \text{and} \quad Y \sim \begin{pmatrix} b & 1 & 0 \\ b^2/2! & b & 1 \\ b^3/3! & b^2/2! & b \end{pmatrix}$$

and by successive multiplication obtain

$$MY^n \sim (a_{1,n}b^{n+2} \quad a_{2,n}b^{n+1} \quad a_{3,n}b^n).$$

The same procedure may clearly be followed for any value of R_1 and R_2, and we shall have (when R_1 and R_2 are integers),

$$M_1 \sim \left(\frac{b^{R_2}}{R_2!}, \frac{b^{R_2-1}}{(R_2-1)!}, \ldots\ldots\ldots, b, 1\right)$$

$$M_2 \sim \begin{pmatrix} b, & 1, & \ldots\ldots\ldots\ldots, & 0 \\ b^2/2! & b, & \ldots\ldots\ldots\ldots, & 0 \\ \vdots & & & \vdots \\ \frac{b^{R_2+1}}{(R_2+1)!} & \ldots & \ldots\ldots\ldots\ldots, & b \end{pmatrix}$$

$$M_m \sim \begin{pmatrix} b, & 1, & \ldots\ldots\ldots\ldots, & 0 \\ b^2/2! & b, & \ldots\ldots\ldots\ldots, & 0 \\ \vdots & & & \vdots \\ \frac{b^{R_1+R_2-2}}{(R_1+R_2-2)!} & \ldots & \ldots\ldots\ldots\ldots, & b \end{pmatrix} \qquad (m = R_1 - 1)$$

$$Y = A \sim \begin{pmatrix} b, & 1, & \ldots\ldots\ldots\ldots, & 0 \\ b^2/2\,!, & b, & \ldots\ldots\ldots\ldots, & 0 \\ \cdot & & & \cdot \\ \cdot & & & \cdot \\ \cdot & & & \cdot \\ \dfrac{b^{R_1+R_2-1}}{(R_1+R_2-1)\,!} & \ldots\ldots & \ldots\ldots\ldots\ldots, & b \end{pmatrix}$$

whence $M = M_1 M_2 \ldots M_m \sim (a_{r,0} b^{R_1+R_2-r-1})$, $\quad r = 1, 2, \ldots\ldots, (R_1 + R_2 - 1)$

and $MY^n \sim (a_{r,n} b^{n+R_1+R_2-r-1})$ $\quad r = 1, 2, \ldots\ldots, (R_1 + R_2 - 1)$

(see para. 4. 1)

(If R_2 is not an integer, we must replace R_2 by $y_1 - 1$, except when $R_1 + R_2$ occurs, and $m = R_1 - 1$ by $m = R_1 + R_2 - y_1$. See para. 4. 1.) When writing down the matrices, the reader will always find it helpful to draw the lattice diagram for the particular scheme.

In para. 4. 1 we prove that the number of paths from the origin to the point $(xb + x', y) \sim Nb^y$, where x, x', y and N are finite. (If this notation is compared with that in para. 1, it will be seen that x and N have been replaced by $xb + x'$ and Nb^y, so as to keep x and N finite.)

The probability of reaching this point is, from (1)

$$\sim Nb^y \frac{X^y}{b^y}\left(1 - \frac{X}{b}\right)^{xb+x'}, \text{ where the fraction defective} = X/b,$$

$$\rightarrow NX^y e^{-Xx}, \text{ a finite quantity.}$$

If the scheme is closed after one of blocks A—*i.e.*, at a sample size of $k(b + 1)$ where $(k - R_1)$ is integral, by a method similar to that used in the exact case the probabilities of reaching the $(k - R_1 + 1)$ acceptance points on the lower boundary . . . $(\overline{R_1 + p \quad b} + R_1, p)$, $p = 0, 1 \ldots (k - R_1)$, and the $(R_1 + R_2 - 1)$ points on the diagonal cut-off . . . $(kb - R_2 + r, k + R_2 - r)$, $r = 1, 2 \ldots (R_1 + R_2 - 1)$, may be found in the limiting form, and the operating characteristic obtained. As in the limiting form for the open scheme, the fraction defective is given as a multiple of $1/b$. For the closed scheme, the result will depend on which points of the cut-off we assign to the acceptance boundary.

3. 2. *Average sample size*

In calculating the average sample size it is necessary to take into account not only the probabilities of acceptance at a finite number of points, but also the probabilities of rejection at an infinite number of points. However, all the points on one segment of the rejection boundary, on the top of, say, the $(n + 1)$th block A, contribute to the A.S.S. a term, S_n, of order b, and a formula is given below for S_n.

If t_0 points on the diagonal cut-off are assigned to rejection, then the last t_0 segments of the rejection boundary will have to be modified. Fig. 4 will make this clear. This shows the accept-

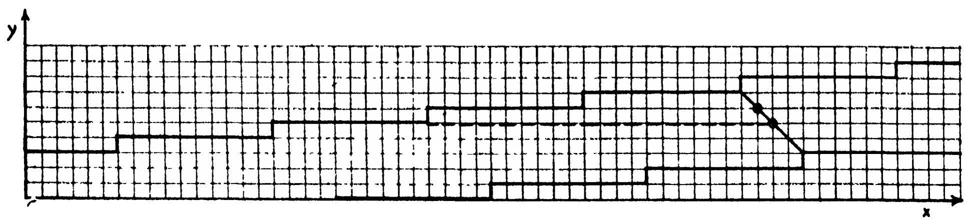

FIG. 4.

ance and rejection boundaries for $b = 10$, $H_1 = 20$, $H_2 = 24$, where the scheme is closed at a sample size of $53(b + 1)/11$ and the diagonal cut-off adds 1 extra point to the acceptance and 2 to the rejection boundaries. Now clearly the rejection boundary can be drawn along the line $y = 5$ (as shown by a dotted line), for if a path reaches this line it must eventually cross the real rejection boundary.

In general,

$$S_n \sim be^{-(R_1+n-1)X} \sum_{r=t}^{R_1+R_2} \phi_{r-t}(X) \;.\; X^{R_1+R_2+n-r-1} a'_{r,n} \quad : \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

where the modified boundary is a distance t below the original one—*i.e.*, $t = R_1 + t_o - k + n$,

$$\phi_r(X) = 1 - e^{-X}(1 + \ldots + X^r/r!),$$

$$a'_{r,n} = (R_1 + n - 1)a_{r,n} + (r - t)a_{r-1,n},$$

and $a_{r,n}$ is defined as in para. 3. 1. (Proof of (3) in para. 4. 2.)

The formula (3) may also be used for the contributions to the average sample size from the segments of the rejection boundary to the left of the first block A. If there are m irregular blocks $M_1, M_2, \ldots M_m$, before the first A (if R_1 is an integer, $m = R_1 - 1$), then $M_1 M_2 \ldots M_j$ will be a $1 \times (R_1 + R_2 - m - 1 + j)$ matrix; and the value of S_n for the segment on the top of M_{j+1} $(j = 1, \ldots m - 1)$ is given by (3) with $n = -(m - j)$ and $a_{r,n} = 0$, $r = (R_1 + R_2 + n), \ldots, (R_1 + R_2 - 1)$.

For the first part of the rejection boundary, $n = -m$, and all the a's are zero except $a_{R_1+R_2-m-1,-m}$, which equals 1, since all paths start from the origin.

Thus the average sample size in the limit as $b \longrightarrow \infty$

$$\sim \sum_{n=-m}^{k-R_1} S_n + \sum_{j=R_1}^{k} jbp_j + kb \sum_{i=1}^{R_1+R_2-t_0-1} q_i,$$

where the p_j are the probabilities of acceptance at the $(k - R_1 + 1)$ acceptance points on the lower boundary, and the q_i the probabilities of reaching the $(R_1 + R_2 - t_o - 1)$ acceptance points on the diagonal cut-off. As in the limiting form of the open scheme, the average sample size is given as a multiple of b, for a fraction defective as a multiple of $1/b$.

Note on computation of S_n.

For all modified schemes with the same upper boundary (*i.e.*, $n - t$ constant), (the index of X) + (the suffix of ϕ) is constant. Hence when such functions as $X^{R_1+R_2+n-r-1}\phi_{r-t}(X)$ have been computed once they may be used many times.

4. *Mathematical appendix.*

4. 1. *Properties of the lattice diagram.*

Fig. 5 shows the acceptance and rejection boundaries for a scheme defined by H_1, H_2 and b

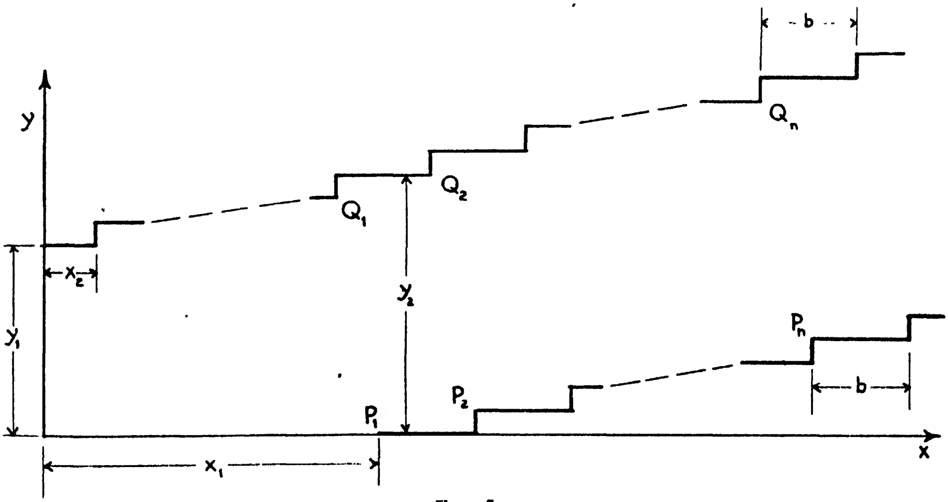

FIG. 5.

x_1, x_2, y_1, y_2 are defined as shown, and Q_2 is the first right-hand end point of a rejection segment to fall to the right of P_1.

By the definitions of H_1, H_2, b we have

$$\left.\begin{array}{ll} x_1 = H_1 & by_1 - x_2 = H_2 \\ b(y_1 - 1) < H_2 \leqslant by_1 & b(y_2 - 1) - x_1 < H_2 \leqslant by_2 - x_1 \end{array}\right\} \quad . \quad . \quad . \quad (4)$$

which give

$$\begin{array}{ll} x_1 = H_1 & x_2 = b\left\langle \dfrac{H_2}{b} \right\rangle - H_2 \\ y_1 = \left\langle \dfrac{H_2}{b} \right\rangle & y_2 = \left\langle \dfrac{H_1 + H_2}{b} \right\rangle, \end{array}$$

where $\langle x \rangle$ is defined as the smallest integer not less than x,

i.e., $\langle x \rangle = x$ if x is an integer
$= [x] + 1$ otherwise.

Putting $H_1 = R_1(b + 1)$, $H_2 = R_2(b + 1)$, we have, from (4),

$$\left.\begin{array}{ll} x_1 = R_1(b + 1) & y_1 = \left\langle \dfrac{R_2(b + 1)}{b} \right\rangle \\ x_2 = b\left\langle \dfrac{R_2(b + 1)}{b} \right\rangle - R_2(b + 1) & y_2 = \left\langle \dfrac{(R_1 + R_2)(b + 1)}{b} \right\rangle \end{array}\right\} \quad . \quad . \quad . \quad (5)$$

The block B *vanishes when* $R_1 + R_2$ *is an integer.*

For the sample size at $P_2 = x_1 + b + 1$,
and the sample size at $Q_2 = y_2 + x_2 + b$ times (the number of complete lengths b between Q_2 and the y-axis).

Since the y-co-ordinate of the boundary increases by 1 for every length b parallel to the x-axis, this number is $y_2 - y_1$.

$\therefore$ Sample size at Q_2 − Sample size at P_2

$$\begin{aligned} &= y_2 + x_2 + b(y_2 - y_1) - x_1 - b - 1 \\ &= (b + 1)\left\langle \frac{(R_1 + R_2)(b + 1)}{b} \right\rangle - (R_1 + R_2 + 1)(b + 1), \quad \text{from (5)} \\ &= (b + 1)\left(\left\langle \frac{(R_1 + R_2)(b + 1)}{b} \right\rangle - (R_1 + R_2 + 1)\right). \end{aligned}$$

The necessary and sufficient condition for the block B to vanish is that the sample size at some Q = the sample size at P_2,

i.e., that $R_1 + R_2$ is an integer.

For sample size at Q_2 = the sample size at P_2, we need also $R_1 + R_2 < b$.

Points on the diagonal P_nQ_n.

We shall consider the case where $R_1 + R_2$ is an integer and $R_1 + R_2 < b$ so that P_n, Q_n are on the same diagonal, and from (5), $y_2 = R_1 + R_2 + 1$.

The co-ordinates of P_n are $R_1(b + 1) + (n - 1)b$, $n - 1$.

The sample size is thus $(R_1 + n - 1)(b + 1)$ and the co-ordinates of Q_n are

$$(R_1 + n - 1)(b + 1) - (y_2 + n - 2), \quad y_2 + n - 2$$

i.e.,

$$(R_1 + n - 1)b - R_2, \quad R_1 + R_2 + n - 1.$$

The points on the R.H. diagonal side of the n-th block A are therefore

$$(R_1 + n - 1)b - R_2 + r,\ R_1 + R_2 + n - 1 - r, \qquad r = 1, 2, \ldots, (R_1 + R_2 - 1).$$

Number of paths to any point (ξ, η) *between the boundaries is* $O(b^\eta)$ *in* b.

(We shall need the result only for integral $R_1 + R_2$, and $R_2 \leqslant b$, for which case the proof holds).

For suppose that the number of paths to the i-th point (ξ_i, η_i) on any diagonal line through a

corner point of either boundary is $N_i b^{\eta_i} + O(b^{\eta_i - 1})$. Then the number of paths to the j-th point (ξ_j, η_j) of the next diagonal

$$= \sum_{\eta_i \leqslant \eta_j} [N_i b^{\eta_i} + O(b^{\eta_i - 1})]^{b+1} C_{\eta_j - \eta_i}$$

$$= \sum_{\eta_i \leqslant \eta_j} \left[N_i b^{\eta_i} + O(b^{\eta_i - 1})\right]\left[\frac{b^{\eta_j - \eta_i}}{(\eta_j - \eta_i)!} + O(b^{\eta_j - \eta_i - 1})\right]$$

$$= O(b^{\eta_j}).$$

Now, the number of paths to the point $(b + k - y_1 + 1, y_1 - k)$ $(1 \leqslant k \leqslant y_1)$, on the first diagonal, is simply ${}^{b+1}C_{y_1-k}$, which is $O(b^{y_1 - k})$, and the result follows by induction for all points on the diagonals.

It follows immediately that the number of paths to any point (ξ, η) between the acceptance and rejection boundaries is $O(b^{\eta})$, for the number of paths to the point (ξ, η) is a non-decreasing function of ξ for fixed η, which is $O(b^{\eta})$ for the greatest and least admissible values of ξ.

4. 2. *Proof of* (3).

The numbers of paths to the points on the line $P_n Q_n$ are given by the elements of the matrix MY^n. We shall denote these by $p_{r,n}$, $r = 1, 2, \ldots (R_1 + R_2 - 1)$.

From para. 3. 1 we have $p_{r,n} = a_{r,n} b^{n + R_1 + R_2 - r - 1}(1 + o(1))$.

We shall now consider the contribution to the average sample size of the rejection boundary between Q_n and Q_{n+1} (excluding Q_n itself), and in particular the limiting form, S_n, of this as $b \to \infty$. We shall require the following lemma :

$$\sum_{j=r}^{b} {}^jC_r \left(1 - \frac{X}{b}\right)^j \sim \frac{b^{r+1}}{X^{r+1}}\left\{1 - e^{-X}\left(1 + \ldots + \frac{X^r}{r!}\right)\right\} \quad \text{as } b \to \infty \quad . \quad . \quad (6)$$

We denote $1 - e^{-X}(1 + \ldots + X^r/r!)$ by $\phi_r(X)$, and notice first, by using Leibnitz's theorem on the repeated differentiation of the product of two functions, that

$$\frac{\phi_r(X)}{X^{r+1}} = \left(\frac{d}{dX}\right)^r \left\{\frac{1 - e^{-X}}{X}\right\} \frac{(-1)^r}{r!}.$$

Now $$\sum_{j=0}^{b} \left(1 - \frac{X}{b}\right)^j = \frac{b}{X}\left(1 - \left(1 - \frac{X}{b}\right)^{b+1}\right), \text{ (Geometrical Progression).}$$

We differentiate r times w.r.t. X and multiply by $\dfrac{b^r(-1)^r(1 - X/b)^r}{r!}$.

Then $$\sum_{j=r}^{b} {}^jC_r \left(1 - \frac{X}{b}\right)^j = \frac{b^{r+1}(-1)^r}{r!}\left(1 - \frac{X}{b}\right)^r \left(\frac{d}{dX}\right)^r \left\{\frac{1 - (1 - X/b)^{b+1}}{X}\right\}.$$

We therefore have to show that

$$\operatorname*{Lt.}_{b \to \infty} \left(\frac{d}{dX}\right)^r \left\{\frac{1 - (1 - X/b)^{b+1}}{X}\right\} = \left(\frac{d}{dX}\right)^r \left\{\frac{1 - e^{-X}}{X}\right\}.$$

But $$\frac{1 - (1 - X/b)^{b+1}}{X} = \frac{1 - e^{-X}}{X} + \frac{P(X)}{b} \quad \text{where } P(X) = \left\{e^{-X} - \left(1 - \frac{X}{b}\right)^{b+1}\right\}\frac{b}{X}$$

can be expanded as a power series in X with coefficients of $O(b^0)$ and radius of convergence b. Hence for $|X| \leqslant b' < b$ it is bounded, and (6) follows.

We may now calculate S_n. For greater generality we consider the modified form of the scheme in which we reject, not on the line $y = R_1 + R_2 + n$, but on $y = R_1 + R_2 + n - t$.

Our rejection points are therefore

$$(R_1 + n - 1)b - R_2 + j, \quad R_1 + R_2 + n - t \quad (1 \leqslant j \leqslant b).$$

The number of admissible paths to this point from the diagonal point

$$(R_1 + n - 1)b - R_2 + r, \quad R_1 + R_2 + n - 1 - r \text{ is } {}^{(j-r)+(r-t)}C_{r-t}$$

(since we must pass through $(R_1 + n - 1)b - R_2 + j$, $R_1 + R_2 + n - t - 1$), and the contribution to the average sample size is

$$\sum_{r=t}^{R_1 + R_2 - 1} p_{r,n} \left(1 - \frac{X}{b}\right)^{(R_1 + n - 1)b - R_2 + j} \left(\frac{X}{b}\right)^{R_1 + R_2 + n - t} {}^{j-t}C_{r-t} \left\{(R_1 + n - 1)b + R_1 + n + j - t\right\}.$$

Summing over j, we have

$$\sum_{r=t}^{R_1+R_2-1} p_{r,n}\left(1-\frac{X}{b}\right)^{(R_1+n-1)b-R_2+t}\left(\frac{X}{b}\right)^{R_1+R_2+n-t}\sum_{j=r}^{b} {}^{j-t}C_{r-t}\left(1-\frac{X}{b}\right)^{j-t}\left\{(R_1+n-1)(b+1)+j-t+1\right\}.$$

The summation over j is

$$\sum_{j=r}^{b}\left(1-\frac{X}{b}\right)^{j-t}\left\{(R_1+n-1)(b+1)\,{}^{j-t}C_{r-t}+(r-t+1)\,{}^{j-t+1}C_{r-t+1}\right\},$$

which as $b \rightarrow \infty$, is, by (6), asymptotically equal to

$$(R_1+n-1)\frac{b^{r-t+2}}{X^{r-t+1}}\phi_{r-t}(X)+(r-t+1)\frac{b^{r-t+2}}{X^{r-t+2}}\phi_{r-t+1}(X).$$

Hence the contribution to the average sample size is asymptotically equal to

$$\sum_{r=t}^{R_1+R_2-1} p_{r,n}e^{-(R_1+n-1)X}\cdot\frac{X^{R_1+R_2+n-t}}{b^{R_1+R_2+n-r-2}}\left\{\frac{(R_1+n-1)\phi_{r-t}(X)}{X^{r-t+1}}+\frac{(r-t+1)\phi_{r-t+1}(X)}{X^{r-t+2}}\right\},$$

which, since $p_{r,n} \sim a_{r,n}b^{n+R_1+R_2-r-1}$, is asymptotically equal to S_n, where

$$S_n = be^{-(R_1+n-1)X}\sum^{R_1+R_2-1} a_{r,n}\,.\,X^{R_1+R_2+n-t}\left\{\frac{(R_1+n-1)\phi_{r-t}(X)}{X^{r-t+1}}+\frac{(r-t+1)\phi_{r-t+1}(X)}{X^{r-t+2}}\right\}$$

$$= be^{-(R_1+n-1)X}\sum_{r=t}^{R_1+R_2}\phi_{r-t}(X)\,.\,X^{R_1+R_2+n-r-1}\left\{(R_1+n-1)a_{r,n}+(r-t)a_{r-1,n}\right\}.$$

Defining $a'_{r,n}$ as $(R_1+n-1)a_{r,n}+(r-t)a_{r-1,n}$, $(t \leqslant r \leqslant R_1+R_2)$, we have

$$S_n = be^{-(R_1+n-1)X}\sum_{r=t}^{R_1+R_2}\phi_{r-t}(X)\,.\,X^{R_1+R_2+n-r-1}a'_{r,n}.$$

Acknowledgments.

Since this paper was completed, the authors' attention has been drawn to a paper by Bartky,[3] in which a very similar matrix method is introduced. Bartky obtains the operating characteristic and average sample size curve for an open scheme defined by H_1, H_2 and b (in our notation), with the restriction that $(H_1 + H_2)/(b + 1)$ is an integer, so that sampling is carried out in blocks of size $(b + 1)$. This makes no difference to the operating characteristic, but does affect the average sample size owing to the difference in the rejection boundary. The solution involves an inverse matrix, for the evaluation of which an approximation is given. Although Bartky considers only open schemes, a solution for closed schemes is implicit in his method. He uses, however, a purely algebraic approach, and apart from our use of the limiting forms, which enables us to deal with the average sample size when sampling is in single units, we feel that the geometrical approach is in many ways easier to grasp.

The matrix method was first suggested to the authors by Mr. G. A. Barnard, who encouraged much of the early development of the subject. Our thanks are also given to Mr. F. J. Anscombe for encouragement and advice throughout the writing of this paper, and in particular for the idea of considering the limiting forms when b is large; and to Mr. H. J. Godwin for very useful help in simplifying some nomenclature and making rigorous corrections in some of the proofs.

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A Modified Probit Technique for Small Probabilities.

By M. S. Bartlett.

1. The probit method of statistical analysis has been most comprehensively described by Bliss [1, 2] in connection with the analysis of toxicity data; but it is of course a general method, with applications in other fields. In one problem, where the relation with temperature of an occasional but serious failure in a certain type of armament was under investigation, it was found convenient to introduce probit technique, but in a modified form. This method, described below, is of general application to cases where the range of values of the variable under consideration (temperature, dosage, etc.) is of most interest in the region of small probabilities. By an obvious symmetry in the theory between small probabilities and high ones, the method is equally available in appropriate instances when the dosage * required for a high percentage kill, 99 or 99·9 per cent., is being estimated.

The principle is to replace the direct method of sampling by the inverse one, in which the number of occurrences determine the size of sample. Such inversion, which is of increasing application in sampling problems,† is of course only possible in any instance if the observational units or " individuals " can conveniently be sampled one by one.

The practical procedure recommended was to choose a dosage round about the 50 per cent. point sample until a given number of " survivors " have occurred (two was adopted as a convenient number), increase the dosage by a suitable interval, repeat the sampling, and so on until sufficient dosages have been tested or the individuals set aside for the experiment have all been treated.

The advantage of this method is that individuals are not wasted at dosages which are irrelevant to the purpose of the experiment, the sampling rapidly entering the region of small probabilites. For example, in an artificial sampling experiment starting at the 50 per cent. point ($p = \frac{1}{2}$), and with intervals equal to $\frac{1}{2}\sigma = \frac{1}{2}/\beta$, where σ is the standard deviation of the underlying normal distribution and β the true slope of the probit-dosage regression line, the distribution of 500 individuals shown in Table I was obtained (using random sampling numbers).

Table I.

Results of sampling experiment.

Deviation	Probability	Number of individuals	Number of survivors
0	0·500	3	2
$\frac{1}{2}\sigma$	0·309	10	2
1σ	0·159	7	2
$1\frac{1}{2}\sigma$	0·067	18	2
2σ	0·023	90	2
$2\frac{1}{2}\sigma$	0·0062	372	1+
		500	

In practice we do not of course know either the 50 per cent. point or σ, but this does not affect the principle of the method; the only practical point is that if the intervals happen to prove rather too large, it might be advisable to interpolate one or two points before using up all the individuals at our disposal.

* It is convenient to use this terminology, provided it is understood that the method is not confined to toxicological data.

† See, for example, the correspondence in *Nature* by Barnard, G. A., Case, R. A. M., Haldane, J. B. S., and Tweedie, M. C. K. (vol. 155, pp. 49 and 453, vol. 156, pp. 115 and 208), the last-named proposing the adjective " inverse."

2. The appropriate analysis for data obtained by this modified technique is next derived. If an event with probability of occurrence p first occurs at the n-th trial, its likelihood is

$$p(1-p)^{n-1}$$

and its logarithm is

$$L = \log p + (n-1)\log(1-p)$$

whence

$$\frac{\partial L}{\partial p} = \frac{1}{p} - \frac{n-1}{1-p},$$

$$E\left(-\frac{\partial^2 L}{\partial p^2}\right) = \frac{1}{p^2 q} = I(p), \text{ say, } (q = 1-p),$$

since $E(n) = 1/p$.

For the case of the second occurrence at the n-th trial, we have

$$\frac{\partial L}{\partial p} = \frac{2-np}{pq}$$

$$\cdot\, I_2(p) = \frac{2}{p^2 q}$$

TABLE II.

Modified probit analysis.

(Weighting coefficients, etc., in the case of two occurrences)

Expected probit	Percentage probability, $100p$	Factor p/z	Weighting coefficient, $2z^2/p^2q$	Expected probit	Percentage probability, $100p$	Factor p/z	Weighting coefficient, $2z^2/p^2q$
1·1	0·00481	0·2421	34·1	4·6	34·458	0·9357	3·5
1·2	0·00723	0·2478	32·6	4·7	38·209	1·0018	3·2
1·3	0·0108	0·2538	31·1	4·8	42·074	1·0759	3·0
1·4	0·00159	0·2600	29·6	4·9	46·017	1·1593	2·8
1·5	0·0233	0·2665	28·1	5·0	50·000	1·2533	2·5
1·6	0·0337	0·2734	26·8	5·1	53·983	1·360	2·3
1·7	0·0483	0·2806	25·4	5·2	57·926	1·481	2·2
1·8	0·0687	0·2882	24·1	5·3	61·791	1·620	2·0
1·9	0·0968	0·2962	22·8	5·4	65·542	1·780	1·8
2·0	0·135	0·3046	21·6	5·5	69·146	1·964	1·6
2·1	0·187	0·3134	20·4	5·6	72·575	2·178	1·5
2·2	0·256	0·3228	19·2	5·7	75·804	2·428	1·4
2·3	0·347	0·3327	18·1	5·8	78·814	2·721	1·3
2·4	0·466	0·3432	17·1	5·9	81·594	3·067	1·1
2·5	0·621	0·3543	16·0	6·0	84·134	3·477	1·0
2·6	0·820	0·3660	15·0	6·1	86·433	3·968	0·9
2·7	1·072	0·3786	14·1	6·2	88·493	4·557	0·8
2·8	1·390	0·3919	13·2	6·3	90·320	5·271	0·7
2·9	1·786	0·4062	12·3	6·4	91·924	6·139	0·7
3·0	2·275	0·4214	11·5	6·5	93·319	7·205	0·6
3·1	2·872	0·4376	10·7	6·6	94·520	8·521	0·5
3·2	3·593	0·4551	10·0	6·7	95·543	10·159	0·4
3·3	4·457	0·4739	9·3	6·8	96·407	12·211	0·4
3·4	5·480	0·4940	8·7	6·9	97·128	14·899	0·3
3·5	6·681	0·5158	8·1	7·0	97·725	18·101	0·3
3·6	8·076	0·5394	7·5	7·1	98·214	22·330	0·2
3·7	9·680	0·5649	6·9	7·2	98·610	27·797	0·2
3·8	11·507	0·5926	6·4	7·3	98·928	32·945	0·2
3·9	13·567	0·6227	6·0	7·4	99·180	44·288	0·1
4·0	15·866	0·6557	5·5	7·5	99·379	56·696	0·1
4·1	18·406	0·6917	5·1	7·6	99·534	73·277	0·1
4·2	21·186	0·7313	4·7	7·7	99·653	95·627	0·1
4·3	24·196	0·7749	4·4	7·8	99·744	—	0·0
4·4	27·425	0·8230	4·1	7·9	99·813	—	0·0
4·5	30·854	0·8764	3·8	8·0	99·865	—	0·0

This case is most relevant here, and is treated in detail. The corresponding information function $I_2(Y)$ on the probit value Y is given by

$$\left.\begin{aligned}\frac{\partial L}{\partial Y} &= \frac{(2-np)z}{pq}\\ I_2(Y) &= \frac{2z^2}{p^2q}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where z is the Gaussian ordinate corresponding to the probit value Y. The method of fitting is to use the estimates $\hat{p} = 2/n$, the corresponding weighting coefficients obtained from the provisional probit line being given by $I_2(Y)$, tabulated in Table II. As $p \to 0$, $I_2(Y) \to 2(5 - Y)^2$. Strictly the sampling should be completed at each dosage, but if owing to the restriction on total number of individuals available, it remains incomplete, little error is introduced if for the last point the standard probit theory and weighting coefficient (ref. 1 or 3) is used (this procedure would be exact if prior to the sampling at this dosage it had been decided to use up all the remaining individuals at the same dosage).

In a more precise analysis the provisional regression line would also be used to obtain adjusted probit values for all the observations, those corresponding to two occurrences being substituted by y, where (*cf.* ref. 1, p. 164)

$$\frac{(2-np)z}{pq} = \frac{(y-Y)2z^2}{p^2q}$$

or

$$y = Y + p(1 - \tfrac{1}{2}np)/z \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The final estimation of the probit line proceeds as usual, whence the fiducial limits for an x corresponding to a given Y are given by (ref. 2, p. 325)

$$x = \bar{x} + \frac{b(Y-\bar{y}) \pm t\sqrt{\{\hat{\sigma}_b^2(Y-\bar{y})^2 + \hat{\sigma}_{\bar{y}}^2(b^2 - t^2\hat{\sigma}_b^2)\}}}{b^2 - t^2\hat{\sigma}_b^2} \quad . \quad . \quad . \quad . \quad (3)$$

in the usual notation, $\hat{\sigma}_b^2$ and $\hat{\sigma}_{\bar{y}}^2$ being the estimated variances of b and $\bar{y}$.

In the original problem, the value of x corresponding to a "survival probability" of 1 in 500 was required—*i.e.*, to the value $Y = 2{\cdot}1218$.

3. It may be helpful to illustrate the analysis by means of the sampling data of Table I. The details are shown below, the analysis being similar to the standard analysis except that for all points up to the last the weighting coefficients (corresponding to the probits obtained from a provisional line drawn by eye) are taken from Table II. For convenience of computation the unit of x is $\frac{1}{2}\sigma$.

x	y	w	wx	wy
0	5·43	1·8	—	9·774
1	4·16	3·0	3·0	12·480
2	4·43	4·5	9·0	19·935
3	3·78	7·2	21·6	27·216
4	2·99	11·1	44·4	33·189
5	2·22	19·4	97·0	43·068
—	—	47·0	175·0	145·662

wx^2	wxy	wy^2
748·40	482·094	491·024
651·60	542·359	451·434
96·80	−60·265	39·590
		37·519
		$\chi^2 = 2{\cdot}071$ (4 d.f.)

Calculated probit line : $Y - 3{\cdot}0992 = -0{\cdot}62257\ (x - 3{\cdot}7234)$ or $Y = -0{\cdot}6226x + 5{\cdot}417$. Since χ^2 is less than expectation, we may justifiably assume the data are homogeneous, so that $\sigma_{\bar{y}}^2 = 1/\Sigma w = 0{\cdot}0213$, $\sigma_b^2 = 1/\Sigma w(x - x)^2 = 0{\cdot}0103$. Further, for a 90 per cent. fiducial interval for x, we may take t from the normal distribution—*i.e.*, $t = 1{\cdot}645$. Alternatively, we obtain from (3) a single estimate for x by putting $t = 0$. The values corresponding to $Y = 2{\cdot}1218$ are found to be $x = 5{\cdot}29$ (90 per cent. fiducial interval $6{\cdot}02 - 4{\cdot}81$). Since in this illustrative example we know the right answer, we can compare it with these estimates. In fact, since x is in units of $\frac{1}{2}\sigma$, we know that the correct equation should be $Y = 5 - \frac{1}{2}x$, so that corresponding to the above estimated values of x the values of Y are given by

$$Y = 2{\cdot}35 \text{ (90 per cent. fiducial interval } 1{\cdot}99 - 2{\cdot}59)$$

whereas the correct value, corresponding to $p = 0{\cdot}002$, is $2{\cdot}12$. If, as is usual in this type of problem, we wish to make reasonably sure we are not underestimating the value of x corresponding to a given Y, we may take the appropriate 5 per cent. (one tail) fiducial limit, which in this example is $6{\cdot}02$. The corresponding value $1{\cdot}99$ for Y would actually give a survival rate of $0{\cdot}00135$, compared with the stipulated maximum rate of $0{\cdot}002$.

4. The formal accuracy of this method in comparison with standard probit technique can be investigated by means of the reciprocals of the asymptotic sampling variances of x for the two cases—*i.e.*, on the assumption that the probit line is linear, by

$$I(x) = \frac{1}{\sigma^2}\left\{\frac{1}{\Sigma w_r} + \frac{(x - \bar{x})^2}{\Sigma w_r(x_r - \bar{x})^2}\right\}^{-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

For convenience the scale of x is in this section chosen to be $x = \sigma(Y - 5)$ and for the standard method we consider a symmetrical arrangement for which $\bar{x} = 0$. Since $I(x)$, then, tends to vary directly with $\Sigma w_r x_r^2$ for large x, and the function $wx^2 = z^2x^2/(pq)$ is stationary round about $x = 1\frac{1}{2}\sigma$, we exhibit the standard method to best advantage if we arrange for observations to be at, say, $x = 0, \pm\sigma, \pm 2\sigma$ (this will not, of course, be exactly realized in practice when the probit line is unknown). For this arrangement, the function $I(x)$ for $5 \times n$ observations is given by

$$I(x) = \frac{n}{\sigma^2}\{0{\cdot}564 + 0{\cdot}520\, x^2\}^{-1}$$

or per observation by

$$I_0(x) = \frac{1}{\sigma^2}\{2{\cdot}82 + 2{\cdot}60\, x^2\}^{-1}$$

For the alternative method the theoretical scheme corresponding to Table I was considered. Again in practice this would not be exactly realized, but provided a reasonable number of observations are arranged, the accuracy will be comparable. The maximum value of x reached by this method will depend on the total number of individuals available, and three stages are considered (1) when sampling for the value $x = 2\sigma$ is completed (two survivors), the expected value for the total number N of individuals being in this case 141, (2) for sampling up to $x = 2\frac{1}{2}\sigma$, $E(N) = 463$, (3) for sampling up to $x = 3\sigma$, $E(N) = 1944$. It will be noticed $E(N)$ in this last case far exceeds the actual number $N = 500$ used in the original problem and illustrated in Table I. It is included here mainly for theoretical comparison with cases (1) and (2).

Using the weighting coefficients given in Table II, we find $\bar{x} = 1{\cdot}35\ \sigma$, $1{\cdot}74\ \sigma$ and $2{\cdot}13\ \sigma$ in the three cases. The information function $I(x)$ is calculated from (4); this represents the information for the whole theoretical experiment, for which on the average $E(N)$ observations are made. For comparison with $\sigma^2 I_0(x)$ by the standard method, the value $\sigma^2 I(x)/E(N)$ is shown in Table III.

TABLE III.

Comparison of $\sigma^2 I_0(x)$ *and* $\sigma^2 I(x)/E(N)$.

	Standard method	Modified method		
		(1)	(2)	(3)
$x = 2\sigma$	0·076	0·110	0·092	0·035
$x = 2\frac{1}{2}\sigma$	0·053	0·054	0·051	0·030
$x = 3\sigma$	0·038	0·030	0·027	0·018

In the interpretation of these figures considerable caution is advised. The immediate conclusions are:—

(*a*) on the assumptions made, including that of linearity of the probit line over the whole range of x, the standard and modified schemes (cases (1) and (2)) are comparable in accuracy. They appear equivalent in accuracy at $x = 2\frac{1}{2}\sigma$, at $x = 2\sigma$ the standard scheme appears somewhat worse, and at $x = 3\sigma$ somewhat better.

(*b*) there appears (case (3)) no point in using further individuals (over, say, the 500 used in the original experiment) to attempt to explore directly the region $x > 3\sigma$ and over.

To these conclusions must be added, however, the following comment. It has been pointed out that the dominant contribution to the error for large x arises in the standard method from the second term, which corresponds to the error in the regression coefficient b; this is determined in the representative standard scheme over the range $x = -2\sigma$ to 2σ. While in the original problem evidence available indicated that the probit line was linear over a sufficient range of x in the relevant region, any unwarranted extension of this assumption was to be avoided. While the values listed in Table III for $x = 2\frac{1}{2}\sigma$ are equal, the contribution to the total variance of an estimated x due to the error in the coefficient b is much less in the modified than in the standard method, in spite of the actual formal error in determining b being greater. This implies that the modified technique will be less sensitive to moderate departures from linearity, as well as having the advantage that b is determined from observations nearer the region of interest.

These advantages are summed up in the commonsense view which first dictated the form of the proposed procedure. To estimate an x for given small p, we require knowledge on the location and slope of the probit line in the relevant range of x. The proposed method accumulates information on *both* these parameters as nearly as possible in the relevant range, with a minimum of extrapolation from other ranges.

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THE ANALYSIS OF A SERIES OF EXPERIMENTS BY THE USE OF PUNCHED CARDS.

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Introduction.

The 2^n type of experiment is of great practical utility and the design and analysis of this type of experiment have been fully described by Yates.[1] The analysis consists of the evaluation of $2^n - 1$ treatment effects, each effect being the difference between the means of two sets of 2^{n-1} plots, and Yates gives a method of obtaining these effects by a process of continued additions and subtractions. This method is extremely convenient for a single experiment, but becomes laborious when a large number of experiments have to be analysed. A method of analysis by the use of punched cards was devised and tested out on a series of 2^5 experiments on the manurial requirements of sugar-beet carried out under the direction of Rothamsted Experimental Station. The present paper describes the method applied to that series of experiments.

The experiments.

At each centre the experiment was of the standard $2 \times 2 \times 2 \times 2 \times 2$ factorial type, the treatments consisting of all combinations of the following five factors:—

nitrogen: nil or 0·8 cwt. N as sulphate of ammonia per acre (n),
phosphate: nil or 1·0 cwt. P_2O_5 as superphosphate per acre (p),
potash: nil or 1·2 cwt. K_2O as muriate of potash per acre (k),
salt: nil or 5·0 cwt. agricultural salt per acre (s),
boron: nil or 20 lbs. borax per acre (b).

Only one replicate of the 32 combinations of the 5 factors was used. The plots were arranged in 4 blocks of 8 plots, confounding 3 high-order interactions. The observations carried out on each plot which required statistical analysis were:—

weight of dirty roots,
tare factor,
sugar percentage,
weight of tops,
plant number,
purity,
noxious nitrogen.

Analyses were made of these seven sets of observations, together with two sets of derived results—the weight of clean roots and total weight of sugar. As there were 21 experiments in all, a total of 189 analyses had to be carried out.

The usual method of analysing experiments of this type is to evaluate by repeated additions and subtractions all the 31 treatment effects, each treatment effect being the difference between the averages of two sets of 16 plots. The treatment effect of n, for example, is the difference between the mean of the 16 plots with n and the mean of the plots without n and is called the N effect. Symbolically the effects may be written in the following way:—

$$N = \tfrac{1}{16}(n-1)(p+1)(k+1)(s+1)(b+1),$$
$$NP = \tfrac{1}{16}(n-1)(p-1)(k+1)(s+1)(b+1),$$

and so on to

$$NPKSB = \tfrac{1}{16}(n-1)(p-1)(k-1)(s-1)(b-1),$$

where the right-hand side, when expanded, gives the particular combination of the plot yields for each effect, 1 being the plot receiving none of the treatments, n the plot receiving sulphate of ammonia only and so on.

The treatment effects confounded with blocks were NSB, PKB, and $NPKS$, and treatment effects NPB, NKB, $NPKB$, PSB, $NPSB$, KSB, $NKSB$, $PKSB$ and $NPKSB$ were used to estimate

the experimental error. The partition of the degrees of freedom in the analysis of variance in each of the experiments was therefore as follows:—

	D.F.
Blocks	3
Treatments	19
Error	9
Total	31

Since, however, each of the 31 degrees of freedom corresponds to a treatment effect, it was not necessary to carry out the formal analysis of variance.

Hollerith cards and machines.

The usual 80-column card was used. Each column has twelve positions called, *Y*, *X*, 0, 1, 2, 3, 4, 5, 6, 7, 8, 9. In numerical work, the " *Y* " and " *X* " positions, which lie above the " 0 " position, are not generally used, except for special purposes, such as controlling machine processes or to convey qualitative information. The *X* and *Y* positions are principally used to enable any letter of the alphabet to be punched in one column by a two-hole code.

The machines used in the analysis were the sorter, reproducer, multiplying punch and senior rolling total tabulator. As its name implies, the sorter is used to separate cards according to what is punched in any one desired column; the theoretical speed of sorting is 24,000 cards per hour, but on the average a good working speed is about 20,000 cards per hour. The function of the reproducer is to punch information from cards in other cards with the same designation in pre-assigned columns, and the maximum speed for this type of work is 6,000 cards per hour. The reproducer can also be used as a summary punch to punch on cards information obtained by a tabulator. The multiplying punch multiplies a numerical field on the card by another field on the same or a different card, and punches the product on the card after rounding it off to the desired number of figures. The greatest possible number of digits in the multiplier and the multiplicand is 8, and as many digits as are required of the product may be punched on the card.

The function of the tabulator is to add up numerical fields obtaining totals for groups into which the cards have been sorted. The latter is effected by what is known as control. The tabulator is plugged to read the cards on the columns by which the cards are classified into groups: this information is read from the card at one cycle before the field on the card is actually tabulated, and it is compared with the information on the card then being tabulated; if the information is the same, the process is continued, but if not, the card feed stops, and whatever totals the machine is directed to take at the end of a card group are taken and dealt with as required. The possible things to be done with the totals are that they be printed and/or taken (technically known as " rolled ") to other counters, added or subtracted. The tabulator has distributors which enable the numerical field on the card to be taken to one of several counters, according to the information punched in another part of the card, or which can be used for treating totals differently according to their designations. The particular operations performed by the reproducer, multiplying punch and tabulator are controlled by switches and removable plugboards: for example, on the tabulator there are three separate plugboards and a large number of switches, and when these are set correctly all that is required is to feed in and take out the punched cards and to keep the machine supplied with paper. The larger tabulators have 6 counters of 9 or 11 wheels. More detailed descriptions of some of the Hollerith machines are given by Comrie,[2] and Comrie, Hey and Hudson.[3]

General description of the method.

After preparation of the data involving multiplications, conversions from plot units to yields per acre and so on, the treatment effects are tabulated. The treatment effects are obtained by adding or subtracting each plot value, with division by a factor of 16 at some stage. Each plot yield occurs in each treatment effect either with a positive or negative sign, and this information is punched on the card. In all 31 ($= 2^5 - 1$) columns on the card were used for this purpose. When tabulating a particular treatment effect, the cards are fed through the tabulator, and the numerical field being treated is led, according to whether the distribution column indicates a positive or negative sign, into one of two counters—the counter accumulating the positive contributions or that

accumulating the negative contributions. At the end of the group of cards—that is, of the 32 cards of a centre—the amount in the negative counter is subtracted from that in the positive counter and the result printed, with a symbol if negative. As the treatment effects were to be analysed further, they were punched on cards: this could have been done automatically by linking the summary punch to the tabulator, but in this analysis it was not convenient to do so.

The analysis falls into the following well-defined stages:—

(1) preparation of the raw data for punching, punching and checking of punching,

(2) operations to be carried out on the raw data by the multiplying punch,

(3) the analysis of original and derived data and the preparation of summary cards containing the treatment effects,

(4) further analyses on the summary cards.

As the object of this paper is to give an example of the technique of the use of punched cards, the actual analysis will be described in fair detail.

The original data and their allocation to the cards.

The original data are listed below, together with the columns which they occupied.

Reference		No. of columns	Actual columns
	Centre	2	56–57
	Plot number	2	58–59
$a1$	Weight of dirty roots in lbs. per plot	4	60–63
$a2$	Tare factor	2	64–65
$a3$	Sugar percentage	3	66–68
$a4$	Weight of tops in lbs per plot	3	69–71
$a5$	Plant number	3	72–74
$a6$	Purity	3	75–77
$a7$	Noxious nitrogen	3	78-80

A two-column code was used for each centre, one column giving its soil group, and the second its reference within a soil group. A two-column code from 01 to 32 was used for the plot number, 01 being the plot receiving no treatment, 02 the plot receiving n, 03 the plot receiving p, and so on in the standard order, the plot receiving n, p, k, s and b having the code number 32. It will be noted that the data were punched at the right end of the card: this is always done when the card is not completely filled, to save time in punching. To ensure complete accuracy of punching, in addition to verification the punched data were tabulated to give totals for each centre of all the numerical fields which were checked against totals obtained by hand.

Use of the multiplying punch.

A number of multiplications and conversions were made by the multiplying punch. In all cases the conversions were carried out to give the results in terms of one-sixteenth of an acre, so that the tabulator would produce the effects in the units finally required, with the exception that the mean would be given as twice its correct value. The multiplications are set out in the table below; this table gives the following information:—

(1) reference of item obtained,

(2) name of item,

(3) its method of construction in terms of the data available (C_1, C_2, C_3 and C_4 being conversion factors from lbs. or number per plot to tons or number in thousands per $\frac{1}{16}$ acre and C_5 being $\frac{1}{16}$),

(4) whether the multiplication is such that the multipliers are obtained from each detail card separately (individual multiplication, denoted by I) or from a master card which gives the multiplier for a group of detail cards (group multiplication denoted by G),

(5) the number of columns required for the punching of the result of the multiplication.

A further instruction was necessary to indicate at which digit in each product rounding-off should take place. On this machine rounding-off is accomplished by what is known as the "$\frac{1}{2}$

Reference	Item	How obtained	Type of multiplication	No. of columns required	Position of product. actual columns
A 1	Dirty roots in tons per $\frac{1}{16}$th acre	$(a1) \times C_1$	G	4	1–4
A 2	Clean roots in lbs. per plot	$(a1) \times (a2)$	I	4	5–8
A 3	Clean roots in tons per $\frac{1}{16}$th acre	$(A2) \times C_1$	G	4	9–12
A 4	Total sugar in lbs. per plot	$(a3) \times A_2$	I	4	13–16
A 5	Total sugar in cwt. per $\frac{1}{16}$th acre	$(A4) \times C_2$	G	4	17–20
A 6	Tops in tons per $\frac{1}{16}$th acre	$(a4) \times C_3$	G	4	21–24
A 7	Plant number in thousands per $\frac{1}{16}$th acre	$(a5) \times C_4$	G	4	25–28
A 8	Tare factor by $\frac{1}{16}$	$(a2) \times C_6$	G	3	29–31
A 9	Sugar percentage by $\frac{1}{16}$	$(a3) \times C_5$	G	3	32–34
A10	Purity by $\frac{1}{16}$	$(a6) \times C_5$	G	3	35–37
A11	Noxious nitrogen by $\frac{1}{16}$	$(a7) \times C_5$	G	3	38–40

pick up ": when it is desired to round off the product at a certain position, 5 is added to the number at one position to the right before reading off the product to be punched. In the case of the group multiplications, a check was obtained by comparing the sum of the products with the product of the multiplier and the sum of the multiplicands: individual multiplications were checked by repeating the multiplication on a different machine and checking the cards for double punching. The speed of the multiplying punch depends on the number of digits in the multiplier: with a multiplier of 4 digits it is about 1000 cards per hour, and this was approximately realized.

Tabulation of the treatment effects.

Prior to the tabulations of the effects it was thought advisable to reproduce the material actually to be analysed on to new cards, as a further 31 columns are required on the card for the information giving the distribution. It would have been possible to use the X and Y positions of columns occupied by numerical fields for the distribution of these fields, but as some of the fields were obtained by a long series of multiplications, and cards are occasionally (though comparatively rarely) torn up by the machines through faulty handling, the relevant information was put on new cards. The reproduction was made as follows:

Item	New card columns
Centre	33, 34
Plot designation	35, 36
Dirty roots, tons/acre	37–40
Clean roots, tons/acre	41–44
Total sugar, cwt./acre	45–48
Tops, tons/acre	49–52
Plant number, thous./acre	53–56
Tare factor	57–59
Sugar percentage	60–62
Purity	63–65
Noxious nitrogen	66–68

The information giving the distribution of the plot values for the treatment effects was then punched on the cards: all cards were punched " 1 " in the first column and in columns 2 to 32 were punched " 1 " or " 0," according to whether the contribution of the plot to the effect was positive or negative. The distribution columns were as shown on the following page.

The actual information gang-punched on columns 2 to 32 of the detail cards is easily obtained by examining the expressions for the treatment effects in terms of the original plot yields. Thus, cards of plots which received both n and p or neither had " 1 " in column 4 and the others had " 0."

The treatment effects.

The 32 treatment effects were obtained by running the cards through the tabulator, using

Effect	Column	Effect	Column
Mean (× 2)	1	*B*	17
N	2	*NB*	18
P	3	*PB*	19
NP	4	*NPB*	20
K	5	*KB*	21
NK	6	*NKB*	22
PK	7	*PKB*	23
NPK	8	*NPKB*	24
S	9	*SB*	25
NS	10	*NSB*	26
PS	11	*PSB*	27
NPS	12	*NPSB*	28
KS	13	*KSB*	29
NKS	14	*NKSB*	30
PKS	15	*PKSB*	31
NPKS	16	*NPKSB*	32

individual card distribution on the relevant columns. For example, the cards were first sorted to columns 33, 34 (centre), on which also control was effected: the *N* effect was obtained by distributing on column 2, the fields of cards with " 0 " in column 2 going into counter number one, say, and those with " 1 " in column 2 going into counter number two. At the end of the group of cards (32 in number) for each centre, when the control on columns 33 and 34 operated, counter one contained the negative part of the treatment effect and counter two the positive part. The number in counter one was then rolled into counter two to obtain a grand total as a check. On the next cycle the number in counter one was rolled into itself, and on the following cycle the resulting number in counter one was subtracted from that in counter two. The result in counter two was the treatment effect, which was printed as a true balance—that is, the actual number if positive, and the actual number and not its complement, together with a symbol, if negative. All the instructions to the machine were permanently set up on its plugboards with two exceptions:—

(1) the fields on the card to be added and distributed,
(2) the column on which the cards are to be distributed.

When the plugging of (1) above had been done, the cards were run through a total of 32 times, distributing on column 1 on the first run, on column 2 on the second run, and so on. In all, 9 fields had to be added and distributed, and as two fields could be tabulated in one run, five multiple runs (1 multiple run equals 32 single runs) were required.

A part of the result of a typical run is given below (the *S* effect on total sugar and tops at each of the centres):—

11	5816	21668	
	254*	684*	16161616
21	7472	26282	
	272	1040	16161616
22	3360	15762	
	206	140*	16161616
23	5695	22483	
	645	1111	16161616
24	6944	25986	
	120	738	16161616

The first column gives the code of the centre, the second gives for total sugar twice the mean (a check), and the particular effect for each centre to the nearest hundredth of a cwt., and the third column gives the same results for tops to the nearest thousandth of a ton. An asterisk denotes a negative effect. The fourth column is entirely a check column, indicating that the distribution has been carried out correctly.

As the treatment effects are obtained for each centre, the number of cards in each card group is 32. The time for each group of 32 cards was as follows:—

0·6 sec. for listing and tabulating the first card
12·4 secs. for tabulating remaining 31 cards
1·2 secs. for rolling operations
1·2 secs. for printing
0·4 sec. for zeroising the counters

15·8 secs.

One single run giving a particular treatment effect for two sets of observations for all 21 centres should have taken, theoretically, about 6 minutes. The cards were then taken from the stacker and placed in the feed, some more paper inserted in the print unit, and the distribution changed. Were it not for the fact that the alterations in the plugging at the end of each single run were very simple, the complete tabulation would have been rather slow. The only plugging alteration, however, was the changing of one plug leading from the drum (giving the distribution designation) to the following card column on which the distribution was taking place. This plug leads to the "following card column" "1" for the first run, to "2" for the second run and so on, leading to "following card column" "32" for the thirty-second run. At the end of each group of 32 runs a change in plugging was necessary only on the first of the three panels—the changing of the plugs from the card columns of the fields to be added to the distributors. In spite, therefore, of the tabulation of the treatment effects consisting of 160 separate tabulator runs, the whole was done in about 30 tabulator-hours, including time lost for minor troubles of controls acting when they should not (giving split totals) and so on.

The summary cards.

All the treatment effects at each centre were punched on cards. The constitution of these summary cards was as follows (the designation of the treatment effect being the number of the column by distribution on which it was obtained):—

	Actual columns
Centre	1, 2
Designation of treatment effect	3, 4
Dirty roots	5– 9 + 10
Clean roots	11–15 + 16
Total sugar	17–21 + 22
Tops	23–27 + 28
Plant number	29–33 + 34
Tare factor	35–39 + 40
Sugar percentage	41–45 + 46
Purity	47–51 + 52
Noxious nitrogen	53–57 + 58

Five columns were necessary for the numerical part of each effect, together with another column giving its sign (in this particular instance a "1" denoted a positive effect and a "5" a negative effect).

There were therefore 22 columns remaining on the summary card which could be used to carry out supplementary analyses.

Checking of tabulation.

For the purpose of checking the tabulation and for other purposes, the designation of the treatment effects was supplemented by a five-column code in columns 59, 60, 61, 62, and 63, twice the mean being 00000, the N effect 10000, the NP effect 11000 and so on. The summary cards were run through the tabulator in exactly the same way as the original cards, distributing first on column 59 and then on column 60. The result of this tabulation should be exactly 32 times the yield of plot 31 and plot 30—*i.e.*, the plots receiving *pksb* and *nksb* respectively. The process could be continued

to reproduce all the original plot values (multiplied by 32), but a couple of runs were thought sufficient to check the whole of the tabulations. If the result of the check tabulation had not been correct, it would have been necessary to do the set of tabulations again, since the check only indicates that some of the 31 effects are wrong and not which ones; in fact, with this particular analysis, the check tabulations brought to light only one tabulating error, which was easily located and corrected.

Supplementary analysis.

In previous years the series of experiments consisted of 2^4 experiments on the four factors, *n, p, k* and *s*. It was therefore decided to obtain treatment effects in the absence of *b*—for example, that for the factor *n* being $N - NB$. (Since, however, the factor *b* had little effect, these treatment effects were not used.) When the 5-column code for the treatment effects had been put on the cards, these quantities were obtained from the tabulator by sorting the cards into groups ignoring the last column, and subtracting the field on the card with " 1 " in this last column from that on the card with " 0 ". It was first necessary to abstract the cards for effects *NSB, PKB* and *NPKS*, which were confounded with blocks and were assumed to be zero. Using two counters, all 15 treatment effects and the mean (multiplied by 2) were obtained for one factor in one run through the machine. The rate at which these results were obtained on a tabulator was rather slow, since there were in general only 2 cards in each group, the theoretical time for each group of 2 cards being:—

0·6 sec. for first card listing and tabulating
0·4 sec. for tabulating the second card
0·4 sec. for subtracting one counter from the other
0·6 sec. for printing the result
0·4 sec. for zeroising the counters

2·4 secs.

The theoretical speed was therefore 3000 cards per hour, so that a single run giving the effects at all 21 centres for two sets of observations took about a quarter of an hour of running time.

Samples from the 32 plots were grouped for chemical analysis, and it was necessary to obtain the yields of the eight combinations of the three treatments, *n, s,* and *b* averaging over the other two treatments. At the same time the triple interaction of the three treatments was to be taken as zero. The required yields are compounded of the six treatment effects *N, S, NS, B, NB, SB* and the mean (multiplied by two). The following information was punched on the cards containing these effects:—

Effect	Designation	Column							
		64	65	66	67	68	69	70	71
Mean (× 2)	01	1	1	1	1	1	1	1	1
N	02	0	1	0	1	0	1	0	1
S	09	0	0	1	1	0	0	1	1
NS	10	1	0	0	1	1	0	0	1
B	17	0	0	0	0	1	1	1	1
NB	18	1	0	1	0	0	1	0	1
SB	25	1	1	0	0	0	0	1	1

Tabulating these effects, distributing in turn on column 64 to 71 and subtracting the sum of the 0's from the sum of the 1's gave the yields from the treatments nil, *n, s, ns, b, nb, sb* and *nsb* averaged over *p* and *k* at each of the 21 centres. This tabulation was again rather slow, as there were only 7 cards in each group, and the speed realized was about 2000 cards per hour.

Presentation of results, computation of errors and supplementary hand analysis.

The treatment effects were listed in order for convenience in abstracting and other work. A section of the listing showing the effects at one centre for total sugar (in units of one-hundredth of a

cwt.), and for tops (in units of one-thousandth of a ton) is given: a " 1 " attached to a particular effect indicates that it is positive, and a " 5 " that it is negative.

It was not considered economic to obtain the errors by the use of Hollerith machines. These were obtained very rapidly by listing the 9 treatment effects which were used to estimate the error. The sum of squares of these 9 effects multiplied by 8/9 gives the error mean square (s^2) and standard error of each treatment effect is $\sqrt{\frac{s^2}{8}}$.

A number of covariances—*e.g.*, of yield on plant number—had to be examined, and this was also facilitated by the separate listing of the treatment effects which were used to estimate the error

Centre	Effect	Total sugar		Tops	
23	01	5695	1	22483	1
23	02	101	1	3409	1
23	03	27	1	283	5
23	04	83	5	145	5
23	05	465	1	1263	1
23	06	167	1	485	1
23	07	113	1	289	1
23	08	3	1	435	1
23	09	645	1	1111	1
23	10	179	1	33	1
23	11	91	5	419	5
23	12	103	1	413	5
23	13	197	5	881	5
23	14	151	5	455	5
23	15	159	1	229	1
23	16	1	1	53	5
23	17	51	5	633	5
23	18	41	5	327	5
23	19	75	5	419	5
23	20	123	1	557	5
23	21	11	1	299	1
23	22	171	5	741	1
23	23	249	5	295	5
23	24	93	1	791	1
23	25	93	5	765	5
23	26	37	1	111	5
23	27	105	5	491	5
23	28	109	1	937	5
23	29	173	1	371	1
23	30	63	1	545	1
23	31	107	5	221	1
23	32	5	5	559	1

If it were desired to test the regression of the result of observation B on that of observation A, for example, it was necessary only to form the sum of products—$[AB]$ say, of the error treatment effects of A and B. The sums of squares of the A effects $[AA]$ and of the B effects $[BB]$ would have been obtained already in the computation of the errors and the test of significance is the variance ratio test of $[AB]^2/[AA]$ against $\{[BB] - [AB]^2/[AA]\}/8$ with 1 and 8 degrees of freedom respectively.

General considerations.

The actual analysis was carried out by Hollerith in about six days, two days being taken for punching and multiplying, two days tabulating using two tabulators, and two days for supplementary work. The computation of errors and classification of the results took a comparatively trivial time. This may be compared with the time required in doing the analysis by hand using electric calculating machines; it is estimated that four computers would be occupied for something of the order of six weeks doing the same number of analyses. From the point of view of cost there is little to choose between the two methods, but the time factor was important, for the results of the experiments were not all available until early in January, and the analyses were required by the end of February, when the experimental programme for the next season came under consideration.

The time factor is also important, in that the analysis of most agricultural experiments must be carried out during the following winter, and imposes a heavy seasonal load on a computing staff. The method described does not require skilled computers to a large extent, and therefore can be used to relieve the seasonal load.

The analysis of this type of experiment by the use of punched cards is of particular value, in view of the present emphasis on series of similar experiments rather than on single experiments of varying designs.

Application of the method to 3^n factorial designs.

The simplicity of the analysis of the 2^n type of experiment lies in the fact that the treatment effects are all linear functions of the plot yields with coefficients of plus or minus one-sixteenth. Another common type of experiment is the 3^n testing all combinations of n factors each at three levels. In the case of one replicate of a 3^3 experiment arranged in three blocks of nine, with one pair of the eight triple interaction degrees of freedom confounded with blocks, the partition of the degrees of freedom in the analysis of variance of the results is frequently as follows (assuming the three factors to be n, p, k):—

	D.F.
Blocks	2
N { linear	1
N { curvature	1
P { linear	1
P { curvature	1
K { linear	1
K { curvature	1
NP linear by linear	1
NK linear by linear	1
PK linear by linear	1
Error	15
Total	26

The evaluation of the total sum of squares presents little trouble, particularly if the plot yields are listed conveniently and a fully automatic Monroe calculating machine is available. The linear effect of n (the levels being 0, 1 and 2 units) is $N' = \frac{1}{18}\{[n_2] - [n_0]\}$, where $[n_2]$ is the sum of the plots receiving n at level 2 and so on. The curvature is given by the equation $N'' = \frac{1}{18}\{[n_2] - 2[n_1] + [n_0]\}$ and the linear-by-linear n and p interaction is $N'P' = \frac{1}{6}\{[n_0p_0] - [n_2p_0] - [n_0p_2] + [n_2p_2]\}$. The blocks may also be regarded as a three-level factor (b) and the two degrees of freedom split into a linear and a curvature degree of freedom. It is then possible to obtain by distribution four linear effects B', N', P', K', four curvatures B'', N'', P'', K'', and the three linear-by-linear two-factor interactions, but without the appropriate numerical divisors. In the case of the 2^n type of experiment the divisor is the same for all treatment effects, and no difficulty arises. Given the effects without the divisors, however, the completion of the analysis of variance by inserting the individual degrees of freedom sum of squares, consisting of a certain fraction of the square of the number obtained by the tabulator, would be a light task. In order to make the analysis completely self-checking, it would be advisable to evaluate the square for each degree of freedom—*i.e.*, in addition to the above, the 6 linear-by-curvature and 3 curvature-by-curvature two-factor interactions, and the four pairs of triple interactions W, X, Y and Z (one of these being identical with blocks), each of which would be split into two separate degrees of freedom.

The above method is analogous to that used in the analysis of 2^n experiments, in that each degree of freedom corresponds to a treatment effect consisting of a simple linear combination of the plot yields. In certain instances, however, it is desirable to examine all the interactions of pairs of factors. In the case of a 3^4 experiment, for example, it is possible to obtain a reliable estimate of the experimental error using triple and quadruple interactions only. In this case with one replicate arranged in blocks of 9 plots, the partition of the degrees of freedom could be used, as shown on the facing page.

The use of the method described above for the 3^3 experiment and the construction of three-by-

	D.F.
Blocks	8
Main effects	8
Two-factor interactions	24
Error	40
Total	80

three tables, showing interactions between pairs of factors, from the separate treatment effects would be laborious, and it would be simpler to prepare tables showing these interactions directly from the tabulator. A table showing the totals of plots with each of the nine combinations of two factors with all the marginal totals can be produced by the tabulator in a single run of the machine. All the tables involving two factors for each centre can therefore be produced in six runs of the cards through the tabulator. The analysis could then be completed in the normal way. This method is, of course, only worth applying when several sets of observations on a large series of experiments have to be analysed.

Summary.

A method of analysing 2^n experiments by the use of punched cards is described, and illustrated by the analysis of a 2^5 series of experiments on the manurial requirements of sugar beet. Some suggestions are made for the analysis of a series of experiments of the 3^n type by the same means.
The Hollerith work on this analysis was carried out by the British Tabulating Machine Company, to whom acknowledgements must be made.

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The Statistical Analysis of Variance-Heterogeneity and the Logarithmic Transformation.

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1. *Preliminary remarks.*

While a useful approximate null test of significance of the heterogeneity of variances is available (ref. 1), it is often required in more detailed investigations of variance heterogeneity to apply the powerful technique of "analysis of variance" to the data, when suitably transformed. For an estimate s^2 of a variance σ^2 based on n degrees of freedom, the distribution of ns^2/σ^2 is well known to be a χ^2-distribution with n degrees of freedom, if the estimate s^2 has been obtained from a normal sample. It immediately follows that the distribution of $\ln s^2 - \ln \sigma^2$ is entirely independent of σ^2, and hence that that of $\ln s^2$ only depends on σ^2 through the term $\ln \sigma^2$ in its mean value.* The variate $\ln s^2$ (or any equivalent variate such as $\ln s$, $\ln \sqrt{\frac{2s^2}{\pi}}$, $\log_{10} s^2$, etc.) is thus a convenient variate to consider. The above argument clearly applies in the more general case when the original population from which the sample yielding s^2 was obtained has any distribution of the form $f\left(\frac{x-m}{\sigma}\right)\frac{dx}{\sigma}$. The more detailed discussion of the distribution of $\ln s^2$ in the next section refers to the case of normal samples.

2. *Properties of the distribution of* $\ln s^2$.

Some precautions are necessary when the analysis of variance is applied to a transformed variate (cf. ref. 2). In the ideal case:

(*a*) the transformed variate would be normal,
(*b*) its variance would be unaffected by changes in its mean,
(*c*) real effects would be additive on the transformed scale,
(*d*) the arithmetic mean would provide a fully efficient statistic for the unknown parameter in the case of homogeneity. (These requirements are of course not independent, the fourth being a consequence of the first three.)

In the present case condition (*b*) is exactly satisfied. Condition (*c*) is approximately satisfied provided the heterogeneity effects are not large, and are additive on some scale, say the original one, since they are then necessarily additive to the first order on the transformed scale. Alternatively, the heterogeneity effects may be precisely defined by arithmetic addition on the transformed scale, though this has the disadvantage that the definition in many applications would be an unnatural one, leading to "interaction" effects which would be artificial. With regard to condition (*a*), the characteristic function of $\ln s^2$ is

$$M(t) = \int_0^\infty (s^2)^{it} \left(\frac{ns^2}{2\sigma^2}\right)^{\frac{n}{2}-1} e^{-\frac{ns^2}{2\sigma^2}} d\left(\frac{ns^2}{2\sigma^2}\right) \Big/ \Gamma\left(\frac{n}{2}\right)$$

$$= \left(\frac{\sigma^2}{n/2}\right)^{it} \Gamma\left(\frac{n}{2}+it\right) \Big/ \Gamma\left(\frac{n}{2}\right),$$

whence the cumulant function $K(t) = \ln M(t)$ is

$$K(t) = it(\ln \sigma^2 - \ln \tfrac{1}{2}n) + \ln \Gamma\left(\frac{n}{2}+it\right) - \ln \Gamma\left(\frac{n}{2}\right),$$

and

$$\kappa_1 = (\ln \sigma^2 - \ln \tfrac{1}{2}n) + \psi\left(\frac{n}{2}\right),$$

$$\kappa_{r+1} = \psi^{(r)}\left(\frac{n}{2}\right), \quad (r > 0),$$

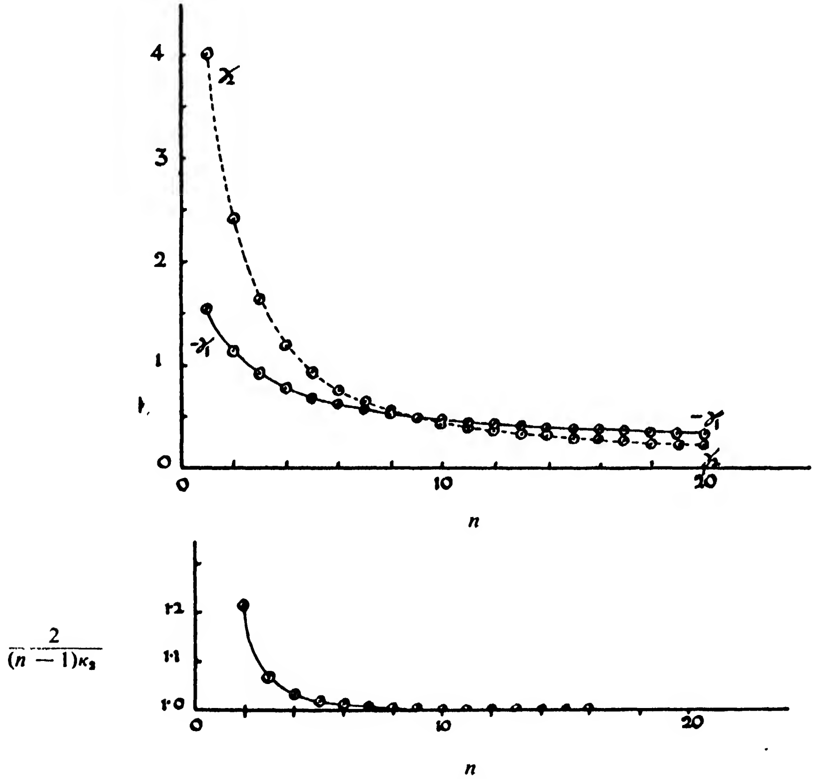
* The notation ln is used for Napierian logarithm.

where $\psi(x)$ is $d \ln \Gamma(x)/dx$, and $\psi^{(r)}(x)$ is the rth derivative of $\psi(x)$.* From these results the values of κ_1, κ_2, $\gamma_1 = \kappa_3/\kappa_2^{3/2}$ and $\gamma_2 = \kappa_4/\kappa_2^2$ are given for reference in Table I up to $n = 20$. For larger values of n, it is sufficient to take

$$\kappa_1 \sim -\left(\frac{1}{n} + \frac{1}{3n^2}\right) + \ln \sigma^2, \qquad \kappa_2 \sim \frac{2}{n-1},$$

$$\gamma_1 \sim -\sqrt{\frac{2}{n-1}}, \qquad \gamma_2 \sim +\frac{4}{n-1}.$$

The values of γ_1 and γ_2 are also plotted in Fig. 1, and the ratio of $2/(n-1)$ to κ_2 in Fig. 2.



FIGS. 1 and 2.
Constants for the distribution of $\ln s^2$ (see Table I).

With regard to condition (*d*), it is noted that in the case of complete homogeneity the information in the mean value of a set of statistics $\ln s_r^2$ tends, as the number in the set becomes large, to be proportional to the value of $1/\kappa_2$, whereas the information in the sufficient statistic s^2 is proportional to $n/2$. A measure of the efficiency is thus $E = 2/n\kappa_2$, and this is also given in Table I and plotted in Fig. 3.

3. *Number of degrees of freedom required for individual variance estimates.*

While no hard-and-fast rule can be laid down, the above results suggest that the transformation may safely be used for $n = 10$ and over, more tentatively from $n = 5$ to $n = 9$, and probably not at all below $n = 5$. In the first case E is over 90 per cent.; in the second between 80 per cent. and 90 per cent.; and in the third below 80 per cent. The values of γ_1 and γ_2 indicate, however, that

* These functions are tabulated in ref. 4.

the approach to normality is rather slow, and this is confirmed by the relatively small difference in the distributions of $\ln s^2$ for $n = 5$ and $n = 9$ (Fig. 4). Of γ_1 and γ_2, the latter is more important in analysis of variance problems, owing to the tendency for γ_1 to be eliminated when *differences* of the variates are tested. In Fig. 5 the tails of the symmetrical distributions with the same values of γ_2 are shown for $n = 5$, $n = 9$ and $n = \infty$, and the slow convergence to normality is again illustrated. The nature of the tail for $n = 5$ and $n = 9$ suggests that even for n greater than 9 the chance of large deviations will be appreciably higher than would be inferred on the normal

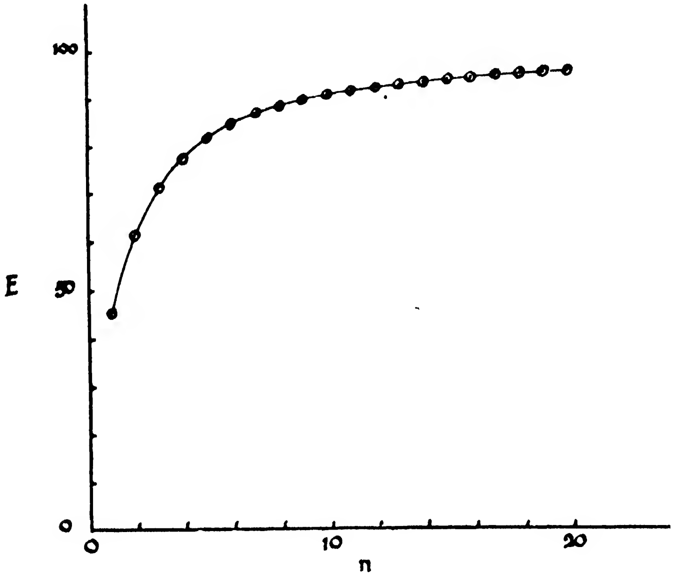


FIG. 3.
The efficiency of the mean of $\ln s^2$ for estimating σ^2.

TABLE I
Constants for the distribution of ln s^2

n	$\kappa_1 - \ln \sigma^2$	$-\left(\frac{1}{n} + \frac{1}{3n^2}\right)$	$1/\kappa_2$	$\frac{2}{(n-1)\kappa_2}$	γ_1	γ_2	$E = (2/n\kappa_2) \times 100$
1	−1·27036	−1·33333	0·20264	—	−1·535	+4·000	40·53
2	0·57721	0·58333	0·60793	1·2159	1·140	2·400	60·79
3	0·36898	0·37037	1·0697	1·0697	0·917	1·613	71·30
4	0·27036	0·27083	1·5505	1·0337	0·780	1·188	77·53
5	0·21313	0·21333	2·0393	1·0197	0·688	0·931	81·57
6	0·17583	0·17593	2·5321	1·0128	0·621	0·763	84·40
7	0·14961	0·14966	3·0270	1·0090	0·570	0·644	86·49
8	0·13018	0·13021	3·5233	1·0067	0·529	0·557	88·08
9	0·11521	0·11523	4·0205	1·0051	0·496	0·490	89·34
10	0·10332	0·10333	4·5183	1·0041	0·469	0·437	90·37
11	0·09365	0·09366	5·0165	1·0033	0·445	0·395	91·21
12	0·08564	0·08565	5·5150	1·0027	0·425	0·360	91·92
13	0·07889	0·07890	6·0138	1·0023	0·407	0·330	92·52
14	0·07313	0·07313	6·5128	1·0020	0·391	0·305	93·04
15	0·06815	0·06815	7·0119	1·0017	0·377	0·284	93·49
16	0·06380	0·06380	7·5111	1·0015	0·364	0·265	93·89
17	0·05998	0·05998	8·0104	1·0013	0·353	0·249	94·24
18	0·05658	0·05658	8·5098	1·0012	0·342	0·234	94·55
19	0·05355	0·05356	9·0092	1·0010	0·333	0·221	94·83
20	−0·05083	−0·05083	9·5088	1·0009	−0·324	+0·210	95·09

approximation, and the real significance of observed deviations consequently somewhat less than the apparent significance.

This conclusion is also illustrated by a comparison of the direct use of the logarithmic variance for a null test of heterogeneity with the approximate test already referred to (ref. 1). Both tests make use of the χ^2 approximation, the one now under consideration simply by comparing the observed variance of $\ln s^2$ with its theoretical variance. In Table II the *true* values of χ^2 required

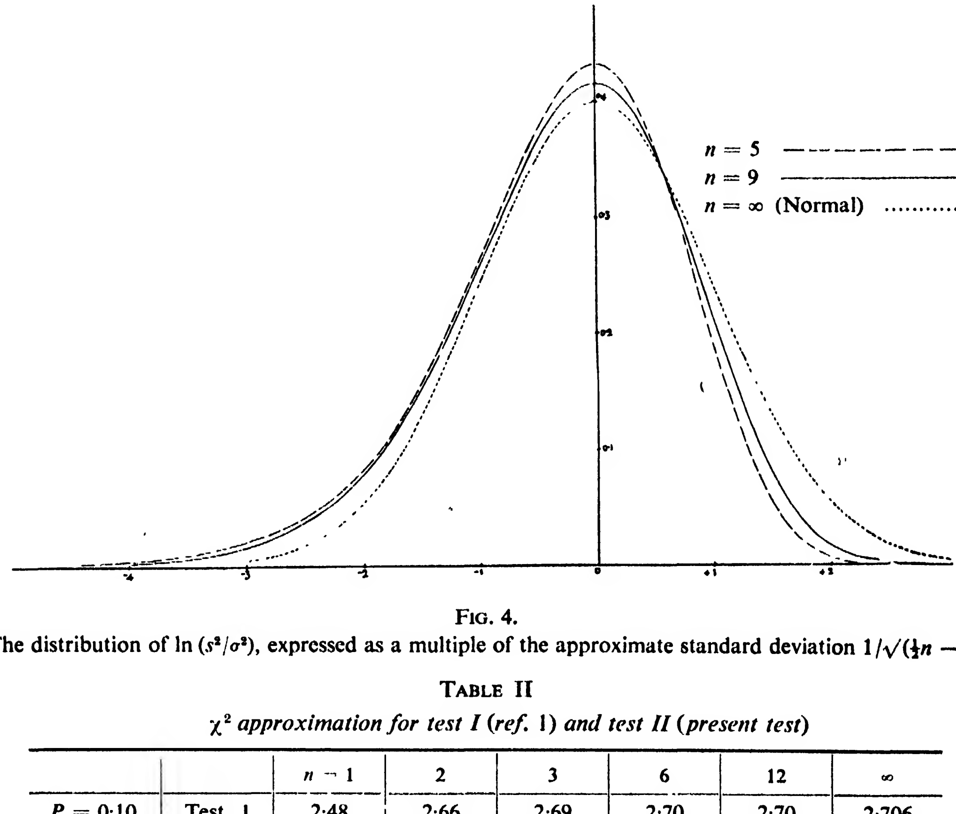


FIG. 4.
The distribution of $\ln (s^2/\sigma^2)$, expressed as a multiple of the approximate standard deviation $1/\sqrt{(\frac{1}{2}n - \frac{1}{2})}$.

TABLE II

χ^2 approximation for test I (ref. 1) and test II (present test)

		$n = 1$	2	3	6	12	∞
$P = 0{\cdot}10$	Test I	2·48	2·66	2·69	2·70	2·70	2·706
	II	2·62	2·64	2·65	2·68	2·69	2·706
$P = 0{\cdot}05$	Test I	3·39	3·72	3·80	3·83	3·84	3·841
	II	4·25	4·08	4·01	3·93	3·88	3·841
$P = 0{\cdot}02$	Test I	4·61	5·16	5·31	5·39	5·41	5·412
	II	6·99	6·42	6·12	5·78	5·59	5·412
$P = 0{\cdot}01$	Test I	5·54	6·27	6·47	6·60	6·63	6·635
	II	9·52	8·52	7·97	7·32	6·97	6·635

for significance are shown for both tests in the case of two variance estimates with equal numbers of degrees of freedom. These values were obtained from the well-known tabulated significance levels for s_1^2/s_2^2 by taking s_1^2 and s_2^2 such that s_1^2/s_2^2 had the required critical ratio, and calculating χ^2 by each of the two methods. The limiting values for $n = \infty$ given in the last column of the Table correspond to the critical values of χ^2 from the tabulated distribution of χ^2 for one degree of freedom, and are the values which would be used for significance in practice. It is seen that, apart from the requirement of equal numbers of degrees of freedom for the different variance estimates,

the present test is much inferior to the other one for small n; and even for $n = 12$ is exaggerating significance at the higher significance levels.

4. *Example.*

The following data (Table III) represent $\log_{10}\left(10\sqrt{\frac{2s^2}{\pi}}\right)$, for three groups A, B and C, the columns corresponding to different days' results. Each item corresponds to a variance estimate with average number of degrees of freedom approximately equal to 48 (the number varied slightly from item to item). The resulting analysis of variance is shown in Table IV, the theoretical mean square being $(\log_{10} e)^2/2(n - 1) = 0{\cdot}0020$.

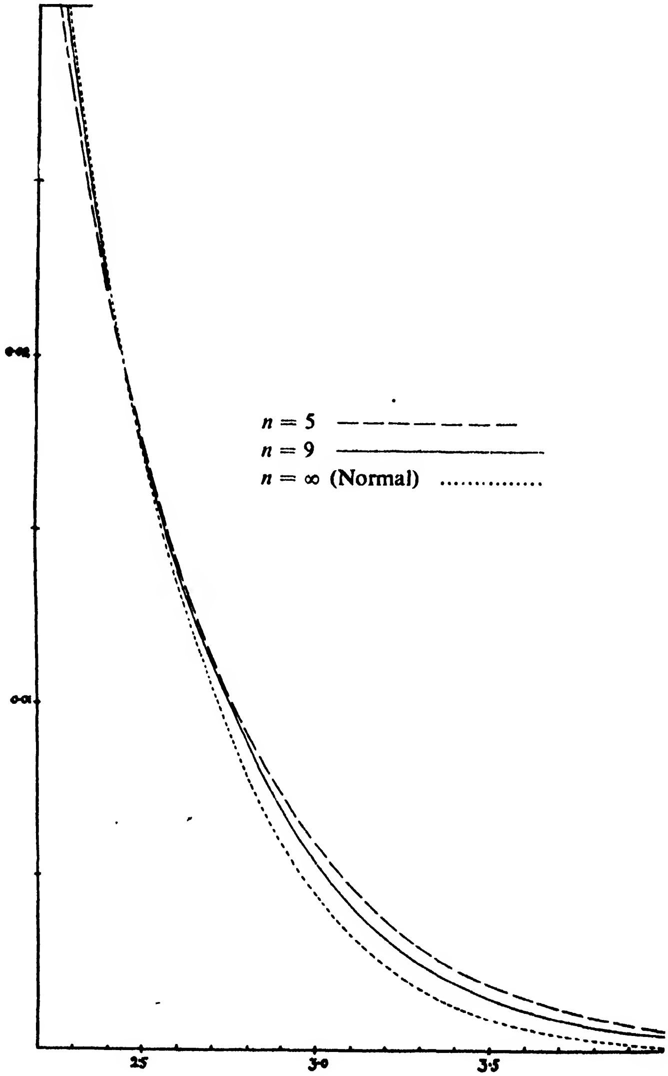


FIG. 5.
Tail of the distribution of $|\ln (s^2/\sigma^2)|$, scales as in Fig. 4.

TABLE III

Day:	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
A	1·10	1·17	1·27	1·05	1·06	1·09	1·06	1·20	1·09	1·05	1·14	1·25	1·19	1·11	1·07
B	0·93	1·02	0·92	0·90	0·85	1·02	0·98	0·96	0·93	0·97	0·93	0·97	1·08	0·88	1·03
C	1·05	0·89	1·09	0·92	0·95	1·08	0·95	0·98	0·83	0·94	1·02	0·99	1·03	0·92	0·90

TABLE IV

	D.F.	S.S.	M.S.
Groups A, B, C	2	0·2667	0·1333
Days	14	0·1047	0·00748
Residual	28	0·1005	0·00359
Theoretical variance	—	—	0·0020
Total	44	0·4719	—

It will be seen that the residual variance is significant ($P \sim 0{\cdot}01$) compared with the theoretical variance, denoting residual heterogeneity. The "groups" and "days" terms are therefore tested against the residual variance, giving a highly significant "groups" effect, and a just significant ($P \sim 0{\cdot}05$) "days" effect.

5. *A method of estimating the amount of heterogeneity.*

In the above example the occurrence of a residual heterogeneity led to the adoption of a different basic level of variability against which the other items were tested. A definite form,

$$f(u/\alpha)du/\alpha \quad , \qquad \text{where } \alpha = E(u),$$

for the distribution of the "real" variance $u = \sigma^2$ is implied by this procedure, and effectively we are using the residual term in the analysis to estimate the dispersion of the u-distribution, *i.e.*, the "amount" of the heterogeneity. There is usually little but common sense and ultimate convenience to guide us in postulating a hypothetical form for $f(u)$; the analysis of variance would be made rigorous if it were possible to choose $f(u)$ so that the logarithm of the *observed* variance (w) would be normally distributed, but this cannot be done. For let $w = uv$, so that u and v are statistically independent, and v is proportional to a χ^2-variate. Then the normal distribution of

$$\ln w = \ln u + \ln v$$

implies, by a theorem of Cramér,[3] that $\ln v$ is normally distributed, and this gives a contradiction.

Since it is impossible to justify the analysis completely, we might try to make the resultant distribution of $\ln w$ satisfy the conditions (b) and (d) of Section 2, by arranging that

(b) the sampling variance of $\ln w$ is independent of the mean "true" variance α; and
(d) $\Sigma \ln w$ is a sufficient statistic for α.

That this also is impossible follows from an unpublished theorem of L. Solomon (1944),* which asserts that a variate (here $\ln w$) has these two properties if and only if it is normally distributed.

It is, however, possible so to choose $f(u)$ that $\ln w$ has the distribution investigated in Section 2, but with the number of degrees of freedom reduced to allow for the inflated variability. Explicitly, we choose $f(u)$ so that w is distributed as

$$\alpha\chi_\lambda^2/\lambda \quad ,$$

the new parameter λ being a measure of the heterogeneity. In the case of complete homogeneity, we have of course $\lambda = n$, and in general λ will be less than this value. The distribution of w being now

$$\frac{(\lambda/2\alpha)^{\frac{1}{2}\lambda}}{\Gamma(\frac{1}{2}\lambda)} e^{-\lambda w/2\alpha} w^{\frac{1}{2}\lambda - 1} dw,$$

* We are indebted for this reference to Dr. A. C. Aitken.

the maximum likelihood estimates for α and λ (formed from samples of k independent observations) are given by

$$\bar{w} = \Sigma w/k = \hat{\alpha} \quad ,$$

and

$$t = k \ln \bar{w} - \Sigma \ln w = k\{\ln (\tfrac{1}{2}\hat{\lambda}) - \psi(\tfrac{1}{2}\hat{\lambda})\}, \sim k/(\hat{\lambda} - \tfrac{1}{3}) \quad (\hat{\lambda} \text{ large}),$$

and it is readily seen that these are a *pair* of sufficient statistics: in fact we have (see the Appendix for details)

$$p(S|\alpha, \lambda) = p(\bar{w}|\alpha, \lambda)\, p(t|\lambda)\, p(S|w, t).$$

The distribution of $\bar{w}$ is obtained at once from first principles, while that of t follows from the argument given earlier by Bartlett [1] for the homogeneous case, and on employing the approximation developed on that occasion we find

$$\bar{w} \doteq \frac{\alpha}{k\lambda} \chi_{k\lambda}^2 \quad ,$$

and

$$t \doteq \frac{1}{\lambda}\left(1 + \frac{1 + 1/k}{3\lambda}\right) \chi_{k-1}^2, \text{ approx.},$$

the two χ^2-variates being independent. This approximation to the distribution of t is very accurate, the first four cumulants being correct to 5 per cent. even when λ is as small as 5.

To illustrate the method, we shall apply it to the three "groups" A, B, and C of Table III, estimating for each a value of λ, the heterogeneity parameter. We find :

TABLE V

Group	t	Estimate of λ	90% fiducial interval
A	0·852	16·8	8·1 to 28·2
B	0·583	24·4	11·6 41·0
C	0·815	17·5	8·4 29·4

It will be recalled that each variance estimate contributing an entry to Table III was based on about 48 degrees of freedom. If, therefore, the data were homogeneous, the true value of λ would be 48. Since in each case the 90 per cent. fiducial interval for λ excludes this value, there is significant evidence for heterogeneity, the observed variances fluctuating as if they were based on about 20 instead of 48 degrees of freedom. Since it is to be expected that the day-to-day variations for the three groups may be correlated, the three χ^2-variates corresponding to the three values of t cannot be supposed to be independent. If they *were* independent, it would have been possible to test the disparity between the three λ-estimates by calculating the statistic

$$T = 3 \ln \bar{t} - \Sigma \ln t, = 3\{\ln (\tfrac{1}{2}\hat{\Lambda}) - \psi(\tfrac{1}{2}\hat{\Lambda})\}, \text{ say},$$

and applying the null test corresponding to the above method of estimation, testing whether Λ were significantly less than $(k - 1) = 14$. This null test, of course, is identical with the test of homogeneity given by Bartlett in the earlier paper [1] already referred to, which itself, since the numbers of degrees of freedom are here equal, is equivalent to the original test of J. Neyman and E. S. Pearson.[5]

Since the three estimates of λ are not independent, it is not clear how they should be combined. Assuming that there is no real difference in heterogeneity behaviour between the three groups, and proceeding as if the estimates were independent, we find as the pooled estimate of λ the harmonic mean of the three previous estimates,

$$\hat{\lambda} = 19{\cdot}0.$$

On a future occasion one could thus assign fiducial limits to the mean "true" variance, given an observed variance, or test for a real difference between variances observed for the same "group" on different days, by assigning to each observed variance 19 instead of 48 degrees of freedom, and then proceeding exactly as if there were no heterogeneity. One *cannot*, however, make any statement about the fluctuations to be expected when an observed variance is based on other than 48 degrees of freedom. This is a severe limitation of the present method, the full force of which will be felt when, in the next section, we show that the form $f(u)$ of the implied heterogeneity distribution varies with n, the (actual) number of degrees of freedom possessed by the observed

variances. In some circumstances, therefore, it will be preferable to sacrifice the internal consistency of the method of estimation developed here, and to return to the more crude but more adaptable method of the logarithmic transformation.

It is very instructive to analyse the same set of data for heterogeneity by means of the logarithmic transformation. In the treatment given above we assumed that an observed variance was still a χ^2-variate, the sole effect of heterogeneity being to reduce the effective number of degrees of freedom. We now suppose that an observed variance is a constant multiple of the product of three independent χ^2-variates, the first accounting for the normal sampling fluctuations, the second fluctuating from day to day, and the third fluctuating from one observed variance to another and representing the effect of general heterogeneity in the conditions of the experiment. Allotting to these χ^2-variates n, μ_1, μ_2 degrees of freedom, respectively, we wish to estimate μ_1 and μ_2. For the natural logarithm of the square-root of an observed variance, the second cumulant is, to a good approximation,

$$\kappa_2 = \frac{1}{2}\left(\frac{1}{n-1} + \frac{1}{\mu_1 - 1} + \frac{1}{\mu_2 - 1}\right).$$

(A fourth factor, varying with the identity of the group, will not concern us here.)
Thus, referring to the analysis of variance in Table IV, we have for the " days " term,

$$\frac{1}{2}\left(\frac{1}{n-1} + \frac{3}{\mu_1 - 1} + \frac{1}{\mu_2 - 1}\right)\chi_{11}^2 = 0\cdot1047(\ln 10)^2 = 0\cdot5552.$$

and for the " residual " term,

$$\frac{1}{2}\left(\frac{1}{n-1} + \frac{1}{\mu_2 - 1}\right)\chi_{28}^2 = 0\cdot1005(\ln 10)^2 = 0\cdot5328.$$

From the latter we obtain an estimate $\mu_2 = 61$ for the general heterogeneity, with a 90 per cent. fiducial interval of (25 to 220). Substituting the estimate of μ_2 in the expression for the " days " term, *and neglecting the uncertainty in* μ_2, we obtain an estimate $\mu_1 = 74$ for the day-to-day variations, with a 90 per cent. fiducial interval of (24 to 340). It will be observed how the greater flexibility of the logarithmic transformation analysis makes it possible to disentangle the " days " effect from the general heterogeneity, and evidently more complicated situations could be dealt with in the same way.

If the two methods of analysis were equivalent we should have

$$\frac{1}{\lambda - 1} = \frac{1}{n-1} + \frac{1}{\mu_1 - 1} + \frac{1}{\mu_2 - 1}$$

—which suggests incidentally how values of λ corresponding to different " actual " numbers of degrees of freedom, n, might be compared. In fact we have, for the left-hand side, an estimate of 1/18, and for the right-hand side an estimate of 1/19.

A numerical estimate of the " amount " of the heterogeneity is of course only of value if there is some reason to suppose that the variability in the conditions of experiment itself possesses some statistical regularity. But the methods given here provide a rational means of putting this hypothesis to the test.

6. *The heterogeneity distribution as the " quotient " of two χ^2-distributions.*

We shall now substantiate the claim made in Section 5 by finding the form

$$f(u/\alpha)du/\alpha \qquad (\alpha = E(u))$$

of the distribution of the " real " variance, u, which has the effect of changing the distribution of the observed variance w from that of

$$\alpha\chi_n^2/n$$

to that of

$$\alpha\chi_\lambda^2/\lambda \quad .$$

Thus, if we write $w = uv$, then multiplication by the independent variate u has the effect of reducing the number of degrees of freedom of v, while leaving its mean value and the form of its distribution unchanged.

It is to be noted that we cannot say " $u = w/v$, and so $\frac{1}{2}\ln u$ must be (apart from an additive constant) a Fisher z-variate with (λ, n) degrees of freedom", for w and v are not independent.

We are, in fact, concerned with a problem in what P. Lévy has called "the arithmetic of distribution functions."

The resolution of the problem is formally very simple. Let $h(w/\alpha)dw/\alpha$ and $g(v)dv$ denote the distributions of w and v, respectively, so that

$$h(w)dw = \frac{(\frac{1}{2}\lambda)^{\frac{1}{2}\lambda}}{\Gamma(\frac{1}{2}\lambda)} e^{-\frac{1}{2}\lambda w} w^{\frac{1}{2}\lambda - 1} dw,$$

while $g(v)dv$ has the same form, λ being replaced by n; then we must have

$$h(w) = \int_0^\infty f(u) g\left(\frac{w}{u}\right) \frac{du}{u} .$$

To solve the integral equation, let $F(\theta)$ denote the Mellin transform

$$F(\theta) = \int_0^\infty f(u) u^{\theta - 1} du,$$

and let $G(\theta)$, $H(\theta)$ be formed in the same way with the functions $g(v)$ and $h(w)$. Then clearly

$$F(\theta) = G(\theta) = H(\theta) = 1, \text{ for } \theta = 1 \text{ and } 2,$$

while the integral equation transforms into

$$H(\theta) = F(\theta)G(\theta).$$

Since

$$G(\theta) = \frac{\Gamma(\frac{1}{2}n + \theta - 1)}{\Gamma(\frac{1}{2}n)(\frac{1}{2}n)^{\theta - 1}}, \qquad H(\theta) = \frac{\Gamma(\frac{1}{2}\lambda + \theta - 1)}{\Gamma(\frac{1}{2}\lambda)(\frac{1}{2}\lambda)^{\theta - 1}},$$

the formula for $F(\theta)$ is

$$F(\theta) = \left(\frac{n}{\lambda}\right)^{\theta - 1} \frac{\Gamma(\frac{1}{2}\lambda + \theta - 1)\,\Gamma(\frac{1}{2}n)}{\Gamma(\frac{1}{2}n + \theta - 1)\,\Gamma(\frac{1}{2}\lambda)},$$

or equivalently

$$f(u)du = \frac{(\lambda/n)^{\frac{1}{2}\lambda}\Gamma(\frac{1}{2}n)}{\Gamma(\frac{1}{2}\lambda)\Gamma(\frac{1}{2}n - \frac{1}{2}\lambda)} u^{\frac{1}{2}\lambda - 1}(1 - \lambda u/n)^{\frac{1}{2}n - \frac{1}{2}\lambda - 1} du, \quad (0 < u < n/\lambda),$$

while $f(u)$ is identically zero for other values of u.

It will be observed that the distribution of the "real" variance u involves n, the "actual" number of degrees of freedom of the observed variance. This fact, as we have already pointed out, makes it impossible (without approximation) to apply the first method for the estimation of heterogeneity described in Section 5 unless we are concerned always with variance estimates based on the same number of degrees of freedom. To illustrate the nature of the distribution for different (fixed) values of n, it has been drawn in Fig. 6 for constant mean and variance. It is easily shown that the variance of $f(u)du$ is

$$F(3) - \{F(2)\}^2 = F(3) - 1 = \frac{1 + 2/\lambda}{1 + 2/n} - 1 = \frac{2}{\mu}, \text{ say,}$$

and so in order to have the same variance for two different values of n we must have

$$\frac{1 + 2/\lambda'}{1 + 2/n'} = \frac{1 + 2/\lambda}{1 + 2/n} .$$

In particular, $\lambda' = \mu$ when $n' = \infty$. The parameter μ might, in fact, very well be adopted as an (approximate) absolute measure of heterogeneity, independent of the value of n. For large λ and n we should then have

$$\frac{1}{\lambda} \sim \frac{1}{n} + \frac{1}{\mu},$$

which agrees with the alternative method of comparing values of λ associated with different values of n given at the end of Section 5. The example there discussed gave $n = 48$, $\lambda = 19$, and so $\mu \sim 31$. The curves of Fig. 6 represent the more extreme case when $n = 20$, $\lambda = 10$, and so $\mu \sim 20$. In either case the extra variability attributed to heterogeneity is of the same order as the inherent variability consequent upon the finite size of the samples. The actual constants used in the construction of Fig. 6 are given in Table VI.

TABLE VI

n	5	10	20	∞
λ	3·793	6·471	10	22 $(=\mu)$

It will be seen from Fig. 6 that when $n = 5$, $\lambda = 3{\cdot}8$, the form of the distribution $f(u)du$ is not one that could reasonably be expected in most practical problems, and even when $n = 10$, $\lambda = 6{\cdot}5$, the distribution drops to zero at the upper end more sharply than we should wish. Such an infinite "cliff" for the upper end of the distribution will occur whenever $n - \lambda$ is less than 2; *i.e.*, for small

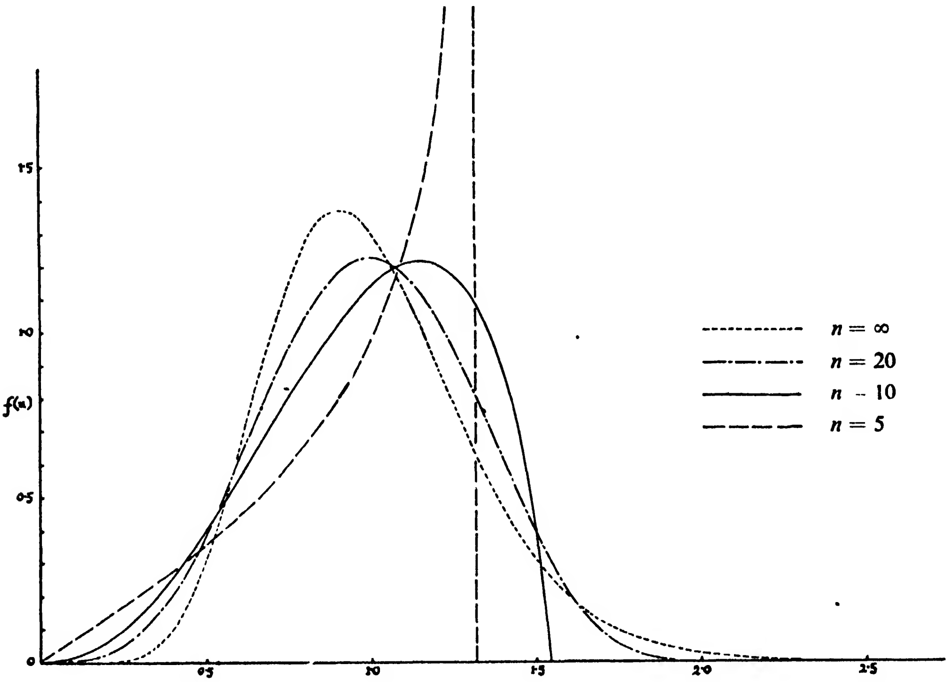


FIG. 6.

Distribution of $u = \sigma^2$ (in the case of heterogeneity) which has the effect of reducing the number of degrees of freedom in the distribution of the observed variance $w = s^2$.

values of n, and also in cases when the amount of heterogeneity is small (so that λ is not much less than n). This latter case of failure is disappointing because it makes the link we have established between our method of estimating the heterogeneity when it is appreciable and the earlier null test of homogeneity (test that $\lambda = n$) less satisfactory than at first appeared. However, when appreciable heterogeneity is present and the observed variances are based on not too small a number of degrees of freedom, the form of the heterogeneity distribution will obviously be quite acceptable as an initial assumption.

If it should prove convenient to combine the assumption of the heterogeneity distribution $f(u)du$ with the logarithmic transformation, then it should be noticed that the earlier restriction on the use of that transformation to values of n not too small now becomes the (heavier) restriction to values of λ not too small.

We wish to make acknowledgement to Mr. F. J. Anscombe with whom these problems were discussed at the outset of this investigation, and to the Chief Scientific Officer, Ministry of Supply,

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Appendix

The joint sampling distribution of $\bar{w}$ *and* t.

We have asserted that if

$$\bar{w} = \Sigma w_i/k$$

and

$$t = k \ln \bar{w} - \Sigma \ln w_i,$$

where the (w_i) are drawn independently from a distribution of the χ^2 form with mean α and with λ degrees of freedom, then

$$p(S\,|\,\alpha, \lambda) = p(\bar{w}\,|\,\alpha, \lambda)\ p(t\,|\,\lambda) p\ (S\,|\,\bar{w}, t).$$

The justification for this will now be given.* It is obvious at once that $\bar{w}$ will have the distribution of

$$\alpha \chi_{k\lambda}{}^2/k\lambda,$$

and it can also be seen, not quite so immediately, that we shall have

$$p(S\,|\,\alpha, \lambda) = p(\bar{w}\,|\,\alpha, \lambda)\ p(S\,|\,\bar{w}, \lambda).$$

The point we wish to make is that *t is a sufficient statistic for λ in the conditional distribution* $p(S\,|\,\bar{w}, \lambda)$.

To see this, we observe that the joint sampling distribution for the k members of S is of the form

$$F(w_1 + w_2 + \ldots + w_k)(w_1 w_2 \ldots w_k)^{s-1} dw_1 dw_2 \ldots dw_k \quad (\text{each } w_i \geqslant 0),$$

and we transform the variables (following a procedure associated with the name of Dirichlet) by writing

$$w_1 + w_2 + \ldots + w_i = x_1 x_2 \ldots x_{k-i+1} \qquad (i = 1, 2, \ldots, k),$$

so that

$$\frac{\partial(w_1, w_2, \ldots, w_k)}{\partial(x_1, x_2, \ldots, x_k)} = \pm\, x_1{}^{k-1} x_2{}^{k-2} \ldots x_{k-1},$$

$$w_1 = x_1 x_2 \ldots x_k, \qquad w_i = x_1 x_2 \ldots x_{k-i+1}(1 - x_{k-i+2}) \qquad (i = 2, 3 \ldots, k),$$

and $\quad e^{-t} = (w_1 w_2 \ldots w_k) x_1{}^{-k} k^k.$

The distribution then becomes

$$F(x_1) x_1{}^{ks-1} dx_1\ \ k^{-ks} e^{-st} \prod_{i=2}^{k} \frac{dx_i}{x_i(1 - x_i)} \qquad (0 \leqslant x_1 < \infty, 0 \leqslant x_i \leqslant 1, i \neq 1),$$

from which the result follows at once, since $s = \frac{1}{2}\lambda$.

The characteristic function of t is now fairly easily found to be

$$M_t(u) = k^{-iku} \frac{\{\Gamma(\frac{1}{2}\lambda - iu)\}^k}{\Gamma(\frac{1}{2}k\lambda - iku)} \frac{\Gamma(\frac{1}{2}k\lambda)}{\{\Gamma(\frac{1}{2}\lambda)\}^k}.$$

Calculation of the cumulants then leads at once to the χ^2 approximation for the distribution of t given by Bartlett in 1937.

* See also E. J. G. Pitman, *Proc. Camb. Phil. Soc.*, **33** (1937), 212–222.

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Statistical Methods in the Selection of Navy and Army Personnel

By P. E. Vernon

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Introduction

The main theme of this paper is not so much the statistical advances made during four years of psychological work in Navy and Army personnel selection as the makeshifts to which the statistician may be forced when his job is to extract from highly fallible data, in the shortest possible time, results which can immediately be practically applied. This admission does not mean that all our work was amateurish or statistically invalid. For though I cannot lay claim myself to any wide knowledge of statistical theory, the direction of statistical work in D.S.P. (the Army Directorate for the Selection of Personnel) was in the competent hands of Mr. Patrick Slater; and both he and I have received frequent and most valuable guidance from Professor Cyril Burt, and from others, including Mr. Kendall and Dr. Fraser Roberts. The somewhat unorthodox techniques and short-cuts that I am going to describe must be attributed first to the highly abnormal type of material which was so often submitted for statistical analysis, and secondly to the shortage of qualified staff for coping with the huge volume of such material and the necessity for employing largely unskilled or only partially trained assistants for much of the tabulation and routine calculations.

The following is a concrete example. One of the standard Army tests was a test of bodily agility and manual deftness, based on the time taken to transfer a series of iron rings from two posts to two other posts a standard distance away, running backwards and forwards at top speed. Evidence had been accumulating that this test was not very reliable and that its diagnostic value was poor for the purpose for which it was intended—namely, eliminating the least agile recruits from infantry and gunnery. Further, it had to be done in gym shoes, and owing to the rubber shortage the supply of these was running out. A decision was needed early in 1945 whether to withdraw the test or to substitute something similar. Now, a number of alternative physical tests, based on athletic and gymnastic exercises, were being used at an Army re-allocation centre. Several, such as the 100 yards and the five-mile walk, were out-of-door tests, and it would have been unfair to apply them during wintry weather. The results of examination of 490 soldiers on these and on the agility test, together with age, height, weight, medical category and general intelligence score—14 variables in all—at last arrived, barely a fortnight before the answer had to be given. The physical tests had been applied in four different camps, by different instructors, and it was by no means certain that conditions were identical throughout. Some rapid analyses of variance between camps showed that differences were indeed significant, but so small as to have only a slight effect on test inter-correlations. Many of the distributions were far from normal: for example, the 100 yards, and pull-ups to the chin, as shown in Table I. No assistants were available who were conversant with centroids or mean class values, and I had too much other work to calculate the 98 correlations by centroids myself. However, applying the centroid method to a few specimen correlations yielded coefficients very little different from those given by ordinary product-moment technique. An assistant therefore tabulated and calculated the 98 product-moment coefficients in about eight days. I then carried out eight factorial analyses by simple summation (a modification

TABLE I

Specimen distributions of scores on personnel tests, training marks, and grades

100 yards sprint		Pull-ups		Map-reading-marks of motor-driving trainees		Morse receiving scores for accuracy		Percentage grades awarded by two Officer Selection Boards (N = 927 and 901)	
Secs.	f *	No.	f	%	f	%	f	Grade	f%
11	4	10	55	95+	42	100	12	A	4
12	52	9	19	87+	53	99·8+	15	B	56
13	126	8	43	79+	65	99·6+	12	C	15
14	104	7	50	71+	46	99·4+	10	Fail or	25
15	70	6	60	63+	25	99·2+	6	put back	
16	40	5	60	55+	16	99·0+	6		
17	27	4	77	47+	7	98·8+	6	A	12
18	27	3	55	39+	9	98·6+	2	B	37
19	11	2	34	31+	2	99·3	2	C	4
20	13	1	14	30−	1	98·0	2	Fail or	47
21	0	0	23			97·8	3	put back	
22	5				266	96·7	1		
23	6		490			96·0	1		
24	0					93·7	1		
25	3								
26	2						79		
	490								

f = frequency of corresponding score.

of Thurstone's technique), this number being needed to yield communalities by successive approximation, and in order to find the effects of holding Age, Intelligence, Height and Weight constant. I thus obtained the answer in four more days: namely, that the nine physical tests do measure a fairly prominent general factor with group factors common to the running tests, the jumping tests and the heaving tests; but that the standard Agility Test is very poorly saturated with the general and with the more specialized types of physical capacity, so that the proposed tests could not be substituted for it. Obviously, the statistical analysis is open to criticism on the grounds of unreliability of data, illegitimacy of applying product-moment correlation and factor analysis to non-normal distributions, and so forth. But would any other approach have given a usable result in the time, and is it likely that such a result would have been appreciably different from mine?

This was perhaps an extreme case, but the urgency and the shortage of skilled assistance were ever-present circumstances. True, the Army D.S.P. possessed an excellent statistical department in which, at times, as many as thirty people were working, and the Senior Psychologist of the Royal Navy had a rather smaller organization. Though electric calculating machines were provided, it was only rarely that Hollerith or other mechanical tabulating devices were available for our work. During the past four years I must have calculated some 8,000 correlation coefficients of all types, with a median population of about 200 recruits, and I did the tabulations myself for half of them. In addition, there were roughly 1,000 factor analyses, multiple correlations, corrections for multivariate selection and analyses of variance. I should add that I thoroughly enjoyed all this, together with the other aspects of my work, such as test construction and training investigations. But it helps to explain why my paper is mainly concerned with short-cuts, unsolved problems, and practical applications, rather than with any fundamental contributions to methodology.

Types of data

It is desirable first to outline the kinds of data with which we were chiefly concerned, since the peculiarities of the distributions largely determined the peculiarities of methods of analysis. Although the abilities and attainments and other qualities of the recruits were probably normally distributed in the great majority of instances, our measurements of these variables were frequently abnormal. Five main types may be mentioned.

(i) *Test scores.* Tests of general intelligence and of practical, mechanical, spatial and other similar aptitudes can, of course, by suitable selection of items, always be made to yield normal distributions in unselected groups. But it is considerably more difficult to devise good hard items than average or easy items; a difficult reasoning problem or a mechanical item is apt to become unduly specialized or intricate. Moreover, the time limit for a test has to be lengthened if the top end of the scale is to be covered adequately, and this is a grave disadvantage when the total time available for tests is severely restricted. From the standpoint of selection, also, we are more interested in discovering the poorest than the best men. Thus, for many reasons, tests which give negatively skewed distributions of scores are the most practically useful.

Educational tests measuring verbal or mathematical abilities, on the other hand, always tend to yield positively skewed distributions. Roughly one-quarter of the population left school before 14, or were below the top elementary class if they left at 14; about one-half reached the top class and left at 14; only one-quarter received any secondary education, only 6 per cent. achieved School Certificate standard, and less than 2 per cent. reached University. But the men and women with higher education do vastly better than the other 75 per cent. on tests involving education.

These, and certain other minor, reasons explain why it was the exception rather than the rule for our standard tests to yield normal distributions. Two of the best Army tests, in fact, actually gave an almost rectangular, and a slightly U-shaped, distribution.

(ii) An important part of our work was item analysis of new tests. With very few exceptions, the tests were objectively scored, so that recruits could only obtain a right mark or zero on each item. Item distributions were therefore 2-point ones, ranging from 1 or 2 per cent. right on the most difficult items, through 50, to 99 per cent. right on the easiest ones.

(iii) A good deal of other information was collected about each recruit besides his test scores. Age distributions varied widely during the course of the war, according to the Ministry of Labour's policy in reserving and de-reserving older groups. Incidentally, this constituted a continual source of difficulty in establishing test norms. Educational standard was assessed, in the Army, on an 8 by 2 scale. The 2-scale referred to academic on the one hand *versus* technical or vocational on the other hand. The 8 points referred to the standard reached—University degree, Higher Certificate, School Certificate, top class elementary, and so on. As already mentioned, this scale is strongly positively skewed, and the 2-fold sub-classification added to the complexities of this variable. Civilian occupation was an important piece of information, which often needed to be classified under one of half a dozen or so broad groups, for statistical purposes. It is doubtful whether we are any further forward in establishing a satisfactory psychological classification than we were in 1941. Army recruits were, in addition, assessed for certain character qualities, such as aggressiveness or lack of it, for potentiality as future officers, and for liability to nervous breakdown (the latter being based on an interview by a psychiatrist). Such data generally yielded 2-point distributions, with about 5 per cent. of recruits in one category, 95 per cent. in the other, or perhaps a 3-point distribution with 5, 90 and 5 per cent. respectively. Finally, the recruits recorded on a questionnaire their spare-time interests and any useful experience they had had, in industry, motor-driving, scouting, and so forth. Here, too, the distributions were generally 2-fold, and ranged, for example, from about 80 per cent. stating that they liked reading and football, to less than 10 per cent. liking acting and radio repairs.

(iv) The criteria, against which the value of our tests and of selection procedure in general were gauged, often consisted of marks awarded during training courses. Sometimes such marks were badly skewed (two specimens are given in the third and fourth pairs of columns in Table I), and more often than not they showed variations in standard from one class or squad to another. When the variations were very large it was necessary to express the marks as relative to each squad. A conversion based on percentiles was usually more appropriate than turning the marks into sigma scores. For example, one quick, though crude, method was to reduce the marks to a 3-point distribution, and to put the top third of any squad in the top category, regardless of the absolute level of the men's marks, the middle third in the second, and the bottom third of all squads in the last category.

When the trainees received several sets of marks it was often found that, although the Training School added the whole lot together, they were better grouped, on the basis of factor analysis, into two or three distinct sets. Thus among signallers and telegraphists the marks for Morse and other practical work showed little agreement with marks for theory and bookwork. In one instance

marks on two theory examinations ranged from about 15 to 95 per cent., Morse and other practical marks from about 75 to 100 per cent. The Training School solemnly added all these together to give a total mark out of 500, which was of course almost wholly determined by theory. Closer enquiry revealed, however, that the School took little account of its own totals and only failed those men who did not reach a sufficient standard in Morse.

Clearly it was necessary to scrutinize any sets of marks submitted to us, and to break them down and recombine them in various ways before we could accept them as adequate criteria of the recruits' proficiency. Sometimes no marks were provided, only Pass or Fail results, and here, too, variations of standards were troublesome. Thus in three Driver Training Schools failure rates over a considerable period were 16½, 23½ and 7½ per cent., though we knew that the Schools were receiving trainees of just about the same average quality. The differences are highly significant (f-ratio = 18·37 with 2 and 1,085 degrees of freedom). In such cases we found it best to correlate tests with criteria separately in each group and combine the resulting coefficients through the z-transformation. Another problem which was never satisfactorily settled was what to do with recruits who passed after taking part of the course over again. Their original marks might be below the borderline of, say, 50 per cent., but on re-sitting they might obtain 70 per cent. My own solution was usually to group these men in a single category between those who passed at their first attempt and those who failed outright.

(v) Finally, it was often necessary to use gradings or assessments of the recruits' efficiency in the absence of any reliable examination results. Thus among thousands of seamen, instructed in classes of 30, the instructors picked out for us the top 3 and bottom 3 men who, in their opinion, were most and least likely to make good seamen. Here, then, we had a 3-point distribution—10 per cent. of bests, 80 per cent. ungraded in the middle, and 10 per cent. of worsts.

In many investigations we were able to get the instructors or officers to grade on a 10, 20, 40, 20, 10 per cent.—that is, a 5-point—scale. Such assessments are, of course, purely relative ones. But the danger of accepting absolute gradings from Service officers is shown by the two distributions compared in the last column of Table I. Obviously the standards corresponding to A, B, C and Fail are quite different at the two Boards. (It should be added that these figures were collected before the present, more scientific, system of officer selection was instituted.)

Several other techniques have proved useful. A.T.S. officers and instructors are often willing to rank their squads, particularly when the names are given them each on a separate card so that they can be shuffled about. Up to 30 can be ranked at a time, and these rank orders then converted into sigma scores. Sometimes it is better not to ask for gradings, but for a psychologist to hold a conference with the officers who know the men best, to discuss each man's qualities and defects, and for the psychologist himself to decide which men are, say, outstanding, above average, average, below, and definitely poor. But the most widely used technique consists in splitting the conception of efficiency into a number of descriptions of specific qualities; anywhere from 6 to 30 qualities have been used. A conference is held and each recruit is given a +, 0 or − grading on each of the items after discussion among the officers who know that recruit. A representative of D.S.P. guides the conference and sees that the most junior officer or N.C.O. present always gives his opinion first. The method is quicker than it sounds, and it yields very satisfactory data from the statistical viewpoint. For the items can be inter-correlated and submitted to factorial analysis; scores on the most useful ones can then be combined to give a really adequate distribution of proficiency.

Correlational techniques

From the preceding section it may be seen that the variables to be inter-correlated were seldom suitable for product-moment technique, though sometimes the test scores, gradings, etc., could be redistributed into approximately normal form. Product-moment correlations were nevertheless used more than any other, because of their greater reliability. Yet they are not always the most appropriate for selection investigations, since regressions are frequently non-linear. For example, a test of aptitude for Morse correlated fairly well with subsequent Morse proficiency at the top end of the scale, but not at all at the bottom end. And as this test was used primarily for eliminating men unlikely to learn telegraphy, a product-moment coefficient based on the whole range of scores was definitely misleading. Another consideration was that our experimental populations were often very large, so that product-moment correlations took a long time to tabulate by hand, and quicker, even if less reliable, types of correlation were perhaps justifiable.

How were we to deal with our numerous 2-, 3- or 5-point distributions? Some statisticians would claim that χ^2 tests of significance of association would be the only legitimate form of analysis.* But χ^2 is very awkward to handle when one wishes to try the effects of combining several tests into a diagnostic battery. One makeshift was to convert χ^2 into contingency; then a simple correction based on numbers of categories yields a correlation ranging from 0 to 1·0, which is fairly closely comparable to a product-moment coefficient.† But it is obviously undesirable to use such coefficients in multiple correlation and factor analysis calculations. A far better solution is to turn the distributions into centroids or, in Kelley's terminology,‡ mean class values. Correction for coarse grouping is automatically allowed for, by substituting σ for each σ^2 in the denominator. The technique is a lengthy one, though the centroids themselves can be read off with sufficient accuracy from a table; § nevertheless it is preferred now for all our more important calculations.

When an extended distribution was to be compared with a 2- or 3-fold one, I generally resorted to biserial correlation. This is, however, inappropriate for correlating, say, test scores with Pass-Fail results, since it is based on the wrong regression line. We do not require to know how much poorer at our tests are men who fail their training than men who pass; rather we wish to find how much worse low test-scorers do in their training course than high scorers. The method is more suitable therefore for comparing, say, spare-time interests (a 2-fold distribution) with course *marks* (an extended distribution), also for finding the consistency of separate items in a test by correlating each with the total score on that test.

A useful extension of biserial r was worked out for us by Professor Burt for dealing with our results from bests and worsts. Given, for example, nothing but the test scores of the most and the least efficient 10 per cent. of recruits, the following is one of the formulæ at which he arrived:

$$r_{\text{bis}} \quad \frac{M_B - M_W}{\sqrt{4\sigma^2 \frac{z^2}{n} - \frac{xz}{p}(M_B - M_W)^2}}.$$

M_B and M_W are the means of the bests and worsts, σ the standard deviation of their combined distribution. Such coefficients, however, are poor in reliability, their Standards Error being rather larger than those of product-moment correlations based on a population of the same size as the bests and worsts combined. In most of our investigations, therefore, we did our best to obtain fuller information about the middles, and not only the two extreme groups.

A disadvantage of biserial correlation is that when, as so often happened, the extended distribution is skewed, the coefficients are apt to become exaggerated; values greater than 1·0 were sometimes obtained, and this led to the method being banned in D.S.P. Nevertheless, I still consider it a valid and useful technique provided the distributions can be normalized.

The alternative is correlation ratio, which is very readily calculated by analysis of variance. It is, of course, merely the square root of the ratio of sum of squares between groups (*e.g.*, between Passes and Fails on a particular test item) to total sum of squares (of test scores). A serious defect, however, is the dependence of η, the correlation ratio, on the proportions in the two contrasted groups. Thus we cannot compare the consistency ratio of a test item passed by only 5 per cent. of recruits with that of an item passed by 50 per cent. of recruits, since it can be shown that the maximum value of the correlation ratio is only about one-half in the former case what it is in the latter. My own preference in item analysis is for the simple χ^2, but members of D.S.P. staff have carried out extensive studies with correlation ratio and other techniques which will, I hope, be published soon.

Often the distributions on both the inter-correlated variables were so restricted or irregular that the scatter was reduced to 2×2, and a tetrachoric correlation was obtained. The calculation of tetrachorics by means of Pearson's Tables is extremely tedious. Professor Burt provided us with a convenient trigonometrical approximation, but even this was beyond the scope of most of our semi-trained assistants, so that I was led to explore the possibilities of graphical methods. Thurstone ||

* Cf. Chambers, E. G., "Statistics in Psychology and the Limitations of the Test Method," *British Journal of Psychology*, 1943, Vol. 33, pp. 189–199. Also: "Statistical Psychology: A Plea for Scientific Method," Paper read to the British Psychological Society, September 22nd, 1945.

† Cf. Guilford, J. P., *Psychometric Methods* (McGraw-Hill, 1936), p. 359.

‡ Kelley, T. L., *Statistical Method* (Macmillan, 1924), pp. 168–171.

§ Garrett, H. E., *Statistics in Psychology and Education* (Longmans, Green, 1937), Table XXVII.

|| Thurstone, L. L., *et al.*, *Computing Diagrams for the Tetrachoric Correlation Coefficients* (University of Chicago Bookstore, 1933).

points out that tetrachoric correlations are in any case so unreliable that it is merely misleading to determine them to more than two places of decimals. Unfortunately, four variables are included in a tetrachoric scatter such as Table II, namely, p, p', d (or any other cell frequency), and r_t—the coefficient. As graphs cannot conveniently delineate more than three variables, some restriction

TABLE II

Scatter for a tetrachoric correlation

	High	Low	
High	a	b	p
Low	c	d	q
	p'	q'	1·0

is unavoidable. Thurstone's well-known Computing Diagrams get over the difficulty by holding p (or p') constant in each graph, and providing a series of graphs for the values of p: 0·50, 0·49, 0·48, etc. Thus interpolation is required between two graphs if $p = 0{\cdot}485$, for example. An alternative method which I devised was to find the geometric mean of p and p'—say p''. A single graph will then cover all values of p'', d and r_t. When p and p' are nearly equal, this graph alone will suffice; but when they are unequal, a second graph supplies an approximate correction. If the inequality is very great, as when $p = 0{\cdot}5$ and $p' = 0{\cdot}2$ or less, the correction is not accurate enough. Eventually, therefore, a method was adopted for which Professor Burt is again responsible.

Burt's method consists in calculating a/p and c/q. He has shown that for any given value of these two ratios, r_t is but little affected by alterations in the fourth variable, namely p'. Hence instead of Thurstone's forty odd graphs, only four graphs are needed to cover the same range. These were drawn for $p' = 0{\cdot}50$, 0·34, 0·21 and 0·10. As an example of the slow rate of change of r_t with p', when $a/p = 70$ per cent. and $c/q = 30$ per cent., r_t as read off from the four graphs is 0·60, 0·57, 0·55 and 0·50, respectively. Thus interpolation between different graphs is seldom needed, and the average tetrachoric correlation can be worked out and read off by a statistical assistant in about two minutes.

With so quick a method, more than one correlation can often be determined for the same correlation scatter, and the two or more values combined. True, we do not know the Standard Error of an average r_t, but its reliability is certainly greater than that of a single coefficient, and seems to be not much inferior to that of a product-moment correlation. I have sometimes applied this method even to extended distributions where product-moment correlations would have been feasible, in order to save time. Usually I split the distributions close to the 67th and 33rd percentile levels—that is, I compared the top, middle and bottom thirds on each variable, and averaged the two values of r_t. In one investigation, 1,200 Army boy tradesmen had taken between 11 and 18 different selection tests. They had been trained for various trades—fitters, carpenters, electricians, and so on—in small groups under different instructors. Had I correlated all the tests with the later training results by product-moment technique, at least 2,000 coefficients would have been needed. That meant that the work would never have been done at all, as no one had enough time. But by treating all test scores and training marks as relative, and simply tabulating them in three categories, numerous small classes of boys could be combined. The total number of coefficients dropped to 900, and I was able to do all the tabulation and calculation, almost single-handed, in a fortnight.

Factor analysis

Factor analysis constitutes a fascinating occupation, with perhaps as strong an æsthetic appeal as any branch of statistics. But in selection work its practical value is decidedly limited. It is genuinely useful for classifying sets of marks or grades awarded by Training Schools, as in the example cited above of Morse and bookwork among telegraphists. The other main sphere of application is in the development of new tests, where it helps to determine what abilities are measured by these tests in terms of factors defined by old-established tests. Knowledge of the factor content of a new test does not, of course, tell one much about its usefulness for predicting efficiency in naval or army jobs. Nevertheless, as a general rule, no test is likely to be vocationally valuable unless its communality is high when it is factorized as a member of a varied battery of tests. For example, a mechanical

or manual test which does not overlap strongly with other mechanical tests, and does not obtain high general or group factor loadings, is unlikely to be of much use in the selection of mechanics.

The factorial technique most commonly employed has been Thurstone's Centroid technique,* or what Burt † denotes as Simple Summation. But we have seldom carried out rotation of axes as Thurstone does in an attempt to attain so-called " Simple Structure "—that is, a set of orthogonal common factors each accounting for 10 to 20 per cent. of the test variances. For our results have pointed inescapably to a theory of mental structure more akin to that of Spearman than Thurstone, in spite of the fact that no Navy or Army psychologist or statistician was an ardent disciple of Spearman. Over and over again our test inter-correlations have yielded a general factor running through all the tests and covering 30 to 40 per cent. of variance, together with two, three or more group factors, each confined to a few tests and collectively covering less than 20 per cent.

Thurstone's technique, with little or no rotation, provides a good indication of what group factors are present; and my own practice has been to go on from here by Burt's, Holzinger's ‡ or other group-factor techniques in an endeavour to determine general and group factor loadings which would, when multiplied out, duplicate the original correlation matrix as closely as possible.

No doubt the main reason for the difference between our conclusion and those of Thurstone and other American psychologists is that we have been working with such heterogeneous populations. Probably we have had at our disposal more representative samples of the whole adult population, male and female, than any pre-war factorist ever had. In contrast, Thurstone has chiefly worked with highly selected groups of University students and secondary-school pupils. We have obtained ample confirmation of the point, which Thomson has made so clearly,§ namely, the distortions introduced into the factors by selectivity among some, or all, of the tests.

Much light has been thrown on the nature of the group factors which, although more prominent than Spearman himself realized, are, as I have stated, of very limited magnitude. A remarkably consistent grouping of tests into two main types—verbal, arithmetical and educational on the one hand, *versus* practical, mechanical and spatial on the other hand—recurs again and again. Not only among unselected soldiers and sailors, but also among skilled tradesmen, officers, even among African natives, similar group factors emerge. Curiously, however, this patterning or structure of abilities seems to be less clear-cut among women. These broad group factors are, in addition, found to break down or sub-divide indefinitely into smaller types whenever suitable batteries of tests are analysed. Thus arithmetical, scientific, literary and clerical sub-group factors have been discovered within the verbal-educational complex, and the mechanical-spatial complex has yielded relatively distinct mechanical information, manual, and visuo-spatial sub-factors. No other group factors have been found with such wide scope as these two, but a physical capacity factor was mentioned at the beginning of my paper, and other minor ones have suggested themselves when batteries of more specialized tests have been studied.

Multiple correlation

As Thomson ‖ has shown, multiple correlation technique is more important in selection than factor analysis, since it enables us to calculate how several tests will work in combination. Thus the general intelligence test which has been the most widely used in the Navy and Army, having been applied to some three million men and women, is gradually falling into disuse or being omitted altogether, since we have found so repeatedly that a battery of verbal-educational and spatial-mechanical tests covers the same ground more adequately. In combination with them the general test usually has a zero or slightly negative β coefficient. Again, in trying out any new test for a particular job—say motor-drivers, mechanics, Radar operators—the validity of the test itself is a secondary consideration. The chief point to be investigated is whether this test adds enough to the standard battery, or raises multiple r sufficiently, for the time and trouble it involves to be worth while.

Now, most psychologists would suppose that multiple correlation would tell us how to weight

* Thurstone, L. L., *A Simplified Multiple Factor Method* (University of Chicago Press, 1933).
† Burt, C. L., *The Factors of the Mind* (University of London Press, 1940).
‡ Holzinger, K. J., *Student Manual of Factor Analysis* (University of Chicago, Department of Education, 1937).
§ Thomson, G. H., *The Factorial Analysis of Human Ability* (University of London Press, 1939).
‖ *Op. cit.*

our tests in order to predict efficiency in a variety of jobs. For instance, verbal tests are likely to be most relevant in selecting clerks, mechanical-spatial tests in selecting mechanics, and the β coefficients should show us how the tests in our standard battery can be most effectively combined. We have, indeed, with this end in view, determined literally hundreds of multiple correlations, using efficiency at different Navy, Army or A.T.S. jobs as the criterion. Here again Professor Burt has assisted us by introducing what he calls the techniques of Single Division and Complete Division, which are considerably simpler than Doolittle's, Aitken's and other methods. The method of Complete Division can be applied even by assistants who have not sufficient training to work out standard deviations and product-moment correlations.

Most of our validation has to be done with selected groups—that is, we can only find out if our tests for, say, mechanics, are working well by following up the careers of mechanics who were originally chosen largely on the basis of these tests. Thus it has become standard procedure not only to determine the multiple correlation, but to correct the original validity coefficients and the multiple correlation for selectivity or undue homogeneity in the population, adopting the technique of correction for multivariate selection which Burt has described.*

The more widely these methods have been used, the less satisfactory do they appear, and the more difficulties they raise—difficulties which, unfortunately, we have never had the time to sit down and consider seriously. In the first place, working out all the inter-correlations or covariances of the battery of selection tests in each group of recruits is exceedingly lengthy. Moreover, some of the groups, engaged in rather unusual jobs, may be unavoidably small, even less than 100, and this means that the inter-correlations are far from reliable. On occasion, therefore, I have preferred to take over the covariances from another, larger group—provided that its means and variances are much the same as those of the actual experimental group, both because these larger-group covariances are more reliable, and because of the consequent saving in time. Yet this make-shift is obviously open to criticism.

Next, it is not always realized that with smallish experimental groups, multiple r is inevitably exaggerated. For when the validity coefficients have considerable Standard Errors, some of them are likely to be unduly large, others unduly small, and it is always the largest validities which determine the size of multiple r.

Thirdly, multiple correlation often gives the misleading impression that certain tests are much more valuable than others. Unless the experimental population is very large, the β coefficients have low reliabilities, and it can easily happen that a test which is apparently valueless in one group of subjects jumps to being one of the most valuable in another, parallel, group. This is especially likely to occur when the test battery contains three or four highly inter-correlated tests. All these may show similar correlations with the criterion, but the one which, perhaps by chance, surpasses the other two usually achieves a far higher β coefficient. It is simply not true that this test is very valuable, the others valueless. More reliable predictions of efficiency would certainly be obtained from all three.

Thus multiple correlation technique is, in fact, too efficient, since it shows how the very best predictions can be obtained in the particular experimental group. Only if this group is extremely large, and if other factors, such as the quality of intakes of recruits and the nature of the job and its training, remain constant (factors which are apt to vary rapidly), would it be justifiable to weight the battery of selection tests strictly in accordance with the obtained β coefficients. Often, then, a much rougher procedure, based on Spearman's Correlation of Sums formulæ,† and on the habitual inter-correlations of the tests in other similar groups, will provide an equally good guide as to how the tests are likely to work in combination.

The next difficulty is that we have not, like American Service psychologists, machines for weighting test scores, and it would take our officers and N.C.O.s or Petty Officers in the reception units far too much time to calculate weighted scores for numerous different jobs. Instead the Army has adopted the method of minimum standards on the most relevant tests. To be acceptable as a clerk, a recruit should be above a certain grade on the Clerical, Arithmetical, and other tests, but need not score highly on mechanical tests; similarly with other jobs. Now, while this method does weight the tests, it does not duplicate multiple correlational findings. It is a substitute whose

* Burt, C. L., "Validating Tests for Personnel Selection," *Brit. J. Psychol.*, 1943, Vol. 34, pp. 1–19.
† Cf. Kelley, *op. cit.*, Chap. IX.

validity is definitely lower than that predicted by multiple *r*. Only in one, particularly important, job have we been able to use summed, weighted scores, and this procedure has broken down rather widely because many of the officers are utterly sceptical of negative weights. Nothing would persuade them that better predictions could be obtained by subtracting scores on certain tests from the summed scores on certain other tests.

Note that minimum standards always imply a high regression coefficient at a certain point in the distribution. But I mentioned earlier that our regressions are frequently non-linear, so that a multiple correlation based on the whole range of scores may or may not be a true guide when it is a matter of cutting out the poorest 10 or 20 per cent. of recruits. For these and other reasons the Navy seldom resorts to minimum standards, and prefers to judge a recruit's capacities from his score on all the standard tests equally weighted, though paying more attention to one or two specialized tests when considering him for a specialized job. In both Services, it should be added, selection or rejection is never a matter of test scores alone; a recruit's interests, previous experience, and temperamental traits as judged in an interview or assessed from his work record, are all considered together before his suitability is decided.

This brings us to corrections for homogeneity or heterogeneity. The object of Burt's technique is to deduce, from the correlations of tests with a criterion in a selected group, and from the test variances and covariances in the selected and unselected populations, what would be the correlations with the criterion in the unselected population. Its value in vocational investigations is lessened, however, by the fact that selection is never based solely and explicitly on the battery of tests. In the Navy and Army various interviewers pay varying amounts of attention to the test scores, some being more influenced by the other considerations I have mentioned. Moreover, the variances in the selected group may be reduced not so much because selection was based on the tests as because other factors, whose selectivity we may not know, such as education and civilian occupation, have brought about indirect selection. An additional difficulty is that most tests do not show normal score distributions; a positively skewed test, for example, may actually possess a higher variance in a superior, selected, group than in an unselected group.

It must be admitted then that we are very rarely able to make adequate allowance for selection, and that a reliable picture of the worth of the selection tests could only be obtained by following up unselected men. Table III gives a good example of the effects of selection and of modifications in the criterion which occur only too often in everyday practice. A new mechanics branch was

TABLE III

Correlations between tests and job proficiency among the first 1,100 *trainees*

Test No.	First 300	Next 300	Next 500
0	0·623	0·399	0·255
1	0·465	0·568	0·294
2	0·527	0·352	0·329
3a	0·553	0·379	0·164
3b	0·638	0·426	0·369
4	0·377	0·282	0·276

started, and the first 300 men sent for training were almost unselected, as we knew little about the requirements. The six selection tests—their content does not concern us at the moment—gave remarkably high coefficients. With the next 300 our Selection Officers knew something about the job, and most of the coefficients fell markedly; but the syllabus also altered, and this, together with sampling errors, probably accounts for the higher validity of Test 1. As the Training School got used to the better quality of well-selected trainees, the syllabus was further stiffened, and in the third group of 500 the correspondence of our tests with the criterion is decidedly poor.

It is not possible, in this paper, to discuss the intricate matter of fixing minimum standards with a view to achieving minimum wastage. It is desirable, of course, to cut out as many potential failures as possible with the smallest possible sacrifice of good men. In spite of considerable investigation, there are still many unsolved difficulties when one is dealing with selected groups, skewed distributions and imperfectly linear regressions.

Analysis of variance

Doubtless it will be asked why our work appears to have been based almost solely on correlation methods. Actually this is not the whole story, but I have omitted applications of χ^2, the determination of test norms, of means and group differences, and the like because they did not raise any novel points of particular interest. Analysis of variance and covariance has been widely used, though to a lesser extent than might be thought desirable, for the following reasons. First, the vocational psychologist has little opportunity to impose good experimental design. Masses of data are submitted, often in most inconvenient forms, and he must make the best of them. For example, the success of Army officers in the field is influenced by many factors, including age, length of service, the Arm in which they are commissioned, and it may, or may not, correlate with the grades assigned to these officers when they were candidates passing through the selection procedure. But it was quite impossible to collect even small groups of officers of each age, each length of service, in each Arm, and therefore impossible to estimate the variance contributions of such factors in combination. In such situations, partial correlation often yields a more hopeful approach. Other minor reasons are the comparative rarity of assistants who can carry out even routine analyses competently, the shortage of calculating machines which are vital here but are less essential for many correlational calculations, and lastly, the very great difficulty which the layman or partially trained selection officer has in grasping the meaning of an analysis of variance. Unfortunately, the sales value of our work had to be considered frequently, and many people who think (mistakenly) that they understand correlations, shy at any mention of variance and F-ratios.

Nevertheless there are numerous everyday applications where the data provide their own design—for example, the significance of variations between classes or instructors in training-school marks; variations in the results of a test of vision when given in different testing centres; variations in efficiency between men drawn from different broad occupational groups; and so forth. A very common problem in multiple correlation was solved by Mr. Slater as follows. Recruits may be trained at two, three or more similar jobs, such as operating different Radar sets or firing different kinds of guns. A battery of selection tests is correlated with each type of proficiency, and it is found that the patterns of test validities are somewhat, though not exactly, similar. Does this mean that the tests should be differentially weighted, or accorded different minimum standards, for each job, or will a single set of weightings for all jobs give predictions which are insignificantly inferior? The technique is to calculate the multiple correlation in each job group, and in all groups combined. Analysis of variance then readily shows whether separate regression equations for each group cover significantly more variance than a single equation for all groups.

Finally, we have occasionally been able to arrange our own investigations. Thus in one study of the use of films and filmstrips for training seamen, I was able to analyse the contributions of these visual aids, of good and weak instructors, and other minor factors, and to allow for variations in intelligence of the trainees by analyses of covariance. In the Army there have been studies of the effects of remedial physical training on physical status, intelligence and achievement, with due allowance for improvements due merely to practice at the tests; and similarly the effects of special education courses for illiterate recruits have been analysed. The effects of menstruation among A.T.S. on test performance was a particularly intricate study. For these, and numerous other developments of analysis of variance, Mr. Slater was statistically responsible, and accounts of them will, I hope, soon be published.

Discussion on Dr. Vernon's Paper

Professor Burt: I have great pleasure in proposing a vote of thanks to Dr. Vernon. As a member of the War Office Advisory Committee on Personnel Selection, I have been in touch with his work from its inception; and have always been astonished at the skill and ingenuity with which he has attacked the new and innumerable problems to which his work gave rise. To-day he has presented us with a review which has been, I am sure, as deeply interesting to statisticians as it has been to psychologists. The questions he has raised are by no means matters of the past; they are likely to be of much importance in the work that awaits us in the immediate future. As he has pointed out, the statistical methods taken over for work on personnel selection during the war had largely been elaborated during earlier investigations carried out in the field of child psychology; and it has been most encouraging to those of us who had some small hand in developing those

methods to learn how successful they have been in these newer tasks. But, in much the same way, the further experience gained in these war-time applications will in turn be of special value in our new peace-time problems—in education, in industry, and in commerce.

Those of us who attempt such studies in the laboratory suffer from one great disadvantage. The samples that we are able to test are either very small or else highly selected—usually both: they consist, as a rule, either of children at school, or of students at the University. Dr. Vernon, on the other hand, has been able to analyse data obtained from exceptionally large samples of unselected adults. It has therefore been particularly gratifying to hear how the broad factorial hypothesis, put forward thirty years ago, on the basis of a few experiments on children tested in schools, has been in the main confirmed by his more recent and extensive studies: the hypothesis was that mental abilities include *both* a very general factor, popularly termed "intelligence," *and* a number of more special abilities, technically termed "group-factors." Spearman doubted the existence, or at any rate the importance, of the group-factors; Thurstone doubts that of the general factor. Dr. Vernon finds both: "over and over again" he has (he tells us) encountered "a general factor running through all the tests," and, in addition, "two, three, or more group factors," closely corresponding to those originally discovered in the educational field.

On the other hand, he deplores the fact that, unlike the laboratory psychologist, he has rarely been able to plan his experiments on new techniques. Here, therefore, the laboratory psychologist may still be of service. Indeed, any suggestions that Dr. Vernon can put to us for further work in this direction will be most welcome, particularly in regard to the development of less cumbersome statistical procedures. I note, for example, that in factor-analysis he has found that the newer "centroid" method, developed by Thurstone in America, has proved more convenient in practice than the older technique of "simple summation" developed in this country. It would, therefore, be most helpful if he could tell us (perhaps on some later occasion) in what respects the former has been found more speedy or effective, especially as his use of Thurstone's technique has not apparently led to Thurstone's own conclusions, but rather confirmed the older views.

But to-day, I imagine, Dr. Vernon, like myself, would rather hear suggestions and questions from others present at this meeting. He has, it seems to me, raised two groups of questions for the expert to answer: first of all, how far does the statistician consider these quicker methods, adopted to meet the exigencies of practical work, to be valid and trustworthy; secondly, how far can we trust the more elaborate and refined statistical techniques, when, owing to the peculiarities of the data, the conditions which they presuppose are not strictly fulfilled? Perhaps to a large extent the answer is to be obtained, not so much by theoretical discussion as by a study of information gained in this way from practical experience in the practical field. Dr. Vernon himself has described many of these practical trials—trials carried out on a scale quite unheard of in previous psychological researches; and he has described them with admirable lucidity. We shall all look forward to the more detailed accounts that he has promised as soon as war-time restrictions are relaxed. Meanwhile, we are most deeply indebted to him for this preliminary review; and it gives me the greatest pleasure to have the privilege of moving the vote of thanks.

Dr. Fraser Roberts: I have very great pleasure in seconding the vote of thanks to Dr. Vernon for his admirable paper, because it has been my privilege during the War to see something of the work that he and his colleagues have been doing. And very fine work it has been, work of great interest and value, and now that security restrictions are being lifted one hopes that a great deal of it will be published in the *Journals* and become generally available.

Dr. Vernon has made very clear the difficulties under which psychologists have worked. They were, after all, pitchforked into the urgent practical business of personnel selection after the War had started and while expansion was very rapid, so that again and again, as Dr. Vernon has explained to you, they had to sacrifice the possibility of leisurely long-term experiments in order to cope with urgent, practical, everyday problems. It is a thousand pities that all this work could not have been started long before. It is greatly to be hoped that the lesson has now been learned, and that people like Dr. Vernon can go on with their work under peace-time conditions and be able to plan their experiments as they would wish. I am sure that, admirable as have been the results obtained in war-time, the results of the future will surpass them.

It is perhaps with the future in mind that I should like to say a word about his observations on multiple correlation techniques. In attempting to devise tests for selection, one can consider the problem at two stages. The first is to attempt to predict after, say, an hour's testing what the practical man will discover, say, six months later. This presupposes that one accepts the practical man's examinations or judgments of ability as one's criterion. The further stage is to review the whole procedure of examination and practical judgment as well; that is, to attempt to improve the practical estimation of success. In the circumstances of the War it was seldom possible to reach stage two: that is clearly long-term planning, and one hopes it will now be undertaken, but as regards the first stage, need one be worried by disagreements between practical men? I doubt it. If some sailors are classified as best by some observers and worst by others, which sometimes happens, if there is no correlation between examiners' judgments, then there is no problem—there is no difference between a good sailor and a bad one and there is nothing to predict. But if, as is

usual, there is some agreement, and there may be high agreement, then the technique of multiple regression, or more broadly of discriminant function analysis can be used to select the best tests and allot to them the best weights.

I cannot feel that Dr. Vernon's finding that in one sample test " A " contributes heavily and test " B " very little, while in another sample this is reversed, is any criticism of the technique, which has, in fact, indicated the correct answer. " A " and " B " are highly correlated; it matters little which we use, and the use of both adds little to what is given by one alone.

An empirical approach may lead to the discovery of some unsuspected contribution; there have been instances of apparently very bizarre measurements which, though relatively inefficient by themselves nevertheless contributed effectively to a battery, because these additional tests were measuring something which the other tests were not. One is, of course, up against the old difficulty of finding psychological tests which are not going to measure the same thing over and over again.

Again, the peculiar distribution sometimes encountered may yield to methods of transformation and, of course, it is always possible to use more than the simple linear components. Even brutal normalization may not be unjustifiable sometimes. Above all, I remember a remark by Professor Fisher: " When you doubt your metric, use rank order."

I hesitate to mention my own trifling experiments, but I had the opportunity on one occasion of getting instructors independently to place in rank order classes as big as fifty, simply asking them to place the subjects in the order in which they would choose them for service with themselves in a ship. Although the instructors had emphasized their different points of view, the correlations turned out to be 0·65 or somewhat more. Starting with such a criterion, one can then build up the test battery. I do feel that any attempt to split up the practical men's grading into a number of components is difficult because, of course, one has to select the man as a whole.

With those few observations I should like to say once again how very much I have enjoyed listening to Dr. Vernon's Paper, and I am sure you all realize what an immense lot of fine work there is behind it, and, as I said before, I hope that we shall see much of it published in due course.

THE CHAIRMAN read a letter received from Mr. Alec Rodger, Senior Psychologist at the Admiralty, in which he expressed his regret at being unable to attend; he would have greatly liked the opportunity of paying tribute to the work Dr. Vernon had done for them all at the Admiralty. He felt sure that no one else could have played the same technical advisory rôle with such quick understanding and impartiality.

MR. PATRICK SLATER said he was glad to be able to corroborate or amplify some of Dr. Vernon's points. The paper gave a very just account of the statistical problems encountered in selecting army personnel and of the methods adopted for treating them. He, Mr. Slater, could not speak of problems concerning navy personnel.

The most striking difference between the conditions under which their investigations were conducted and those which obtain in peace-time was one that he would describe by saying that under the conditions in which they worked their reasoning could be conducted in an enclosed universe of discourse. The strongest impression he retained from his army work was that of the elegance and simplicity which could be introduced into a psychological argument when the universe of discourse in which it was conducted could be enclosed.

An instance would explain what he meant. Suppose it was necessary to select men who could be expected to succeed as motor mechanics. After observing what motor mechanics do, and excogitating upon it, they might advance a theory that superiority of mechanical aptitude was one of the characteristics which differentiated successful from unsuccessful mechanics. This theory might lead to endless controversy, over such questions as whether mechanical aptitude was a unitary trait, what were the best means of assessing it if it existed, and so on. Controversies of this kind were demonstrably endless when they concerned traits *underlying* human behaviour, *i.e.*, psychologically postulated traits treated by common consent as outside the range of direct observation.

These controversial questions need not arise in an enclosed universe of discourse. Taking the two propositions:—

firstly, that degree of success as a motor mechanic depended on amount of mechanical aptitude, and

secondly, that amount of mechanical aptitude was measured by score on a certain test,

without attempting to establish the truth of either, the conclusion was that an individual's probability of success could be inferred from his test score. To verify this conclusion experimentally, they followed up a group of men whose test scores were known, and obtained assessments of their success. The results could be used to prepare scales showing the probability of success of individuals with different test scores.

The experimental condition which enclosed the universe of discourse, was that the methods of observation used on the follow-up were those used in the selection procedure. This condition could be satisfied in a large unit like the army, but not in units as small as psychological clinics usually were. It involved standardizing methods of observation and maintaining with adequate

authority a central organization for research. He would generalize by saying that the advantages of reasoning in an enclosed universe of discourse should be taken whenever possible.

If the investigators habitually conducted their arguments in terms which they had designed and standardized for their own convenience, they found they could change the perspective in which they viewed many psychological problems. Precise observations about individuals were required in order to estimate with the smallest possible amount of error what their expected behaviour is under precisely defined conditions. The statement that a man had a score of 48 or S.P. Test 1 meant more than the statement that he had I.Q. of 120; for the former specified the test which had been used and the latter did not. The statement that he was a man of superior intelligence meant even less, for it did not imply that any controlled method of observation had been used. "What is the nature of intelligence?—or of will-power, courage or any other hidden mental trait?"—often described as the fundamental problems of psychology—are questions which there had been no need to consider.

The use of standardized methods of observation led also to a change in methods of statistical analysis. An investigator who designed a psychological test for a private research could modify it to his liking, add new items, discard old ones, and make it longer, shorter, easier or harder. The variance observed in the scores of a group of individuals might mean little to him because the scale on which he measured it was arbitrary.

But once the scale was fixed, measurements in terms of test scores could be treated like measurements in terms of inches or ounces or seconds, gaining the advantages obtainable by maintaining a conventional notation. In fact, the investigators accustomed themselves to considering the relative homogeneity or heterogeneity of different groups of men, to using regression coefficients when once they would have used correlations, and to making much more use of analysis of variance. On the other hand, factor analysis, although a favourite technique of psychologists, was one for which they found relatively few applications.

He took this opportunity of calling attention to an approach to problems of selection which he believed largely original. Using standardized methods of observation, records could be prepared of a representative sample of the individuals from whom some were to be chosen, and of a representative sample of individuals engaged in an occupation to which some were to be directed. Hence the discriminant function could be found, and with it the multiple regression equation which described the appropriate method of selection in terms suitable for application. If several occupations were to be considered simultaneously, the same methods of observation could be used for all, and the samples drawn from different occupations could be treated both in combination and separately, and thus classified into the groups for which similar regression equations were applicable. This, in his opinion, was the general form of treatment most suitable for solving problems of selection. It was based on the assumption that the average man engaged in any occupation was neither too good nor too bad in his work. This assumption appeared to be reasonable.

If the data were treated in this manner there was no need to collect proficiency ratings—ratings which were often subjective and had little relevance to the specific requirements of particular occupations. Furthermore, the regression equations we obtained enabled us to take account of the alternative possibilities that a man might be too bad for an occupation or too good. Regression equations obtained by using proficiency ratings as criteria allowed account to be taken of the first alternative only. Furthermore, they provided no convenient method for comparing the requirements of different occupations.

How to use observations of a crude kind, such as classifications of men into those who gave right or wrong answers to a particular question, or into a few roughly defined grades of ability, were questions which constantly arose, as Dr. Vernon had already remarked. The relationship of an attribute with a dichotomous classification to other attributes or variables could be considered in various ways. To express its relation to a continuous variable in terms of a regression coefficient such as biserial r seemed to him artificial; the variance ratio or the correlation ratio expressed it in simpler terms. If the dichotomized attribute were treated as dependent on more than one independent variable, the discriminant function could be found (allowance should be made for the relative frequency of the two alternatives). If the dichotomized attribute were treated as independent, and its influence in combination with other independent variables on a dependent one were considered, its independent effect could be isolated by analysing the variances and covariances of the remaining variables within and between groups. If large numbers of dichotomized variables were considered simultaneously, four-point correlations could be used. In short, the occasions on which it was strictly necessary to use a biserial correlation coefficient arose in his opinion, very infrequently.

When a biserial r was calculated (using Pearson's formula) from data from which the product moment correlation had already been found, the biserial r was often a poor estimate of the product moment r. Slight irregularities in the form of distribution of the continuous variable might have unfortunate effects. In his experience, tetrachoric r (calculated from Pearson's tables) generally gave a closer estimate, particularly with a good-sized sample, say 400 cases or over, or if Yates's correction for continuity was applied to the frequencies. This was his defence for having used the authority conferred on him by the Director to bann the use of biserial r in the Directorate of Selection of Personnel.

His experience confirmed Dr. Vernon's, that when data were graded on a rough scale, in intervals that could not be assumed to be equal, the most consistent results were obtained by using centroids. Mr. Slater had no experience in the use of the correction which Dr. Vernon mentioned, as he had used the method only when no finer groupings of the data could be obtained, *i.e.*, when there had been no need to speculate on what the variance would have been if the variable had been measured on some other scale.

In conclusion, he desired to confirm that every effort was made by the War Office to provide the facilities needed by D.S.P.'s statistical section, that no requests presenting a reasonable case for additional staff or equipment were ever summarily rejected, and that the Adjutant-General's Statistical Department invariably extended to the section every assistance possible.

DR. IRWIN wished to congratulate Dr. Vernon on the energy and ability he had shown in applying statistical methods under the trying conditions of war-time.

To have worked out 8,000 correlation coefficients, no matter how, and 1,000 factor analyses was in itself an achievement. But he felt that Dr. Vernon had left out one important factor which probably contributed more than anything to the success of his work; that was his own flair for understanding the meaning of his data. Given this last, he would probably have found out much about the right way to select army and navy personnel if he had confined himself to simple tabulations and the working out of averages; without it his correlation coefficients and factor analyses would have been of little avail.

In reading an account of miscellaneous methods of determining correlation, one felt that reference should be made to the original work of Karl Pearson; contingency, tetrachoric r, biserial r, and he thought also the method of centroids were all discovered by Karl Pearson originally as a sort of by-product of his fundamental work. Also he was the pioneer in investigating the influence of selection on variation and correlation.

In the thirties he, the speaker, maintained, and had never really receded from the position, that coefficients of contingency, tetrachoric r, biserial r and the like should be used as sparingly as possible, and never as an end in themselves; because, except for very large samples, one knew practically nothing about their sampling distributions and could not apply tests of significance. He thought this was still the position. And he would like to ask this question: if all sorts of short-cut methods were used to work out correlation coefficients, if distributions were not normal, if samples were relatively small and the results were applied to factor analysis, did anyone know what significance to attach to the results?

This was not really a criticism of Dr. Vernon because, if he, Dr. Irwin, understood his paper, he did not use these correlation coefficients as an end in themselves; he had to use existing methods with all their imperfections, and when he had done his factor analyses or multiple regressions, significance tests not being available, he had to rely on common-sense only.

For instance, the grouping of tests into two main types—verbal arithmetical and educational versus practical mechanical and spatial—would probably have emerged whatever technique was employed.

And, after all, the comparison of the methods of selection he devised with the available objective criteria must have provided a check. But where there was a failure of a series of tests to correlate with an objective criterion, it must sometimes have been difficult to know whether the tests or the criterion were at fault. When he, Dr. Irwin, read that the practical marks in Morse ranged from 75 per cent. to 100 per cent., he was reminded of a similar difficulty experienced by Mr. Farmer many years ago when he was trying to find tests for selecting naval gunners and the only objective criterion was a test in which everyone scored about 80 or 90 per cent.

Finally he would like to end as he began by congratulating Dr. Vernon on the success of his work.

SERGEANT KINSMAN said that, speaking as one of the semi-skilled assistants to whom Dr. Vernon had referred, he agreed that there had certainly been snags in the work they had carried out, particularly in dealing with high correlations. He did not presume to say which was right or which wrong, but there was, for instance, the question of centroids. They had found in a number of cases the centroid yielding a correlation of over 1·0, so in those particular instances at least the centroid method had to be cut out. They had then gone on to the graphical method, which was extremely handy to use and very fast, but there were two snags to it: one was with correlations of over 0·95, when it was impossible to interpolate at all, and the other was when there was a cell with zero.

When it was impossible to use the graphs one had turned to Burt's trigonometrical method. This, too, had been found to yield correlations of over 1·0. Finally, coming down to Pearson's tables, these could not be used if there was a zero in the cell in the bottom right-hand corner.

MR. KENDALL thought he knew less about psychology than anybody in the room, but there were two types of problem which were thrown up by this kind of work which the theoretical statistician ought to tackle. He agreed with Dr. Irwin as to the general unreliability of the short-cut methods of correlation, but the plain fact was that, however unreliable they were, the psycholo-

gist had to use them. In the past psychologists had been dealing with rather small samples; in the future, if this work was carried out with plenty of staff, it ought to be possible to discover something about the sampling variation of these coefficients by experimental means.

A problem which was thrown up peculiarly by psychological work, and was also occurring at the present time in various other fields, arose when one suddenly added another variate and found that all the coefficients in a regression equation altered considerably. If one had a scatter of points in n dimensions and fitted a number of planes to them by any of the approved techniques, it was possible to account for the major part of the variation in a certain lower number of dimensions. But some of the points would lie slightly outside those dimensions, and the question was how far this could be regarded as a sampling effect—was there, in fact, a sampling variability in a dimension number? or, in psychological terms, how many factors must one take before the data are "satisfactorily" described? He hoped that this peculiar and interesting problem would be taken up.

MR. A. H. J. BAINES said he understood that in factor analysis Dr. Vernon employed simple summation followed by a group factor method. In Dr. Vernon's hand the latter technique was without doubt a most powerful weapon, but in his, the speaker's, its dependence on personal judgment might be dangerous. He had found that a group factor analysis might give rise to a communality which was impossibly high in view of the known reliability of the test concerned. If the underlying assumptions were varied, the impossibility might merely shift from one test of the battery to another. In such a case he would be inclined to make a linear transformation of the simple summation analysis, to bring it as nearly as possible into line with the group factor analysis. He was aware that such a transformation would not in general be strictly a rotation, but it would approximately preserve the communalities of the simple summation analysis and avoid the paradox he had just mentioned. The transformed factor loadings corresponding to zeros of the group analysis might indicate the presence of small group factors not allowed for in that analysis. He hoped that this treatment was in accordance with the views of Dr. Vernon and Professor Burt.

DR. VERNON expressed his gratitude to the Society for the honour it had done him in inviting him to address the meeting, and also to Professor Burt and Dr. Fraser Roberts and others for the kind remarks they had made.

Replying to some of the points raised, Dr. Vernon said that Professor Burt had contrasted the work of the laboratory psychologist, who can carry out carefully designed experiments under fully controlled conditions, and that of the vocational psychologist in the Services who has to work at high speed and produce results of immediate practical utility. There are grave dangers in the latter approach. As one member of Personnel Selection Staff had expressed it, "we have become psychologically and statistically illiterate." If this work continues in peace-time, it is most desirable that Service psychologists should have more time for fundamental research and should maintain closer contacts with laboratory psychologists.

Referring to Dr. Fraser Roberts' remarks on multiple correlation, Dr. Vernon entirely agreed that this technique (or the discriminant function technique) is of value for choosing the best selection tests out of a large battery. But for the shortage of staff and lack of time, Service psychologists would undoubtedly have devised many more tests, and studied their predictive value in particular jobs. The difficulties that he had described in multiple correlation methods arose when a single battery of some five to ten standard tests was given to trainees in numerous jobs, and it was desired to determine the appropriate weightings of the component tests in the battery for the various jobs. While he would dispute Mr. Slater's assumption that the average man engaged in any occupation is neither too good nor too bad at his work, he had often employed criteria of this type in studying the relative value of tests in a battery, and had generally found the results to coincide closely with those obtained when other criteria, such as proficiency ratings, were used.

Sergeant Kinsman's difficulties with tetrachoric and centroid coefficients are likely to arise only when the populations are very small, and the distributions on the correlated variables so irregular that no correlational technique can satisfactorily express the agreement between them. Mr. Baines's method of linear transformation of simple summation factorial analyses clearly possesses statistical advantages over his (Dr. Vernon's) group factor method. But as it is guided by the previous group factor analysis, it too must depend rather largely on the analyst's personal judgment.

Dr. Irwin and Mr. Kendall commented on our ignorance of the sampling distributions of "short-cut" correlations. He would point out, first, that in many investigations more orthodox correlational, chi-squared, or analysis of variance techniques were applied, and the ordinary tests of significance were therefore applicable. Secondly, he would recall the essentially pragmatic approach of the psychologist in the Services. It was only rarely that fine discriminations were needed; much more often a correlation, or other statistic (which was based on populations numbering several hundreds), was so obviously significant that conclusions derived from it could be applied forthwith, or else it was too small to possess any practical value.

Finally, Dr. Vernon said that there were good prospects of Service psychologists and statisticians being allowed to publish their work, and of similar work being continued, in a more leisurely and more thorough manner, during peace-time.

The Application of some Commercial Calculating Machines to Certain Statistical Calculations

By H. O. Hartley

[Read before the Research Section of the Royal Statistical Society, June 5th, 1946, Dr. J. Wishart in the Chair.]

Introduction

It seems to be a popular belief that a statistician must, of necessity, be good at figures. This may be true of some statisticians, but is certainly not the rule. Many good statisticians loathe figures and are weak at mental arithmetic; others are good at it; others, again, are indifferent. Let us hear what L. McMullen says in his appreciation (13) of the great "Student":

> "It might be supposed from the amount he did in the time, that he [Student] was unusually good at arithmetic and the arrangement of work; this, however, was not the case, for his arithmetic frequently contained minor errors. In one of his obituary notices a tendency to do work on the backs of envelopes in trains was mentioned, but this tendency was not confined to trains; even in his office much work was done on random scraps of paper. He also had a great dislike of the tabulation of results and preferred to do everything from first principles whenever possible."

Later on McMullen relates an amusing incident:

> "When he handed over to me a routine calculation which he had done for many years, I was astonished to find that he had written out every week an almost unvarying form of words with different figures. To my question, 'Whyever don't you get a printed form?' he did not reply, 'Doing it from first principles every time preserves mental flexibility.' He would have considered such a remark unbearably pompous. He said, 'Because I'm too lazy,' to which I replied, 'Well, I'm too lazy not to.'"

There are here two diametrically opposed opinions, both justified at the time when they were formed. When a new statistical calculation is carried out for the first time it would seem a waste of time to think about designing a form or to get one printed, only to find perhaps that after two or three applications some essential features in the data have to be altered and the form scrapped. Yet when one finds that the work has developed into a standard routine, a printed form will obviously save a great deal of labour.

A printed form is perhaps the simplest computational aid, and the little story I have told about one illustrates an important point affecting all such aids and procedures. They are all intimately linked with details of organization; in particular their efficiency and economy depend entirely on the size of the job and the frequency with which a particular calculation has to be performed. The research statistician must bear this constantly in mind. His task is, in this respect, more difficult than that of his colleague who is concerned with large-scale routine work of a set and unvaried character.

The research statistician may have to carry out a new analysis not knowing whether it will ever be repeated. It may never occur again; it may be the first of a large number of similar analyses. He cannot afford to wait until such a decision is reached—indeed, the decision about repetition often depends on the result of the "pilot." The varied and changeable character of research and development work therefore necessitates a flexible and adaptable organization of computing methods. It is clear, therefore, that it is impossible to advocate a "best computing method" for any particular statistical calculation. If somebody were to ask, "What is the best way of calculating a correlation coefficient?", I am afraid the answer would be the Joadish, "It all depends what you mean by correlations!" Is it a single correlation coefficient calculated from (say) 30 pairs of two-figure observations? If so, is this calculation to be repeated daily, weekly, monthly or annually? Are the 30 observations multivariate sets, having each 5 variables, 10, 20 or even 100 varieties, and are all intercorrelations required? Is it only a selection of intercorrelations that is required? In which form are the data recorded? Are they scattered in note-books, are they tabulated on manuscript sheets, or have they been punched on Hollerith

cards? All this affects the choice of method, the equipment to be used and, indeed, the time to be spent on planning the computational method to be employed. It is obviously foolish to spend a day planning a more efficient way of doing a particular calculation only to find that the work could have been completed by an existing inefficient method in a fraction of that time.

We must therefore at the outset disappoint the reader who expects a clear-cut answer as to the best computational aid and best method for any statistical calculation in all circumstances, and will state immediately the very limited programme of this paper. Only commercial calculating machines are dealt with (leaving out, therefore, machines or gadgets specially built in small numbers for statistical calculations). The use of these machines is illustrated in Part I by selecting one important type of statistical calculation—multi-variable analysis. This problem has been selected because here heavy computing occurs fairly frequently. The analysis of variance is included as a special case (single variable).

Adding and listing machines, calculating machines and Hollerith equipment are described from the point of view of their uses in multi-variable analysis. A description of some of their *functions* only is given, without embarking on technical details. The methods of Part I are those believed to be most suited to these machines in the light of several years of computing experience at Scientific Computing Service, Ltd. They are, as it were, a sample drawn from the " bag of tricks " of this organization, and grateful acknowledgment is made to Dr. L. J. Comrie and my former colleague G. B. Hey for having contributed in discussion many of the tricks in the bag. The sample drawn is by no means random, but is biased by my personal choice and preference as to which Jack-in-the-box should come out of the box—whether Hollerith's " Punch," Brunsviga's " Judy " or others! In particular, I must take the blame for having somewhat distorted an unpublished note that Mr. Hey prepared on the application of Hollerith equipment to multi-variable work on which section (iv) (b) is based.

Little reference is made to publications by American authors on Hollerith methods, partly because British tabulators differ considerably from American, and partly because in the United States Hollerith equipment appears to be more readily available at little or no cost for scientific calculations (10), and is therefore often used in circumstances in which its use in this country cannot be afforded. A good survey of American applications is given in (10) and (1).

Because of the widely varying circumstances as to availability of equipment, no *general* comparisons of economy of computing aids are made; such statements as are made are based on the assumption that Hollerith equipment would have to be specially hired for the purpose of doing the particular calculation.

In Part II we give a miscellaneous special selection of important statistical calculations. Reference is made to the functions of calculating machines described in Part I. The mechanical methods described are, to the best of my knowledge, new. Only elementary mathematics is used throughout.

PART I. *Multi-variable work mechanized*

(i) *The problem*

The data consist of a number of observations, each comprising several (say k) variables. For example, the observations may consist of a sample of machine parts made from steel, and the variables observed for each individual part may relate to a series of hardness tests and/or determinations of the chemical composition of the steel from which each part was made; or the observations may consist of a group of school-children, and the variables represent different ability and intelligence tests carried out with each child; or, again, the observations may correspond to a series of years, and the variables may be given by the yield per acre of different crops and/or meteorological data appertaining to each year.

Such data may be set out formally as shown in Schedule (i) below:

Observation number	Variable number 1	2	3	4	. . . . .	k
1	a_{11}	a_{21}	a_{31}	a_{41}		a_{k1}
2	a_{12}	a_{22}	a_{32}	a_{42}		a_{k2}
. . .	.	.	.	.		. .
N	a_{1N}	a_{2N}	a_{3N}	a_{4N}		a_{kN}

Schedule (i)

The computational task is usually to calculate all the possible $\frac{1}{2}k(k-1)$ intercorrelations between every pair of variables. The bulk of this work consists in forming all possible sums of products obtained by pairing two variables. This involves forming line by line the products of these two variables and finally adding the products thus formed for all lines. Mathematically speaking the task is to calculate

$$S_{ij} = a_{i1}a_{j1} + a_{i2}a_{j2} + \ldots + a_{iN}a_{jN}$$

for all possible pairs of variable indices i,j. These quantities should normally include the k sums of squares

$$S_{ii} = (a_{i1})^2 + (a_{i2})^2 + \ldots + (a_{iN})^2$$

This calculation is the fundamental and also the heaviest part of all multi-variable work. Indeed, in certain least squares problems the above raw sums of products S_{ij} represent the symmetrical matrix of the normal equations to be solved, and no modifications are needed. For multiple regression work they would first have to be converted into sums of squares and products of *deviations* from the mean by

$$Ns_{ij} = NS_{ij} - T_iT_j \quad \text{where} \quad T_i = a_{i1} + a_{i2} + \ldots + a_{iN}$$

Finally, for the calculation of intercorrelations r_{ij} we must form

$$r_{ij} = s_{ij}(s_{ii}s_{jj})^{-\frac{1}{2}} = (Ns_{ij})(Ns_{ii}Ns_{jj})^{-\frac{1}{2}}.$$

The work involved in these final stages is comparatively light, and will not be considered further. If the number of variables is large, or even moderate, the work of solving the simultaneous equations with matrix (S_{ij}) or matrix (s_{ij}) is, of course, not negligible, and is discussed in Part II (iv).

(ii) *Digital accuracy*

Without much loss of generality it may be assumed that the values a_{ir} are one-, two- or three-digit quantities. Where the original data are given to more significant figures we may subtract from all observed quantities in the same column (*i.e.*, same variable) a suitably chosen working mean. If the resulting deviations still exceed three figures it will usually be found that the original data were recorded to a spurious accuracy. For the subsequent calculations on machines it is advantageous to choose working means smaller than the lowest value of each variable, so that all deviations are positive; this is better than making them as small as possible. It is then more appropriate to speak of a working origin or, more colloquially, of a dropped constant. With these changes (including changes of scale) it may be assumed without loss of generality that all a_{ij} entering into the calculation of the S_{ij} lie between 0 and 999. The return to the original units is either unnecessary (correlations) or trivial (means, standard deviations or regression coefficients).

An example of the above treatment is given in Tables I and II. Table I gives length and thickness (in inches) for 5 logs, while Table II gives their deviations (in 10 thous.) from a working origin of 45″ and 68″ respectively.

TABLE I

Length, ins.	Thickness, ins.
46·78	68·77
48·73	68·99
45·31	71·34
48·43	72·33
49·77	69·09
239·02	350·52 *

TABLE II

Length (10 thous.)	Thickness (10 thous.)
178	77
373	99
31	334
343	433
477	109
1402	1052 *

With biological data the choice is usually between a convenient working origin with three-figure deviations and a less convenient working origin with two-figure deviations. With industrial data, where the variations are frequently less than 5 per mille, it often suffices to ignore one or two constant leading figures. This has the advantage of rendering the listing of deviations unnecessary.

The data supplied to the statistician will hardly ever be in this ideal form of positive deviations from a working origin. Indeed, in many cases they will not even be in the form of Schedule (i). Where many variables are to be correlated these will often be recorded by different observers;

they may be scattered in various note-books or on various forms, and will have to be assembled. Wherever possible the work of collating the data and of converting them into deviations from working origins should be avoided by providing the observer, at the outset, with a suitable recording form and instructions for its use. There are, naturally, often practical limitations to this procedure. As a rule, therefore, this task becomes part of the computation.

(iii) *The use of an adding and listing machine and of a calculating machine in multi-variable work*

The teaming of the above two machines will be found very helpful—in fact, they represent an attractive combination for general work in a statistical laboratory.

(a) Adding and listing machines and their use in multi-variable work

For a good general description of these machines we may refer to (6). Here we will only re-state some of their features.

There are two types of machines, differing principally in the method of setting numbers. On full-keyboard machines there is a column of keys numbered 1 to 9 representing units, a second column (to the left) representing tens, and so on. Capacities range up to 8 columns (*i.e.*, 9999 9999) or 10 columns (*i.e.*, 99999 99999) or more. Figures are set by depressing the appropriate keys in the required columns. On ten-key machines there are only ten keys corresponding to the digits 0, 1, 2 . . . 9. The keys corresponding to the amount to be entered are struck in succession, as on a typewriter. On either type pressing the motor bar after a setting causes the amount set to be printed on a paper tape and also added to (or subtracted from) the amount already stored in the adding mechanism or register of the machine. When all figures to be added have been thus entered, depressing a TOTAL key prints the total and clears or zeroizes the register. A similar SUB-TOTAL key prints as before, but retains the total in the register. With a full-keyboard machine (see the Victor in Fig. 1) the large capacity can often be utilized for adding paired observations simultaneously. A cipher split (which usually splits a 10-column keyboard in the middle, but can be specified in any position) permits the printing of two separate series of observations side by side (see table of length and thickness in Table II). The vertical spacing of the printed lines is, of course, uniform.

We now turn to the use of such machines in multi-variable work. The several variables are added (in order of observation number) from the various note-books and forms. In this process the operator does not set the original observations, but their deviations (formed mentally), thus obtaining a printed record of the deviations and their totals. In Table I we give original length and thickness records, and in Table II a reproduction of the tape of the adding machine. This work may often be checked by comparing the printed totals with those of the original variables if given in the note-book. If they are not, they must be produced by the operator in a separate run. In the event of disagreement, the tapes are a great help in finding the error.

When all the tapes have been checked they are assembled and pasted up in the form of Schedule (i). This paste-up may be folded in harmonica fashion, so that any two columns may be brought into juxtaposition for the subsequent formation of sums of products. Alternatively, each tape may be left separate, and need only be reinforced so that the tapes for any two variables may be pinned together. If N exceeds 30, the deviations should be split (by horizontal dividing lines) into sections, each with its own total. If k is large, the sections should not exceed 20 in length or even 10 (see S-check in (iii) (b)).

The above procedure is also convenient if the original data are recorded in some non-decimal system unsuitable for the subsequent formation of sums of squares and products (such as lbs. and ozs., stones and lbs., shillings and pence). In Table III is given a sample of height and weight records of schoolboys recorded, in feet and inches and stones and lbs.

TABLE III

	Weight		Height	
	st.	lb.	ft.	ins.
Scholar 1	3	12	4	1·1
„ 2	4	1	4	11·4
„ 3	4	11	4	5·7
„ 4	3	8	3	11·8
„ 5	5	6	4	8·1
	19	38 *	19	38·1 *

Two small auxiliary tables would be prepared giving the conversion of feet and inches straight into inches with a working origin of integer feet omitted. A convenient working origin would be 3 feet, and a section of such a conversion table is given below:

Ft.	Ins.	Set
4	9	21
4	10	22
4	11	23
5	—	24
5	1	25

This would be placed (or pinned) in front of the operator so that the original height entries are quickly converted into the deviation of inches from the working origin set on the machine and added. In forming the check totals on the original data sheets, inches and feet are, of course, added separately, and the conversion is for the feet *total* only. The work, this time, should be carried out in two *separate* adding runs for heights and weights, as simultaneous use of two conversion tables is confusing. The tapes showing deviations from the working means are shown in Table IV.

TABLE IV

	Weight lb.		Height $\frac{1}{10}$ ins.
Scholar 1	12	Scholar 1	131
„ 2	15	„ 2	234
„ 3	25	„ 3	177
„ 4	8	„ 4	118
„ 5	34	„ 5	201
	94 *		861 *

Thus the adding and listing machine performs three tasks in one operation: (1) recording and checking deviations from working origins, (2) forming and checking totals of these deviations; (3) collating of variables in a suitable working schedule.

(b) Calculating machines and their use in multi-variable work

We now turn to the actual formation of the sums of products S_{ij}. A great variety of calculating machines are available for this work. For a brief description of these we may refer to (6), and for further details to trade literature. We will, however, briefly describe here how sums of products may be formed on these machines.

The process of multiplication is really one of continued addition. If we wish to multiply (say) 347 by 4, we could set 347 and add it four times, thus obtaining 1388. To cater for multiplication by numbers of more than one digit the adding mechanism is mounted in a movable carriage so that it can be stepped or moved into the correct position in sympathy with the units, tens, etc., position of the multiplier. Thus to multiply 347 by 42 we first step the carriage to the tens position and add 347 four times, showing the partial product 13880 in the product register; we then step to the units position and add twice to obtain the final answer $347 \times 42 = 14574$.

On most machines the multiplicand (347 in the above example) is set on a full keyboard (*e.g.*, Friden, Madas, Marchant, Mercedes and Monroe); on others it is set by moving levers into the appropriate digital positions (*e.g.*, Britannic and Brunsviga); on others, again, it is set by means of a 10-key keyboard (*e.g.*, Facit).

On hand-operated machines the continued adding is done by turning the handle of a crank, making revolutions equal in number to the digits of the multiplier (four, then two, in the example) and stepping the carriage with the other hand (Brunsviga and Facit). On electric machines the revolutions of the crank (and often the stepping) are actuated by an electric motor. This motor is controlled either by the operator through a motor bar (semi-automatic multiplication), or by relays (fully automatic multiplication). The setting of these relays is achieved by depressing the digits of the multiplier on a full keyboard (Monroe, Madas and Mercedes) or by striking in succession the keys of a ten-key keyboard (Marchant and Muldivo).

After the multiplier has been conveyed to the machine it is shown in the multiplier register (M.R.), whilst the product is shown in the product register (P.R.). The multiplicand is still set on the keyboard (or its equivalent). Some models provide a capacity of 10 figures in multiplicand and multiplier and 20 figures in the product; this is expressed as a capacity of $10 \times 10 \times 20$.

Machines of capacity $8 \times 8 \times 16$ are also popular. Special features of some machines are described in (6), while Figs. 2 and 3 illustrate two.

A sum of products is simply formed by setting in succession the individual multiplicands and conveying the corresponding multipliers. The P.R. will then accumulate progressively the required sum of products.

The M.R. can accumulate the sum of the multipliers. However, it is desirable (and with semi-automatic multiplication necessary) to make the M.R. show individual multipliers in order to verify that the correct multiplier has been conveyed. It must therefore be cleared after each multiplication. We can, however, produce the sum of the multipliers in the P.R. to the left of the sum of products. To this end, on full keyboard machines, we set 1 in the extreme left-hand column of the keyboard.

Below is shown an example of a sum of two products, $516 \times 312 + 728 \times 479$, formed in this way:

Keyboard	M.R.	P.R.
1 0000 516	312	312 0 160 992
1 0000 728	479	791 0 509 704

The sum of products is 509 704 and the sum of the multipliers 791. The latter should be identical with the total of the variables formed in (*a*), and thereby affords a check on multipliers (apart from compensating errors). Care must be taken that the sum of products does not spill over into the sum of the multipliers.

If a similar check is required for the setting of the multiplicands we may (on certain machines) increase the multiplier also by 1 in the same * relative position on the left. The P.R. will then show, suitably separated, the number (N) of products, the combined sum of multiplicands and multipliers (in the middle) and the sum of products (on the right).

The previous example, redone by the present method, becomes:

Keyboard	M.R.	P.R.
1 0000 516	1 0000 312	1 000 0828 0 160 992
1 0000 728	1 0000 479	2 000 2035 0 509 704

It will be seen that with three-figure multiplicands and multipliers a machine of a capacity $8 \times 8 \times 16$ is just adequate for this type of calculation, although the larger capacity of $10 \times 10 \times 20$ is preferable.

Although the above procedure provides a check, it usually slows up the operation, while on 10-key machines like the Facit it is prohibitive because all the interspersed ciphers have to be struck. Modifications are therefore required to achieve full efficiency on each particular machine. With the Facit, for instance (see, *e.g.*, (7)), we might check the sum of multiplicands only or forgo refinements and accumulate the sum of multipliers in the P.R. The first-described method of checking the sum of multipliers *only* is often preferable (*e.g.*, fully automatic Marchant and semi-automatic machines). In all these cases other fool-proof checks must, of course, be applied; indeed, even if the full check on both multipliers and multiplicands is applied, the copying of the sum of products from the P.R. still remains unchecked.

It is therefore essential to superimpose on the above current checks complete overall checks. In the case of multi-variable work the so-called S-check is normally used. With this well-known device the arithmetical sum of all the variables in each line of the schedule is formed and treated as an additional variable.

For convenience in checking we usually set out a triangular schedule as shown:

$j \backslash i$	1	2	3	4	. . .	k	S
1	S_{11}	S_{12}	S_{13}	S_{14}		S_{1k}	S_1
2		S_{22}	S_{23}	S_{24}		S_{2k}	S_2
3			S_{33}	S_{34}		S_{3k}	S_3
. . .	. . .	. . .	. . .	. . .		. . .	. . .
k						S_{kk}	S_k
S							S_s

Schedule (ii)

* This is, of course, not necessary but convenient.

Checks are now made in accordance with the identity

$$S_i = S_{i1} + S_{i2} + \ldots + S_{ik} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

For instance, for $i = 2$ the quantities underlined in full in Schedule (ii) should be equal to S_2.

A great advantage of this check is that it helps to *find* any errors. Suppose, for instance, that the calculated S_{ij} satisfy the above identities for all i except $i = 2$ and $i = 3$, where (1) is invalidated by the same amount. It is then almost certain that S_{23}—*i.e.*, the point of intersection of the full and broken lines in Schedule (ii)—is in error by this same amount; it should therefore be re-done to confirm this hypothesis. If the identity is not satisfied for $i = 3$ only, the sum of squares S_{33} is the culprit. With a large number of discrepancies this process of error-finding may degenerate into a time-consuming cross-word puzzle. This indicates that the S-check is being abused; it is being asked to check too much work! It is therefore advisable, if N is large, to record separate sums of products for each of the sections of about 20 observations for which check totals were already formed in (*a*) and to apply the S-check to each section. The extra work of copying such sectional sums of products will be found to pay dividends at the S-check stage. Also if each section is first completely checked by its own S-check the adding of the sectional S_{ij} is in turn checked by the adding of the S values. Indeed, this is an additional safeguard that all errors found by the sectional S-checks have in fact been corrected.

In certain cases it will be advisable to split the *variables* into sections, each to be checked by its own S-check. This procedure is useful for very large k, or when not all possible intercorrelations are wanted, but only certain rectangular or triangular arrays of the triangular Schedule (ii). The splitting into sections should then, of course, be chosen to coincide with vertical boundaries of these arrays.

If the S-check is carried out in small sections it may be advisable to dispense with some or all of the current checks on the sums of multipliers and multiplicands. More errors (if made) must then be found by the S-check, but the formation of individual sums of products is speeded up. For instance, by sacrificing the check on the sum of multiplicands, two sums of products can often be formed simultaneously on machines with a 10-column keyboard by setting two multiplicands, one on the left-hand three columns, and another on the right-hand three columns.

For positive multipliers and multiplicands the fully automatic Marchant is probably the fastest machine for this operation.

A word should be said about the special case of a correlation between two variables x and y. The five required answers

$$\Sigma x \quad \Sigma y \quad \Sigma x^2 \quad \Sigma xy \quad \Sigma y^2$$

can be produced in one operation by setting x on the left, y on the right and squaring $(10^d x + y)$. The P.R. then shows, in separate sections, Σx^2, $\Sigma 2xy$ and Σy^2, while the M.R., if not cleared after each multiplication, shows Σx on the left and Σy on the right. This operation is particularly convenient on the Monroe AA-1 or the Madas, as on these machines the multiplicand and the multiplier are set and conveyed from the same keys, so that the quantity to be squared is communicated *once* only to the machine. This method is described by Dwyer (9) but has been practised by users of the above machines in this country for a long time.

A neat application of this method is the sum of squares of quantities in a non-decimal system without conversion. Thus for squaring the heights given in Table III directly we would proceed on the Monroe or Madas as follows:

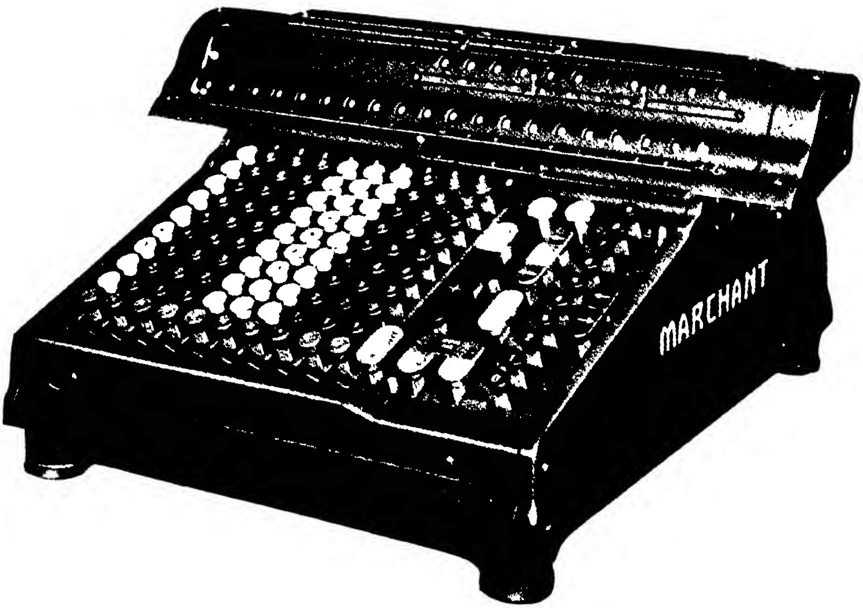
Set on keyboard and square	P.R.
4 000 011	16 000 088 000 121
4 000 114	32 001 000 013 117
4 000 057	48 001 456 016 366
3 000 118	57 002 164 030 290
4 000 081	73 002 812 036 851

At the end of the run we would have in the M.R. 19 000 381, which should agree with the totals in Table III previously obtained, thereby checking the setting. The answer for the sums of squares in ($\frac{1}{16}$ ins.)^2 is then obtained from the quantities in the P.R. thus:

$$\begin{array}{rrcr} & 144\ 00 & \times & 73 \\ + & 12\ 0 & \times & 2\ 812 \\ + & 1 & \times & 36\ 851 \\ \hline & & & 1\ 425\ 491 \end{array}$$

Fig. 1.

[*See p.* 157.



[*See p.* 159.

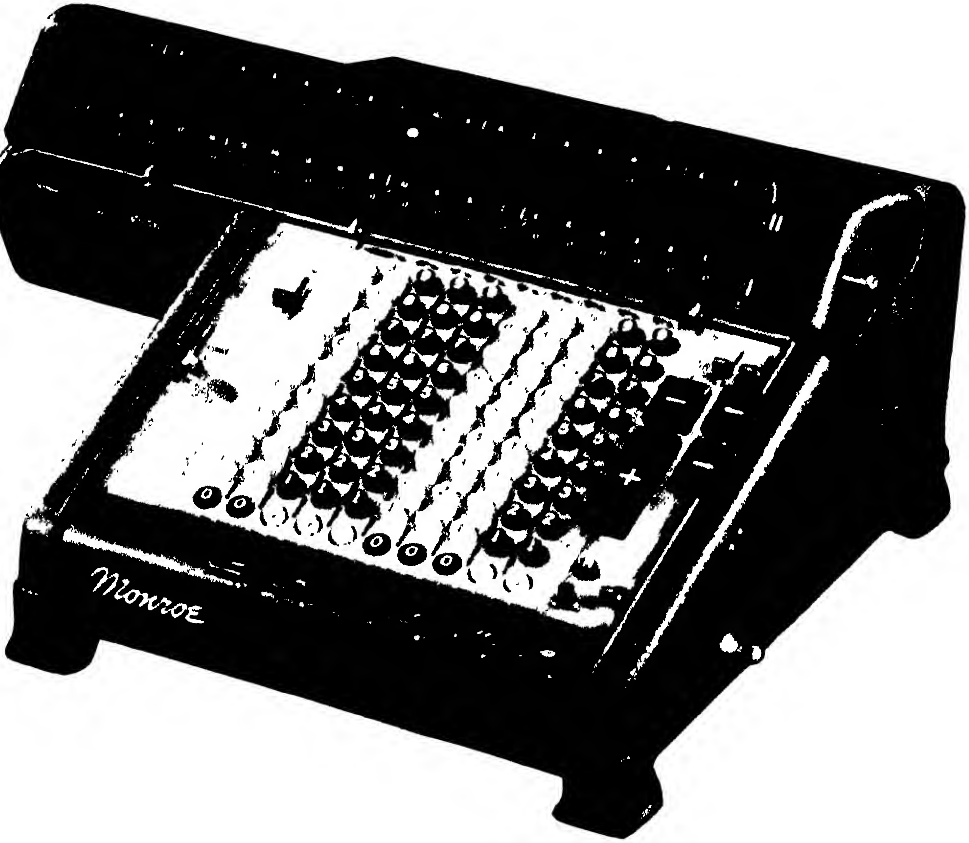


FIG. 3.

[*See p.* 159.

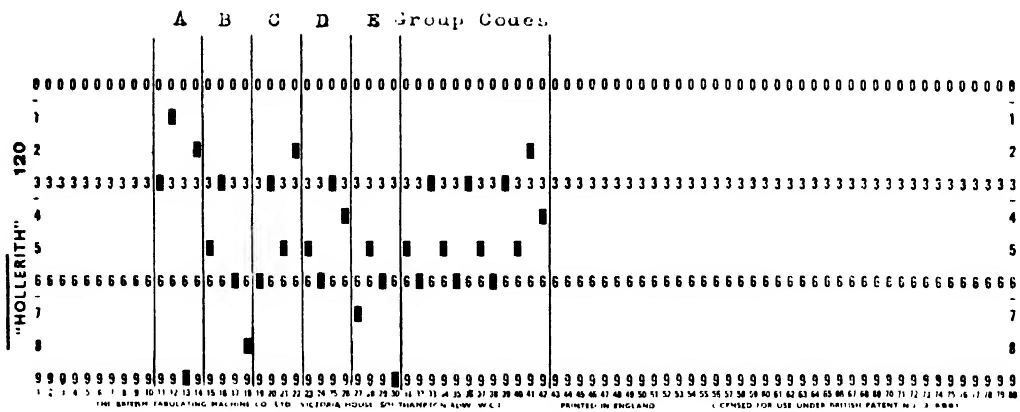


FIG. 4.

[*See p.* 161.

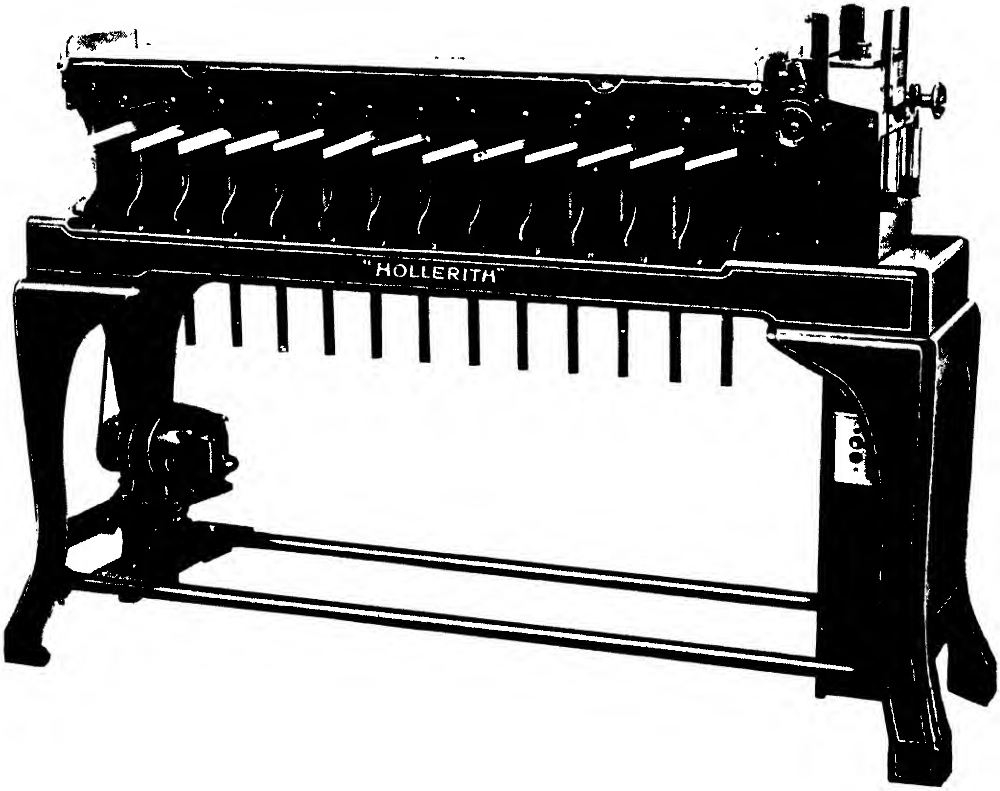


FIG. 5.

[*See p.* 161.

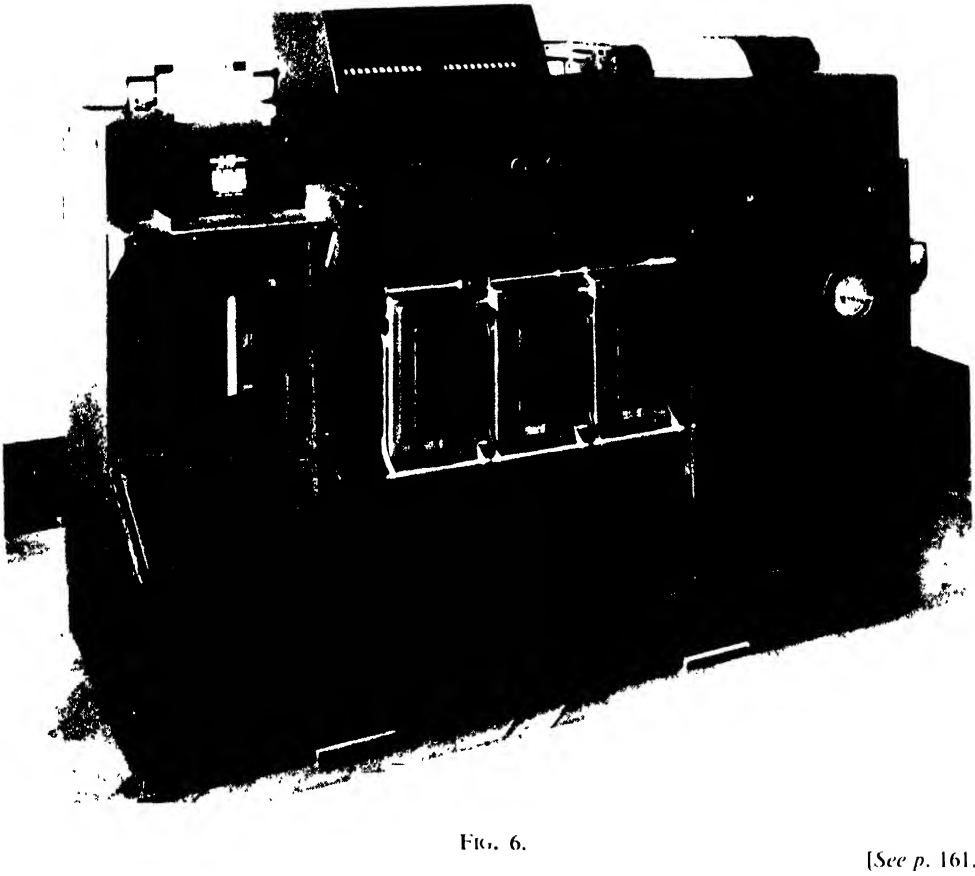


FIG. 6.

[See p. 161.

The number of products to be formed in intercorrelation work is $\frac{1}{2}Nk(k + 1)$, and with the S-check included this figure goes up to $\frac{1}{2}N(k + 1)(k + 2)$. The work is therefore proportional to the sample size (N), and increases as the square of the number of variables. For 20 variables with 300 observations the number of products is already in the neighbourhood of 60,000, while for 50 variables with 1,000 observations it is well over a million. The latter type of requirement is unusual, but the former scale of work is quite common. When work of this magnitude is contemplated it becomes necessary to use machines more powerful than ordinary desk calculators; such help is admirably rendered by the punched-card system.

(iv) *The use of Hollerith punched-card equipment for multi-variable work*

Before describing the method on Hollerith equipment we should recall that there is another punched-card system—the Powers-Samas. Whilst for commercial and routine statistical work (for which these systems were really designed) both have their advantages and disadvantages, the flexibility offered by the Hollerith plugboard, as opposed to the Powers-Samas connection box, is a great advantage for scientific work, where the nature and details of the calculations alter frequently. For a detailed description of both systems we may refer to the excellent account given in (3).

(a) A description of a Hollerith equipment and some of its functions

Since the appropriate Hollerith equipment is well described in (3) and a brief account is also given in a paper submitted to this Society (5), we may confine ourselves here to a brief outline of some of its functions (but not of the technical detail).

Cards. Data can be recorded on cards by punching holes in them. These holes are sensed electrically by the various machines through which the cards are passed. The card (see Fig. 4) is $7\frac{3}{8}$ by $3\frac{1}{4}$ inches and contains 80 vertical columns, each capable of recording one of the digits 0, 1, 2 . . . 9; there are also two extra positions at the head of each column, called X and Y, which have special uses (see, *e.g.*, (b)). A group of several columns is called a field. Thus in a four-column field we may record any four-figure number such as 3192 by punching in its four columns from left to right 3, 1, 9 and 2 respectively (see field A in Fig. 4). Cards for special purposes can be printed with the subdivision of the columns into fields clearly indicated.

Mechanical Key Punch. Cards are punched in the mechanical key punch column by column. A somewhat similar machine is used to verify the punching. The work is done by junior operators, who with some experience will average 150 cards per hour. This is a very good speed if all 80 columns are punched, but is of course exceeded if fewer columns are punched.

The Sorter (see Fig. 5). The function of the sorter is to separate the cards into groups according to the holes punched in any single selected column. The cards are deposited in one or other of 12 boxes, according to whether Y, X, 0, 1 . . . 9 is punched in that column; cards with no holes punched in the sort-column fall into a 13th box. The machine may obviously be used to sort cards into numerical order according to the amounts punched in any field. To do this, sorting is done first on the units, then on the tens, and so on. The theoretical speed of this machine is 24,000 cards an hour.

Rolling Total Tabulator (Fig. 6). This machine senses electrically the holes punched in each column of the card by means of two sets of 80 brushes. It contains six adding registers or counters of 11-figure capacity each; numbers can be added from any column of the card into any wheel of any counter or into several wheels simultaneously. These numbers can, if desired, be printed in any position of a print bank of 80 type-bars. Instructions to the machine are conveyed through a plugboard.

The normal operation is to add the numbers punched in certain fields (such as A to E in Fig. 4) of a group of cards into certain counters to form the field totals. The tabulator can be plugged to sense the end of such a group of cards automatically (technically known as breaking of control). The machine then normally prints the totals in all counters and clears the counters before beginning to add the fields from the next group of cards. Alternatively, it can be arranged that at the end of each group certain counters add (or subtract) their totals into any combination of the other counters in a prearranged sequence of operations. As many as eight such operations, or cycles as they are styled, can be performed at the end of each group.

Simpler tabulators, lacking these transfer facilities, are available at lower rentals.

The fastest models take 0·4 second per card passage, but if the contents of each card is to be printed the time is 0·6 second per card.

The Reproducing Punch. We shall describe this machine in a little more detail and include some non-standard uses.

This machine reproduces the punching in any selected columns of one series of cards in any selected columns of another series of cards—an operation known as reproducing.

The other fundamental operation is to copy-punch information from one particular card (master-card) on to all cards in a group (detail cards); the master-card is placed in front of the detail cards, and the group passed through the machine in what is called a plain gang-punch operation. This can be done with several master-cards, each followed by its associated pack of detail cards; all the cards are passed through without interruption at the end of the groups. Actually in this operation there are at any time two consecutive cards in the machine, which is sensing the holes in the first card; the currents that pass through these holes are plugged to operate knives that punch the same digits in the corresponding positions of the second or following card. At the next cycle the first card is passed out to a stacker, while the second card (having just received its holes from the first) is passed under the sensing brushes and read in order to punch the same information on the third card. When the next master-card passes under the knives a special impulse automatically suppresses the punching for one cycle; to this end master-cards, but not the detail cards, must have an X punched in a certain column, or vice versa.

An important (non-standard) variation of gang-punching is offset gang-punching—a technique that we shall apply later on to serial correlations. Imagine that we plug from the sensing brushes of column 1 to the magnet operating the knives of column 2, from the sensing of column 2 to the punching of column 3, and so on. This plugging may be schematically represented as shown in Schedule (iii).

Sensing columns 1 2 3 4 . . . . 79 80

Punching columns 1 2 3 4 . . . . 79 80

Schedule (iii)

Imagine also that a hole (say a Y) is punched in column 1 of the first card and that all the remaining cards have no holes punched. The effect of the consecutive sensing and punching will then be that the second card will be punched Y in the second column, the third card in the third column, the eightieth card in the eightieth column, and all subsequent cards (if any) will have no holes punched.

Suppose now that *all* the columns in the first card are punched, and denote by (i) the digit punched in the ith column of the first card. Then the effect of offset gang-punching will be as shown in Schedule (iv).

Card No. \ Column	1	2	3	4	5	...	79	80
1	(1)	(2)	(3)	(4)	(5)	. . . .	(79)	(80)
2		(1)	(2)	(3)	(4)	. . . .	(78)	(79)
3			(1)	(2)	(3)	. . . .	(77)	(78)
79							(1)	(2)
80								(1)

Schedule (iv)

If the (i) are single-digit values of a time series, we see that after the cards have been offset gang-punched columns 79 and 80 contain pairs of consecutive observations viz. (79) and (80), (78) and (79), . . . (1) and (2). Columns 78 and 80 contain pairs of observations two time intervals apart, columns 77 and 80 contain observations three time intervals apart, and so on.

A useful modification is to split the offset gang-punching into a series of independent fields. Suppose the (i) represent eight separate 10-decimal quantities punched in columns 1–10, 11–20, . . . 71–80 respectively, and that we omit nine plugs as shown in Schedule (v).

Sensing columns 1 2 3 . . . 9 10 11 12 13 . . . 19 20 : : : 79 80

Punching columns 1 2 3 . . . 9 10 11 12 13 . . . 19 20 : : : 79 80

Schedule (v)

The resultant punching is shown in Schedule (vi).

Column / Card No.	1	2	3	4	...	9	10	11	12	13	14	...	19	20	...	79	80
1	1	2	3	4	...	9	10	11	12	13	14	...	19	20	...	79	80
2		1	2	3	...	8	9		11	12	13	...	18	19	...	78	79
3			1	2	...	7	8			11	12	...	17	18	...	77	78
9						1	2						11	12		71	72
10							1							11			71

Schedule (vi)

We have thus copy-punched fields and at the same time divided the original quantities progressively by powers of 10, but without any rounding off. This technique is of great help when making what may be called ready-reckoner cards, and will also be applied later. A mathematician would undoubtedly call both the above applications trivial, but nevertheless they are useful and efficient. The reason for this is that the machine works on practically all columns of the cards at the same time.

Still another use of the reproducer punch is that it can be connected to a tabulator, and at every break of control punch on to new cards the totals accumulated in any or all counters. When used in this way it is known as a summary punch, and the cards it punches are described as summary cards. The reproducer handles each card in 0·6 second, although when summary punching it adds 2·2 seconds per summary card to the normal time of the tabulator.

The Multiplying Punch. This member of the Hollerith family will sense up to four fields (say A, B, C, D) not exceeding eight digits on a card, form certain combinations like $AB + C \pm D$, and punch the result on unused columns of the same card. Although it can form products at from 700 to 1,200 an hour, according to the number of digits in the multiplier, its somewhat high rental has not encouraged its use in scientific and statistical work, except in certain particularly favourable circumstances. For this reason no applications are cited in this paper.

The Collator. This youngest child of the family feeds two separate stacks of cards through two field units, and has four boxes or receptacles in which cards may be deposited. Instructions are, as usual, conveyed by a plugboard. Its simplest application is to selecting cards having a certain number, or those above or below that number, from a stack of cards. The discriminating number would be on a single card that would remain at its sensing station throughout, and would direct the cards of the main stack into which box they should go. Similarly, two packs of cards, each in order, could be fed through the two feeding units, and would emerge in order as a single pack. As a variant, cards in pack A that found no mates in pack B could be directed to one box, and conversely, and all mated cards to a third box. In short, all kinds of merging and separating tricks can be done with this machine. Its comparative newness and lack of accessibility have not enabled it to be used much so far outside large commercial installations, but it has interesting scientific possibilities.

(b) The calculation of sums of products on Hollerith equipment

In spite of the existence of the multiplying punch, it is usually more economical to use the tabulator to form *sums* of products for $N > 30$.

Generally speaking, the most efficient method is that known here as summary multiplication, or in America as progressive digiting. It has been widely used, both here and in the U.S.A. Good descriptions are given in (5) and (2). Nevertheless, for completeness sake, let us recall this method here briefly.

Suppose we have N cards with a single-digit variable a in column 1 and a series of corresponding variables b (not restricted in size) in other columns of the same cards. The problem is to form Σab. If we sort these cards on column 1 into 10 packets corresponding to $a = 9, 8, \ldots, 1, 0$, we may then add the field b in a tabulator and print totals B of groups as follows:

$$B_9 = \Sigma b \text{ for } a = 9$$
$$B_8 = \Sigma b \text{ for } a = 8$$

$$B_1 = \Sigma b \text{ for } a = 1$$
$$B_0 = \Sigma b \text{ for } a = 0$$

Obviously

$$\begin{aligned}\Sigma ab &= 9\,B_9 + 8\,B_8 + \ldots + B_1 \\ &= B_9 + (B_9 + B_8) + \ldots + (B_9 + B_8 + \ldots + B_1)\end{aligned}$$

The second form shows that if we feed the packs in descending order of a and omit the pack in which $a = 0$, and do not clear the counter after each pack, we shall print the progressive totals $B_9, B_9 + B_8 \ldots$, which need only to be added to give Σab. If we summary punch the progressive totals, a run of the summary cards through the tabulator will form the desired sum of products.

Now, this looks like using a steam-hammer to crack a nut! It would indeed be wasteful were it not for the fact that the large capacity of the tabulator permits further variables, c, d, e, etc., punched in other fields of the same pack, to be added at the same time as the b's. Their progressive totals are included in the same summary cards, and in the same subsequent tabulation run, without adding a second to the machine time. The golden rule for getting a high efficiency factor in summary multiplication is to make the fullest possible use of the counter capacity of the tabulator.

Let us now discuss briefly further details of the method:—

(A) If the variable a is (say) a two-digit number, we repeat the above process, sorting on the tens column of a and adding the variables b, c, d, . . . as before. We may summary punch these progressive totals one column to the left, so that they may be added with those of the first run to form the complete sum of products.

(B) Sums of squares are, of course, merely the special case when the variable on which the cards are sorted is also among the variables to be added.

(C) For moderate sample sizes ($N \sim 30$) some of the digits 1, 2 . . . 9 may not occur in a. Thus if there were no card with $a = 7$, the sum of the progressive totals

$$B_9,\ B_9 + B_8,\ B_9 + B_8 + B_6,\ \ldots,\ B_9 + B_8 + B_6 + \ldots + B_1$$

would not be equal Σab, but would be in defect by $B_9 + B_8$.

The required correction becomes more complicated when there are several gaps, or even runs of gaps. Although there are various ways of allowing for these gaps, it is better to avoid them by punching several red space cards with Y in odd columns and X in even columns, and several blue cards with X in odd columns and Y in even columns. After sorting the cards on a digit of a, put space cards in all empty boxes, making sure that space cards in adjacent boxes are of different colours. These dummy multipliers (X or Y) will break control, and thus cause the desired missing progressive totals to be punched. On the other hand, the holes X and Y in the multiplicand fields do not affect the addition.

The former method of using blank space cards (5), (2) is no longer used, as with the type of control fitted to-day two consecutive blank cards would break control once only. It is obvious that no gaps above the highest digit occurring need be filled. During the next sort all space cards will naturally fall into the X and Y boxes, whence they will be returned to their red and blue packs.

(D) A single 11-wheel counter will hold the progressive totals of two 3-figure variables, provided that the sum of the variables does not exceed 10^5. With 2-figure variables it will rarely be possible to add three variables in a counter, particularly as the sum of the variables will usually have one or two digits more than the variables themselves. In statistical work, therefore, any variation from two variables to a counter will be rare. Hence with a six-counter tabulator maximum efficiency is secured if we have 11 variables and an S.

On a tabulator specially designed by G. B. Hey, and now at the National Physical Laboratory, the left-hand wheel of any counter can be joined to the right-hand wheel of any other; this opens up possibilities for squeezing still more on to the machine.

With (say) 11 variables and their S, which we treat as a variable, we must sort on each variable in turn in order to obtain all sums of squares. We cannot, therefore, do better than add all the 12 variables every time. We thus form Σab by adding the b's after sorting on a, and Σba by adding the a's after sorting on b, and so on. The addition of the summary cards would then form and print the 12 lines of Schedule (ii), completed symmetrically as a full-square pattern, the 60 sums of products would each be produced twice and the 12 sums of squares once. This duplication takes no longer, and has the advantage of providing a good check on the correct working of most of the machine operations. If there are more than 11 variables, it is of course often possible to avoid producing every sum of products twice over; ingenious schemes have been worked out by G. B. Hey in which a good compromise is reached between convenience of plugging the fields to be added and the arrangement in which the answers are printed.

(E) What happens if the number of variables is less than 11? Are we doomed to inefficiency? In the first place, Hollerith tabulators having four counters only are available at a lower rental. Sometimes, however, it may be useful not to punch summary cards, but to use some of the six counters to accumulate the progressive totals as they are being formed. Thus four progressive totals formed in counters 1 and 2 could be added in counters 3 to 6.

The ability to transfer totals from one counter to another (*i.e.*, rolling in Hollerith terminology) is required for this method. Although the rentals of rollers are somewhat higher than those of simpler machines, the above method is very useful and efficient with a few variables, or when no summary punch is available. In certain circumstances it is worth while using this method for $k \geqslant 11$.

The important special cases of one or two variables (plain sum of squares, analysis of variance and covariance) are not usually efficient by this method, since it is, as a rule, not worth while punching cards for the mere purpose of producing one sum of squares or two sums of squares and one sum of products. On the other hand, if the data are already on cards for other purposes (*e.g.*, for multiple classification by qualitative variables), methods like the above may prove economical. However, other methods of summary multiplication demand critical comparison with progressive digiting and, because of its importance, the special case of sums of squares is dealt with by another method in the next section.

(v) *The master-card method of producing sums of squares*

Suppose that N observations of a single 2-figure variable are already punched (say in columns 1 and 2), that four columns are still available (say columns 3 to 6) and that N is of the order 2,000. Suppose also that it is desired to carry out a number of analyses of variances with these data so that sums of squares are wanted for various sub-groups, each of which would, of course, have a designating code punched in certain columns for sorting purposes.

We now punch 99 master-cards for the range $a = 1, \ldots 99$, punching a in columns 1 and 2 and values of a^2 in columns 3 to 6. To distinguish these master-cards from ordinary cards, an X must also be punched in some convenient column. The master-cards, followed by the N data cards, are now sorted into serial order of a. We then gang-punch the squares from the masters on to the data cards. Each card will then contain an observation and its square. The cards are then sorted into the experimental sub-groups and group totals of a and a^2 formed by the tabulator. This process does not have a high efficiency factor, as only two fields are being added, but if several variables $a, b, c \ldots$ are all punched on the same card, and if an analysis of variance is required for each, the fact that only one tabular run is required for all the adding leads to an effective process.

Generalizations of this procedure are obvious. The essential condition is that the range of the observations in units of their last figure should not exceed about $N/20$. If A is a convenient working mean near the middle of the range, the master-cards could contain a and $(a - A)^2$. Hence it is not necessary that the observations should be 2-figure numbers. If the range of observations is greater than $N/20$, the observations may be grouped and Sheppard's correction applied.

PART II. *Miscellaneous applications of calculating machines*

In this section we give a number of selected applications of calculating machines to statistical problems. The first application (i) is straightforward and, although useful, may not be new. The other applications, (ii), (iii) and (iv), are, to the best of my knowledge, new.

(i) *The use of adding and listing machines for the calculation of moving averages*

In Table V is given a time series of 24 values a_r for $r = 1, 2, \ldots 24$; it is required to calculate the 19 moving averages of 6—*i.e.*,

$$m_i = \tfrac{1}{6} \sum_{r=}^{i+5} a_r \quad i = 1, 2, \ldots 19$$

For most purposes it will be sufficient to calculate moving totals, $M_i = 6\, m_i$, as the division by 6 is usually unnecessary, particularly when answers are plotted.

The method of calculation is to keep a moving total of six values constantly in the adding machine, by adding and subtracting a value of the series at each stage. This is illustrated in the tape reproduction in Table VI, to which the following paragraph refers.

TABLE V

Time Series

·361
·359
·351
·360
·349
·356
·312
·278
·210
·208
·223
·198
·216
·151
·139
·162
·210
·236
·287
·223
·235
·312
·378
·415
6·529 *T*

TABLE VI

Calculation of moving totals for time series in Table V

·361	·198 –
·359	·236
·351	1·114 *S*
·360	·216 –
·349	·287
·356	1·185 *S*
2·136 *S*	·151 –
·361 –	·223
·312	1·257 *S*
2·087 *S*	·139 –
·359 –	·235
·278	1·353 *S*
2·006 *S*	·162 –
·351 –	·312
·210	1·503 *S*
1·865 *S*	·210 –
·360 –	·378
·208	1·671 *S*
1·713 *S*	·236 –
·349	·415
·223	1·850 *S*
1·587 *S*	·287 –
·356 –	·361
·198	1·924 *S*
1·429 *S*	·223 –
·312 –	·359
·216	2·060 *S*
1·333 *S*	·235 –
·278 –	·351
·151	2·176 *S*
1·206 *S*	·312 –
·210 –	·360
·139	2·224 *S*
1·135 *S*	·378 –
·208 –	·349
·162	2·195 *S*
1·089 *S*	·415 –
·223 –	·356
·210	2·136 *S*
1·076 *S*	2·136 *T*

Set, print and add the first six values of a. Sub-total to print $M_1 = 2{\cdot}136$. Set a_1 and subtract, set a_7 and add, sub-total to print $M_2 = 2{\cdot}087$, and so on until $M_{19} = 1{\cdot}850$ is formed. We now continue cyclically by introducing six dummy values $a_{25} = a_1 = {\cdot}361, \ldots, a_{29} = a_5 = {\cdot}349$, $a_{30} = a_6 = {\cdot}356$. This gives $M_{20} = 1{\cdot}924$, and so on until the last moving total $M_{25} = 2{\cdot}136$ is produced, when the machine is totalled. To facilitate the picking up of the values a_1, a_7; a_2, a_8—*i.e.*, pairs of values six time intervals apart—it usually pays to cut a little stencil with two apertures exactly six lines apart.

The work is completely * checked by the identity $M_{25} = M_1$. The last six dummy totals are

* Except, of course, for exactly compensating errors.

struck out on the tape, leaving a checked printed list of the remaining 19. Any errors of setting are quickly found by reference to the tapes. If conversion into moving averages is desired, it is advisable to retain the six dummies in order to apply an S-check after dividing by 6.

If moving averages are required for two parallel series, they can be dealt with simultaneously by adding and listing them side by side. (See Part I (iii).)

(ii) *The calculation of serial correlations on Hollerith*

Let us assume that there is a time series of 108 observations,* (1), (2), (3), . . . (108), all consisting of positive 2-figure * numbers. It is required to form all serial correlation coefficients up to order 35*—*i.e.*, we are to correlate observations that are 1, 2, 3, . . . 35 time intervals apart.

The time series is now split into three groups of 36 consecutive observations and each group is punched on to 36 consecutive 2-figure fields of a card, called Fields I, II, . . . XXXVI. The remaining 8 columns are used for indicative matter. Each of the 3 cards thus punched is now placed as a master [see Part I (iv)] in front of a pack of 35 blank detail cards, and the total pack of 108 cards thus formed is offset gang-punched in the manner shown by the ordinary figures in Schedule (vii). Copy-punching is in the direction of the downward arrows

Field		I	II	III	. . .	XXXV	XXXVI
1st Master ...	... ...	(1)	(2)	(3)	. . .	(35)	(36)
First pack of detail cards	1 ...	(2)	(3)	(4)	. . .	(36)	*(37)*
	2 ...	(3)	(4)	(5)	. . .	*(37)*	*(38)*
	3 ...	(4)	(5)	(6)	. . .	*(38)*	*(39)*
	. . .	. . .	. . .	. . .	. . .	. . .	. . .
	33 ...	(34)	(35)	(36)	. . .	*(68)*	*(69)*
	34 ...	(35)	(36)	*(37)*	. . .	*(69)*	*(70)*
	35 ...	(36)	*(37)*	*(38)*	. . .	*(70)*	*(71)*
2nd Master ...	... ...	(37)	(38)	(39)	. . .	(71)	(72)
Second pack of detail cards	1 ...	(38)	(39)	(40)	. . .	(72)	*(73)*
	2 ...	(39)	(40)	(41)	. . .	*(73)*	*(74)*
	3 ...	(40)	(41)	(42)	. . .	*(74)*	*(75)*
	. . .	. . .	. . .	. . .	. . .	. . .	. . .
	33 ...	(70)	(71)	(72)	. . .	*(104)*	*(105)*
	34 ...	(71)	(72)	*(73)*	. . .	*(105)*	*(106)*
	35 ...	(72)	*(73)*	*(74)*	. . .	*(106)*	*(107)*
3rd Master ...	... ...	(73)	(74)	(75)	. . .	(107)	(108)
Third pack of detail cards	1 ...	(74)	(75)	(76)	. . .	(108)	
	2 ...	(75)	(76)	(77)	. . .		
	3 ...	(76)	(77)	(78)	. . .		
		. . .	. . .	. . .			
	33 ...	(106)	(107)	(108)			
	34 ...	(107)	(108)				
	35 ...	(108)					

Schedule (vii)

The pack is then turned round bodily and passed through the reproducer a second time in the reverse order to complete the blank fields. The third pack of detail cards would pass without a master and, if desired, can be needled off and withdrawn from the run.

The third master will precede the second pack of 35 detail cards and the second master the first pack. The effect of this run is shown by the italic figures; copy-punching proceeds in the direction of the upward arrows.

Fields I and II will now contain observations one time interval apart; Fields I and III will contain all observations two time intervals apart, and so on. The remainder of the work consists in producing the sum of products of Field I into itself and into all other 35 fields by one of the methods described in Part I (iv).

A machine (8) built specially for this type of work by assembling Post Office relays is said to have a theoretical speed of 1 second per product.

Under conditions like those here postulated, the Hollerith method is very much faster. Also

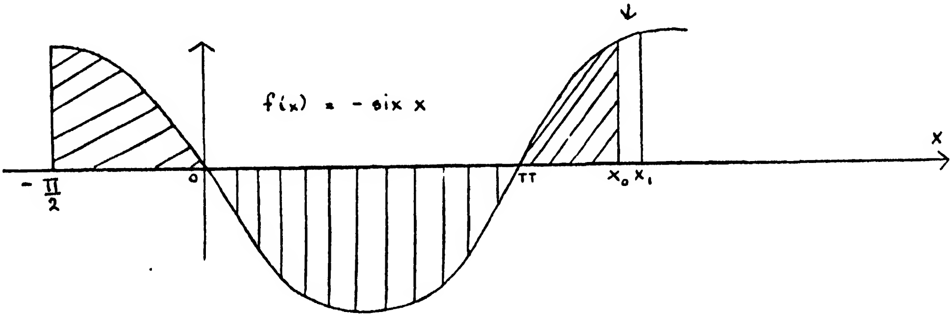
* These are the most favourable conditions for the Hollerith method about to be described, but the necessary modifications for different requirements are obvious from the description of this special case.

the great merit is that these machines are available in larger numbers to statisticians, whereas access to the special machine mentioned is confined to a small group of workers. Moreover it can, by a mere change of plugging, be used for a variety of other calculations.

(iii) *Numerical quadrature on the National machine*

(a) The problem

By numerical quadrature we mean the tabulation of the indefinite integral $\int_a^{x_0} f(x)dx$ of a given integrand $f(x)$ as a function of the upper limit x_0; geometrically the problem is to tabulate the area between a given curve $f(x)$ and the x-axis. The example chosen is one for which the answer is known. In the graph below of $-\sin x$ the area between this curve, the x-axis and the verticals at $x = -\frac{\pi}{2}$ and $x = x_0$ is equal to $\cos x_0$. The calculation is, therefore, one of a more fundamental mathematical nature, and not one that is likely to occur frequently in the work of the practising statistician. On the other hand, it is the fundamental operation in the tabulation of distribution functions and their integrals in every statistical table.



The most important example is that of producing a table of the probability integral from a given table of a distribution function. Moreover, most of the distribution functions in statistics are, by virtue of their definition, given in terms of integrations over a specified sample space. As is well known to mathematical statisticians, the occasions when these integrals can be found by analytical transformation and integration are the exception rather than the rule; more often than not an expression for the probability integral or distribution function of a statistic involves quadrature. The method to be described has already been applied to two large-scale tabulations of probability integrals (one of which has been published recently)—namely:

(A) The distribution of the mean deviation in samples from a normal population (11).

(B) The distribution of the average of eccentricity-readings (distribution of mean χ for two degrees of freedom).

In the case of (A) the extent of the tabulation undertaken would have been hardly practical without this method.

The method is particularly suited to the evaluation of functions defined by quadrature-recurrence, as it will produce in turn a printed table of each function required. See, for instance (11) and (12). It is also suitable for the numerical evaluation of integrals of two or more variables, which will be briefly referred to below.

(b) The method

The National accounting machine, which is used for this method, has been described in detail in this *Journal* (4), so no further description is given here.

With numerical quadrature the integrand $f(x)$ must be given numerically, usually in form of a table at equidistant tabular intervals w. More often than not this table must first be calculated from a formula. In the present method we calculate a table of $g(x) = \frac{1}{24}wf(x)$ rather than of $f(x)$. With every method of numerical quadrature the integrand should be checked first; the usual way

is by differencing. By a special arrangement on the National (shown below), we produce all the differences of $g(x)$ up to the fifth and also the double fourth difference $\Delta_0^{iv} + \Delta_1^{iv}$, which is required later.

Differencing of $-\frac{\cdot 1}{24}\sin x$ in units of 8th decimal *

x	$-\frac{\cdot 1}{24}\sin x$	Δ^v	$-(\Delta_0^{iv} + \Delta_1^{iv})$	Δ^{iv}	Δ'''	Δ''	Δ'	$-\frac{\cdot 1}{24}\sin x$
−·2	82779	—	—	—	—	—	—	82779
−·1	41597	—	—	—	—	—	9958818	41597
·0	0	—	—	—	—	9999585	9958403	0
·1	41597	—	—	—	415	0	9958403	9958403
·2	82779	—	—	0	415	415	9958818	9917221
·3	123133	9999998	2	9999998	413	828	9959646	9876867
·4	162258	9999990	14	9999988	401	1229	9960875	9837742
·5	199761	4	20	9999992	393	1622	9962497	9800239

* Two prints of Δ^{iv} to the right of Δ^v are not given in this table, as these prints can in fact be suppressed when using closely spaced non-print stops.

The stops and schedule of operations for this process are shown below. (S.T. = Sub-total, T. = Total):—

Stop ...	0	+1	3	−1	−1	0	−1 + 4	−1 + 2
Oper. ...	Set x	Set $g_2(x)$	S.T. 1	S.T. 3	S.T. 3	T. 1	S.T. 3	S.T. 4
Print ...	x	$g_2(x)$	$\Delta_{-\frac{1}{2}}^{v}$	Δ_0^{iv}	Δ_0^{iv}	$-(\Delta_{-1}^{iv} + \Delta_0^{iv})$	Δ_0^{iv}	$\Delta_{\frac{1}{2}}'''$
Stop ...	−1 + 6	−1 + 5	−1					
Oper. ...	S.T. 2	S.T. 6	S.T. 5					
Print ...	Δ_1''	$\Delta_{1\frac{1}{2}}'$	$g_2(x)$					

The advantage of having the above finite difference table of $g(x)$ rather than of $f(x)$ is that fractional coefficients in the quadrature formula can be avoided. We use the formula for the increment in the integral I when increasing the upper limit from x_0 to x_1 (this increment is marked by an arrow in the figure).

This is given in the standard text-books as

$$\Delta I = w\left\{\tfrac{1}{2}(f_0 + f_1) - \frac{1}{24}(\Delta_0'' + \Delta_1'') + \frac{11}{1{,}440}(\Delta_0^{iv} + \Delta_1^{iv}) - \ . \ . \ .\right\}$$

in which $f_0 = f_0(x)$, and the first term neglected is $\frac{191}{120{,}960}(\Delta_0^{vi} + \Delta_1^{vi})$ or approximately $\frac{1}{633}(\Delta_0^{vi} + \Delta_1^{vi})$. If we deliberately increase the coefficient $\frac{11}{1{,}440}$ to $\frac{12}{1{,}440}$, and thereby neglect $\frac{1}{1{,}440}(\Delta_0^{iv} + \Delta_1^{iv})$, and remember that $f = \frac{24}{w}g$, we find that

$$\Delta I = 12(g_0 + g_1) - (\Delta_0'' + \Delta_1'') + 0{\cdot}2(\Delta_0^{iv} + \Delta_1^{iv})$$

where the Δ's are now those of the g's. If we regard the integral as being made up of three components, A, B and C, arising from the three terms in ΔI, we note that the fifth difference of A (call it δ^v) is $12(\Delta_0^{iv} + \Delta_1^{iv})$, the third difference of B (call it δ''') is $-(\Delta_0^{iv} + \Delta_1^{iv})$, while the first difference of C (call it δ') is $0{\cdot}2(\Delta_0^{iv} + \Delta_1^{iv})$. These three differences are all simple multiples of the *same* double difference $\Delta_0^{iv} + \Delta_1^{iv}$.

By a special arrangement of stops (shown below) the sum $I = A + B + C$ can be built up in one finite-difference integration by feeding the input differences δ^v, δ''' and δ' into the National on the three first stops of each cycle of operations. $\delta''' = -(\Delta_0^{iv} + \Delta_1^{iv})$ is simply read from the difference table of $g(x)$, and its multiples (12 times for δ^v and 0·2 times for δ') are read off from an auxiliary table of multiples of 12 and 0·2 with argument $(\Delta_0^{iv} + \Delta_1^{iv})$ ranging from − 100 to + 100.

Usually, as in the example, the term C is negligible, and δ' need not be fed into the machine. The quadrature shown below yields cos x to 5-decimal accuracy. The last three decimals are not reliable (see rounding-off errors mentioned below), and a 5 in the sixth decimal has been added for automatic rounding off; in the specimen the sixth, seventh and eighth decimals of the function are not printed.

Quadrature of $-\sin x$

$\delta'''_{\frac{1}{2}}$	$\delta_{\frac{1}{2}}^{v}$	$12(\Delta_0'' + \Delta_1'')$	$12(g_0 + g_1) -(\Delta_0'' + \Delta_1'')$	$12(\Delta_{\frac{1}{2}}''' + \Delta_{1\frac{1}{2}}''')$	$12(\Delta_{\frac{1}{2}}' + \Delta_{1\frac{1}{2}}') -(\Delta_{\frac{1}{2}}''' + \Delta_{1\frac{1}{2}}''')$	$\cos x$	x
0	—	—	—	—	—	1·00000	0
2	99999976	4980	99500421	9936	99005824	0·99500	0·1
14	99999832	14916	98506245	9768	99020754	0·98007	0·2
20	99999760	24684	97526999	9528	99045458	0·95534	0·3
27	99999676	34212	96572457	9204	99079697	0·92106	0·4
38	99999544	43416	95652154	8748	99123151	0·87758	0·5
43	99999484	52164	94775305	8232	99175358	0·82534	0·6
51	99999388	60396	93950663	7620	99235805	0·76484	0·7
56	99999328	68016	93186468	6948	99303877	0·69671	0·8
61	99999268	74964	92490345	6216	99378902	0·62161	0·9

The stops and schedules of operation for this process are shown below:

Stop ...	...	2	4	2	6	1	3	0	0
Oper. ...	...	Set $\delta'''_{\frac{1}{2}}$	Set $\delta_{\frac{1}{2}}^{v}$	S.T. 1	S.T. 3	S.T. 4	S.T. 2	S.T. 6	Set Arg

It should be noted that none of the high-order differences is rounded off, and accumulation of rounding-off errors arises only in the summation of the *integrand* values. This is inherent in every process of numerical quadrature. Its maximum effect on the last figure of the summation is $\frac{1}{2}n$, where n is the number of steps in the integration.

At the end of the quadrature tabulation the contents of the register containing $12(g_0 + g_1) - (\Delta_0'' + \Delta_1'')$ should agree exactly with the corresponding (check) value worked out from the difference table of g. This checks the input of δ''' and δ^{v}, and the operation. A similar check is applied to the input of δ' when this is required. If the tabulation is lengthy, the check should be applied at intermediate arguments.

When applying this method to functions of two or more variables we integrate for each variable in turn. For instance, to evaluate $\iint f(x,y)dx\,dy$ we would first produce, for each y-value in the x, y-grid, a table of the integral $\int_a^{x_0} f(x,y)dx$. For each x_0-value in the grid these integrals would then be integrated over y. The original integrand function to be calculated would be $\frac{w\omega f(x,y)}{144}$, where w and ω are the intervals of integration for x and y.

With this method of quadrature, multiple integrals can be handled just as easily as multiple sums, and it is hoped, therefore, that it will help in the calculation of new distribution functions of small sample theory when evaluation by analytical methods is difficult and numerical evaluation has so far been shirked because of the amount of computational labour.

(iv) *The solution of simultaneous equations on Hollerith machines*

The method of solving simultaneous linear equations here presented is efficient only if the number of variables is greater than 20. If several systems of simultaneous equations have to be solved at the same time, the method will deal efficiently with sets of equations with as few as five variables.

Equations involving a large number of variables arise, for instance, in the adjustment of triangulation surveys, whilst in statistical work one is often faced with the task of solving a whole set of systems of linear equations with the number of variables in each system small or moderate. Only broad principles of the method can be described here.

The present Hollerith process is the straightforward successive elimination of variables. The principle is to transform the matrix of coefficients into the triangular form as shown by the first three of a set of fifty equations in the pattern of coefficients shown in Table VII.

TABLE VII

Unknown Number

Equation No.	1	2	3	4	. . .	50	S
(1)	2·361 581	—·653 584	·356 981	—1·854 386	. . .	·035 189	7·858 392
(2)	0	3·581 735	—·035 786	·985 351	. . .	—1·386 584	·895 218
(3)	0	0	2·238 516	—1·002 531	. . .	·316 584	5·318 780
(4)	—·318 584	·567 814	1·003 516	·318 501	. . .	·001 384	2·386 009
⋮							
(50)							

In these three equations all coefficients to the left of the diagonal are 0. This form of the equations is sometimes called the reduced form. [Once *all* equations have been transformed into this reduced form the matrix is completely triangular, x_{50} is given by the last equation, and the remaining unknowns are calculated in turn ($x_{49} \ldots x_1$) from the other equations in a process known as back solution. This process constitutes only a small part of the work and is not dealt with here.]

In order to transform equation (4) to this form, we must multiply equation (1) by the ratio $\frac{\cdot 318\ 584}{2\cdot 361\ 581} = \cdot 134\ 903$ and add it to (4), finding for the first two coefficients 0 and ·479 644 respectively. We next multiply equation (2) by $\frac{-\cdot 479\ 644}{3\cdot 581\ 735} = -\cdot 133\ 914$ and add it to the other two, and so on. It is obvious that the main arithmetical operation is the multiplication of *all* the coefficients in a reduced equation by each of a large number of multipliers. For instance, all coefficients in the reduced equation (1) must be multiplied by ·134 903 to reduce equation (4), and similarly by the 48 other multipliers required for reducing the other equations. As soon, therefore, as a reduced equation is formed, we cater at once for all the multiples of its coefficients required in all subsequent eliminations. This is done by producing "ready-reckoner" cards giving the following "grocers-weight" system of multiples (m) of each coefficient:

10, 20, 40, 80 Also − 160
1, 2, 4, 8
0·1, 0·2, 0·4, 0·8

0·000 001, . . ., 0·000 008

One Hollerith card will cater for five consecutive coefficients of the equation—*e.g.*, the first card for $m = 2$ in equation (1) will provide twice the first five coefficients of this equation—*i.e.*, the quantities

4·723 162, 999 98·692 832, ·713 962, 999 96·291 228, 2(coefft. of x_5)

will be punched on this card. The multiples of the next set of five coefficients would be given on a second set of ready-reckoner cards, and so on. Any multiple of the coefficients of equation (1) can now be built up from these cards. For instance, to obtain ·134 903 times the reduced equation (1) we would have to select the ready-reckoner cards corresponding to $m = \cdot 1$, ·01, ·02, ·004, ·0008, ·0001, ·000 001, and ·000 002. The multiplier to be applied to the second equation would be − ·133 914. Such negative multipliers are throughout in this process printed as complements to 100000 so that the above multiplier would be shown as 99 999 866 086. We would therefore select the multiples − 160, 80, 40, 20, 10, 8, 1, ·8, ·04, ·02, ·004, ·002, ·000 08, ·000 004, ·000 002. *All* cards for $m = -160$, 80, 40, 20, 10 would be picked as *one* pack.

These cards are now added in the tabulator. Five counters are devoted to the reduction of five consecutive coefficients of equation (4). Into each counter we enter first the original coefficient of equation (4) (*e.g.*, ·318 501 is entered into the counter 4), then add to it the contributions from the ready-reckoner cards of the first, second and third equations. The results of this tabulation are the first five coefficients of the reduced equation. We may denote these by $a_{4,1}$ $a_{4,2}$ $a_{4,3}$ $a_{4,4}$ and $a_{4,5}$ (actually $a_{4,1} = a_{4,2} = a_{4,3} = 0$). At this break of control the tabulator will immediately summary-punch these five new coefficients (offset) thus making a card containing

$$10a_{4,1} \quad 10a_{4,2} \ldots 10a_{4,5}$$

i.e., the (new) ready-reckoner card for $m = 10$. The tabulator will then roll the contents of each counter into itself, thus doubling the contents of all counters, and then summary-punch $20a_{4,1} \ldots 20a_{4,5}$ on to a new card, thus making the ready-reckoner card for $m = 20$. Two more such cards (those for $m = 40$ and $m = 80$) are made in the same way, and finally all counters are doubled a fourth and last time to form $16a_{4,1} \ldots 16a_{4,5}$. This time, however, we arrange to summary-punch the complement of the counter contents (offset), thus producing the ready-reckoner card for $m = -160$, after which the counter is cleared. The plugging for this operation is very special, and certainly non-standard.

The tabulator will now immediately proceed to add the card with the next five coefficients of equation (4) ($A_{4,6} \ldots A_{4,10}$ say), followed by the appropriate ready-reckoner cards selected from those of equations (1) to (3), and thereby produce the next five coefficients $a_{4,6} \ldots, a_{4,10}$ and so on until $a_{4,46}, \ldots a_{4,50}$ are formed. This completes the reduction of equation (4) and the punching of the ready-reckoner cards corresponding to $m = 10, 20, 40, 80$ and -160 for all coefficients in this equation.

The sixth counter of the tabulator is reserved for checking purposes. It will add the indices m of the ready-reckoner cards in such a way that all multipliers are formed and printed, so that the operator will be able to see if any wrong ready-reckoner cards are in the pack.

To produce the remaining ready-reckoner cards we use the simple principle of copy-punching with digit shift in the reproducer punch (see Part I (iv) (a)). From each card $m = 10$ we copy-punch the corresponding cards for $m = 1, 0{\cdot}1, \ldots 0{\cdot}000\,001$ (and so on for 20, 40 and 80) by one offset gang-punch run. This produces all the ready-reckoner cards for the reduced equation (4).

Nothing has been said about the formation of the multipliers to be applied to each reduced equation and the method of selecting the ready-reckoner cards required for each reduction. Indeed, in the above example the multiplier for equation (2) is not known until the contribution from equation (1) has been added to the second coefficient. These and other difficulties have been overcome in the case of a *general* matrix, but we must confine ourselves here to a special case. In most statistical applications we are dealing with a *symmetrical* matrix that has arisen in the course of a least squares solution. Here the process of elimination follows the Gauss–Doolittle method (see, *e.g.*, (14)) with the advantage that the multipliers to be applied to each reduced normal for the reduction of the subsequent normals are known as soon as each reduced normal is formed. They are the coefficients of the reduced normal, divided by its diagonal term, and are produced on the tabulator (with virtually no change of plugging) by selecting the ready-reckoner cards corresponding to the reciprocal of the diagonal term formed on a calculating machine.

All selection of required ready-reckoner cards is done by hand-picking from an ordered file with tabbed guide-cards, so that the cards for *all* coefficients in an equation are picked as small packs, and *not* singly. The refiling of all ready-reckoner cards after each reduction is done automatically on the sorter. It is convenient to work with two copies of the ready-reckoner cards, tabulating cards from the one whilst picking from the other the cards required for the next reduction.

There is no essential limit to the number of variables that can be dealt with. The digital accuracy in the applications so far made is six decimals with two digits in front of the decimal point for all coefficients and multiples thereof.

The process has been completely tested, but so far only in experiments. It promises to be about four times as fast as the elimination method carried out on desk calculators. At the time of writing, preparations are being made to solve an actual case of 28 normal equations for 28 unknowns.

For equations with a large number of unknowns (say 50 or more) the method of hand-picking may become laborious. A method in which the selection of the required ready-reckoner cards is completely automatic has been developed, but this is *not* economical when the number of unknowns is small.

In conclusion I would like to acknowledge with gratitude the great improvements which Dr. L. J. Comrie has made in the presentation of this paper, which, incidentally, resulted in a 20 per cent. cut of the text!

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Discussion on Dr. Hartley's Paper

The Chairman. It gives me particular pleasure to move from the Chair that a vote of thanks be accorded to Dr. Hartley for his interesting and stimulating paper. Dr. Hartley first came to my notice in Cambridge as a pure mathematician, and I must take some responsibility for directing his activities, firstly to mathematical statistics; secondly to applied statistical work both at the Cambridge School of Agriculture and at the National Poultry Institute at Harper Adams Agricultural College; and thirdly to professional work on computation. In this Dr. Hartley was one more victim of the method I have practised in turning out statisticians. The method can be described very briefly. You catch a first-class mathematician, teach him the theory of statistics, then put him into a working laboratory which is performing an advisory function to experimental departments, where contact with the practical work not only stimulates the researches of the mathematician by bringing him up against the theoretical problems for which a solution is needed, but also promotes methodological and experimental advances, which depend on development of theory.

Dr. Hartley's first connection with the more elaborate forms of computation came about through my connection with the Mathematical Tables Committee of the British Association, which he got out of a difficulty in 1938 by serving for a time in the guise of computer, mainly on the National 6-Register machine. It is no accident that statistics and computation are associated. At the beginning of this century, when Professor Karl Pearson was doing his epoch-making work, numerical mathematics had not advanced beyond the limited stage needed by the ordinary mathematician and astronomer at that time, and it soon became evident that statistics required the evaluation of complex integrals by numerical methods, the construction of extensive mathematical tables, and the study of interpolation methods in one, two and more variables. Calculating machines, too, were in their infancy, and their use in the relatively routine calculations involved in summing squares and fitting frequency curves to grouped observational data did a great deal to popularize their use in scientific circles. Professor Karl Pearson used to tell the story of the mathematician, who shall be nameless, who came to him with an integral which he could not evaluate. Could Professor Pearson suggest a method? Professor Pearson's reply, as he twiddled the handle of the Edwardian model of calculating machine on his desk, was that *he* had never met an integral that he could not evaluate.

The development of the science, or should I say "art," of computation led in the 'thirties to the formation of Scientific Computing Service, Ltd., from which first Dr. Comrie came in 1936 and now Dr. Hartley has come to communicate to this Section their contributions to the art; and to the establishment at Cambridge of a Mathematical Laboratory as a special Department of the Faculty of Mathematics. More recently a Mathematics Division of the National Physical Laboratory has been set up at Teddington with a special statistical section, and somewhat similar activities to those being carried out in Cambridge are in operation in Manchester and Liverpool.

We are concerned to-day with computation applied to statistical calculations. These may range from the very simple to the complex, more perhaps because of the repetitive and arduous nature of the calculations than of their elaborate nature. Two extreme forms may be quoted from my own experience. In the 'twenties, when the new forms of experimental design in agriculture due to Professor R. A. Fisher were being tried out, I was roped in to go down to the Fens to help harvest some plots, in order that the statistician might be impressed with the amount of trouble he was causing the experimenter. It was a point of honour with me to produce the analysis of variance calculations, down to the final result, before the train reached Liverpool Street on the way home, and there was only the back of an envelope on which to work. The trick was to use the "dropped constant" method and to keep the observations to two figures ranging between 40 and 60, using the formula for $(50 \pm x)^2$ to work out the squares. Square roots were not quite so easy, but there was very little of this to do. At the other extreme we had before the war the "Greg bequest" experiment carried out by Dr. Hudson and Mr. Hey to throw light on the experimental errors, including sampling errors, for different sizes of plots and sampling units. Helped by Dr. Comrie, this was quite a complex piece of work using Hollerith machines, which turned my laboratory into a miniature factory for the time being.

There are a number of points in Dr. Hartley's paper which should stimulate discussion. I

hope I shall not be thought ungrateful, where so much has been given, if I suggest that more can be said about analysis of variance in its computational aspects than appears in the paper, where it is implied that because it is only a case of summing squares, it is a special case of the methods developed for dealing with multi-variate regression. The applications of the analysis of variance technique are to-day fairly numerous, and rightly so, and it would be helpful to put on record the experience which has been gained in systematizing the computations.

MR. E. C. FIELLER: I have much pleasure in seconding this vote of thanks, and I should like to congratulate Dr. Hartley on a paper that has the clarity that we have all come to expect from the Scientific Computing Service. Later speakers from the National Physical Laboratory can discuss the Hollerith and National Machines with much more intimate knowledge than I possess, and therefore I do not propose to talk at length. I have, however, one query, and one quarrel.

My query concerns the final section of Dr. Hartley's paper: what does he advise us to do, if the interval at which we have to tabulate an integral is so wide, that the fourth differences of the integrand are not negligible? Possibly the practical answer is, that such a situation rarely arises; a reasonable table ought to be interpolable by means of a second-difference formula, and if third differences of the integral are negligible, so are fourth differences of the integrand. This is not a complete answer, of course; a table in which third differences are not negligible can still be interpolated by means of Everett's second-order difference formula.

My quarrel is with one of Dr. Hartley's opening remarks: "many statisticians loathe figures." A statistician, surely, is somebody who makes sense of numbers, and I do not see how he can do that if he hates the sight of the things. I do not deny that there is a very useful class of people who dislike arithmetic themselves, but justify their existence by telling other people how to do it. To my mind, however, they are more appropriately described as statistical mathematicians; the statistician himself need not be good at arithmetic, since he can call on a multiplicity of aids of the type that Dr. Hartley has described and developed, but to do good thinking he must be interested in his figures, interested in the subjects they refer to, and interested in the results that his calculations produce.

The vote of thanks was put to the meeting and carried unanimously.

MR. MANDEVILLE, after adding his personal thanks for the paper and congratulating Dr. Hartley upon the ingenuity shown in the application of a Hollerith Tabulator to the solution of simultaneous equations, said that he felt that most of those who designed unusual applications of Hollerith were in fact standing on the shoulders of Dr. Comrie. Fifteen years ago he learned nearly all he knew of the Hollerith system from Dr. Comrie, and from his encouragement to consider all problems from first principles he had ever since been grateful. Hollerith machines were basically very simple machines, and the first principles of application were therefore simple. The machines were operated largely by a series of circuits similar to those used in the ordinary electric bell, and in fact they were often less complicated than the house bell. Mr. Mandeville showed on the lantern screen a diagram of the ordinary bell as compared with the circuit of the Hollerith distributor, pointing out that the distributor was the less complicated of the two. The great thing, he insisted, was to refuse to consider a mass of interwoven plugs and just to trace out one plug or circuit at a time.

He then showed a diagram illustrating how by the consideration of first principles a Hollerith tabulator could be used to multiply totals, and so weight results derived from additions of quantities recorded on cards. In the case of the National Farm Survey, in which stratified samples were taken from the records of various classes of farms, the theoretical proportions of the samples were modified for the sake of using the tabulator more effectively. Of A class farms 5 per cent. were taken instead of, say, 7 per cent., of B class 10 per cent., of C class 25 per cent., of D class 50 per cent., and of E class 100 per cent. The Hollerith tabulator could then add the acreages and other quantities relating to any class of farms, and produce totals weighted to give the 100 per cent. estimate for that class. This was done by using the distributor of the Hollerith tabulator to alter the position in which a quantity was added into a counter one digit to the left. This multiplied the total by 10. Thereafter advantage was taken of the capacity of the tabulator to transfer totals, and the fact that when totals are transferred from a counter back into the same counter the total in the counter is doubled, to double or quadruple the totals in the counters. Thus the totals for the A class farms were entered one place to the left in the counter by means of the distributor, and thereafter doubled by transferring them into themselves, which multiplied the 5 per cent. sample by 20 and gave the 100 per cent. The other classes were dealt with using one or both of these features of the tabulator to give weighted totals.

This was a good illustration of the statistician modifying the design of a piece of work to suit the machine, and thus making unnecessary hundreds of hours of hand calculation. It was rendered possible by a clear understanding of the first principles of the machine to be used.

MR. J. TODD was sure they were all greatly indebted to Dr. Hartley for revealing the latest ingenious devices of the computer in this field. Mr. Todd's interest in these matters began only

during the war, and the organization with which he was connected had only a small amount of statistical work. His experience was, therefore, very limited, particularly as it was his pleasure to give problems in computing and statistics as soon as possible into the capable hands of his colleagues, Mr. Sadler and Dr. Vajda. There were two points he would like to make, but before doing so he wished to put some comments on behalf of Mr. Sadler, who was unable to attend the meeting.

Mr. Todd then read a summary of the following contribution from Mr. D. H. Sadler.

"I wish to congratulate Dr. Hartley on the ingenuity which he exhibits in adapting calculating machines for scientific computation. Three methods in particular promise to be of considerably wider application than his immediate illustrations—namely, off-set gang-punching, the use of the National machine for quadratures, and solution of simultaneous equations by Hollerith machines.

"It seems difficult to understand, however, why Dr. Hartley should have used the quadrature formula based on function values at the tabular points for which the integral is required; the similar formula based on function values at half-way points is much more convergent and offers a more powerful method, which in fact illustrates his "tricks" more effectively.

"The formula is (in the notation of the paper):

$$\Delta I = w(f_{\frac{1}{2}} + \frac{1}{24}\Delta_{\frac{1}{2}}'' - \frac{17}{5760}\Delta_{\frac{1}{2}}^{\mathrm{iv}} + \ldots)$$

in which the first neglected term is about $+\frac{1}{2637}\Delta_{\frac{1}{2}}^{\mathrm{vi}}$ (as compared with $-\frac{1}{317}\Delta_{\frac{1}{2}}^{\mathrm{vi}}$). If now the coefficient $-\frac{17}{5760}$ is approximated to by $-\frac{16{\cdot}8}{5760}$ (*i.e.*, making an error of $+\frac{1}{28{,}800}\Delta_{\frac{1}{2}}^{\mathrm{iv}}$, as compared with $-\frac{1}{720}\Delta^{\mathrm{iv}}$ in Dr. Hartley's formula), the integral can be built up from the following multiples of some high-order difference of $g(x) = \frac{1}{24}wf(x)$:

$$+24; \ +1; \ -0{\cdot}07$$

"The principal objection to the use of the "half-way" formula is that it may require the special calculation of the function values at the half-way points; but Dr. Hartley's method implies that $g(x)$ will be specially calculated and, in general, there is no more difficulty in calculating for one value of x rather than another. The advantages are that no special difference "set-up" on the National machine is required, integration can be made from any convenient difference and the calculation of initial and check values is simplified. Further, the accuracy is greater and the last term can more frequently be neglected. The only disadvantage is that an extra fictitious figure has to be used in the integrations.

"It is interesting to compare the errors of the two alternative methods: (i) Dr. Hartley's at interval w and (ii) the "half-way" method at interval $2w$. If E represents a rounding-off error and N decimals are retained in $g(x)$ (which of course will be $\frac{1}{12}wf(x)$ in case (ii)), the errors in the increment of the integral in the two methods are:

$$\text{(i)}\ 24\,E.10^{-N} - \frac{1}{720}w^5f^{\mathrm{iv}} + \frac{1}{317}w^7f^{\mathrm{vi}} - \ldots$$

$$\text{(ii)}\ 24\,E.10^{-N} + \frac{1}{900}w^5f^{\mathrm{iv}} - \frac{1}{20}w^7f^{\mathrm{vi}} + \ldots$$

where f^{iv} and f^{vi} are the fourth and sixth derivatives and the coefficients of f^{vi} are approximate.

"Provided the derivatives are of the same order and except for very small number of figures with a correspondingly large interval, the critical term of the error of (i) is the second and not the third. It will thus be seen that, in general, method (ii) can be used at double the interval of method (i), with the added advantage that there will be only half as many intervals and half as many errors to accumulate."

Mr. Todd's first point concerned the problem of the solution of simultaneous linear equations, and indeed matrix arithmetic in general. He was not convinced that the capabilities of punched-card equipment here had been fully exploited. There had been nothing here so striking as Dr. Comrie's discovery of the possibilities of the National. It seemed reasonable to ask a mathematician to devise some new methods of solving problems in this field which were particularly suitable for putting on machines. The idea of solving a problem under assigned restrictions was quite an acceptable one, and the difficulty lay in formulating those restrictions.

Admiralty Computing Service (A.C.S.) began an investigation of methods of determining characteristic values for systems with a large number of degrees of freedom which were suitable for use with ordinary calculating machines. Dr. Aronzajn had provided them with some new and

very powerful methods, which Dr. Fox had tried out and found remarkably efficient. Unfortunately, Dr. Aronzajn returned to Paris before he had written up this work, and so far had not sent a full account. Dr. Fox could supply some more information. Since A.C.S. would become more mathematical in future, they had passed their information to the Oscillation Sub-Committee of the Aeronautical Research Council, who had in turn passed it to the Mathematics Division of the National Physical Laboratory for further development.

Secondly, there was a problem which confronted A.C.S. during the war and which might occur again in the case of Industrial Establishments, and on which Dr. Hartley's views would be valuable. Briefly, it was this: when does one install a punched-card equipment and how much? (Actually, they decided not to, and borrowed machines when they needed them.)

More precisely the decision was between ordinary machines (including Nationals), punched-card equipment (complete or incomplete) and electronic machines. The advantages and disadvantages of the first two were well known; of the third they had little information, and perhaps only faith.

The problem was to measure efficiency or economy in the large—was the measure "time"? To take some examples. Suppose they had decided to have punched-card equipment: should they dispense with a collator and do more sorting, or reproduce their pack and avoid back sorting, or should they dispense with a multiplying punch and use a tabulator, as suggested by Dr. Hartley?

Could these problems be formulated more precisely, and was there a reasonably quantitative answer? Or should they just call in Dr. Hartley to advise them?

DR. H. G. HUDSON wished first to express his appreciation at being allowed to be present to hear Dr. Hartley's most interesting paper and to take part in the discussion. There were a number of visitors there and he was sure they would wish him to join their thanks with his.

The Chairman had mentioned the value of commercial calculating machines in analysis of variance work. In interpreting the results of field trials in Agriculture this method of analysis was often used, but the number of plots, and thus the number of figures to be analysed, rarely exceeded 80, and was frequently between 20 and 30. Modern methods of experimentation, particularly those developed by Dr. Yates and called "confounded," meant that the plot totals had to be collected into sub-aggregates, and it became necessary to obtain the sums of squares relating to each sub-aggregate. The numbers of figures in each sub-aggregate were small (often only 2), but there might well be a considerable number of them. The computation then took rather a different aspect—that of many small calculations of varied nature rather than a few larger ones. He would not like Dr. Hartley to leave them with the impression that commercial calculating machines were of no value in this field; they were, in fact, indispensable; but rather different qualities were desirable, and unless a very large number of similar analyses were to be done, Hollerith machines were not as useful as desk calculators.

During the war we were told that our fighter aircraft were pre-eminent not only because of their high maximum speed, but rather because of that combination of great speed allied to great manœuverability which they possessed. Similarly, for analysis of variance work a machine must not be judged only by its "theoretical speed," but also by its flexibility and adaptability. A machine must therefore be judged solely in the light of the task which it was to be asked to perform, and it did not necessarily follow that the most expensive machine (or the machine with the most gadgets) would be the most useful for a given job. For example, there were certain highly developed machines on which it was difficult to calculate square roots with speed. Indeed, the speaker sometimes found himself wondering if the flexibility of the hand machine (such as the Brunsviga, especially that model on which it is possible to transfer numbers from the Product Register to the Setting Levers) did not make it preferable to some of the highly developed modern electrically operated machines for this work.

The second essential of this type of computing was careful planning. By using suitable methods on the machine (notably those outlined by Dr. Hartley in section B (1)), and clearing at suitable intervals, it was possible to form sub-aggregates and sums of squares in the same operation, and by assuming their correctness temporarily, methods could be devised whereby they were checked in the process of later calculations. With a well-thought-out and planned sequence of operations it was often possible to do all or most of the checking without any purely repetitive operations—*i.e.*, by what might be called planned checking rather than repetitive checking.

To Hollerith he had come as one who had had some familiarity at a time in the past. The mention of Collators, and even of Summary Punches, had brought home to him just how long ago that was, and what progress has been made more recently. He would like to close by asking a simple question for information. There was at one time talk of adapting the Rolling Total Machine to the analysis of variance and co-variance. Had such a practice been devised; if so, was it economical, and if so, how widely was it used?

DR. L. J. COMRIE read the following contribution from MR. G. B. HEY, who was unable to attend the meeting:

"I very much regret being unable to be present to-day to hear Dr. Hartley's most interesting

paper. Having worked with the author for many years, it is a pleasure to see the publication of even a small sample from the many new schemes that have been evolved for handling statistical and other computations.

"I must disagree with Dr. Hartley's way, on p. 164, of providing for gaps in a sequence. When N is about 30 we would only use a Hollerith if there were a number of groups to be done at the same time, and with the XY card method it is essential to put each group through the sorter and tabulator separately. This is very wasteful of time, and renders the checking rather more complicated. A simpler way, which works ideally, is to include a dummy set of cards punched 8, 7, 6 . . . 1, 0 in any necessary columns, making one such set for each group. This scheme usually adds the same total to every sum of products, but this is easily removed, and the entire process is quite automatic.

"On p. 166, where moving averages (but not totals) are required, I would suggest setting the data out in columns of 6 lines each, recording the total of each column. Now set one-sixth on a calculating machine, multiply it by the first total and record the product. Without clearing any register multiply by the difference, formed mentally, between the first values in columns 1 and 2, and record, and so on for the second to sixth pairs. The multiplier register should now contain the total of the second column. Continue the process on the second and third columns, ending with a repeat of the first column. The sum of the recorded results should equal the total of the original values except for rounding off errors. Dummy values are supplied to bring the original series up to an exact multiple of 6.

"The effect described on p. 167 could in practice be better obtained by punching 108 cards in field XXXVI only and gang-punching once. Dr. Hartley's scheme seems unnecessarily complicated both in theory and practice.

"I am intrigued by Dr. Hartley's scheme on p. 171 for getting 10, 20, 40, 80 and 160 times certain quantities punched automatically. This appears to require some internal re-wiring of the tabulator, and it would be interesting to hear how the effect is obtained.

"I would like to make two general observations. The first concerns checking—a matter that Dr. Hartley has mentioned on occasions, but which I feel he has not sufficiently stressed. With the processes he has described it is often much more difficult to devise a foolproof checking system than to plan the actual work. Statisticians and physicists, and even computers, seem to have an entirely unwarranted idea of their ability to do, or direct, a set of computations without error, and the whole subject of checking is worthy of a paper to itself.

"Secondly, I would like to protest about the difficulties in the way of those who desire to make use of the experience and machinery that exist. Computing technique in this country appears in many respects to be far more advanced than in America, but owing to the cost, and the difficulty of access, particularly to Hollerith machines, the use made of such facilities is much less here than in the States. Moreover, where Hollerith is fairly freely available, as in some Government Departments, I know from personal experience that it is often handled in a most inefficient manner, the excessive cost to the taxpayer being hidden by the mysteries of interdepartmental accounting.

"The methods Dr. Hartley has shown are of great power, and ought to be generally available to the scientific world at a price it can afford. The dangers of too much power are, however, serious, and the author's opening quotations on the danger of forgetting fundamentals are very appropriate. For instance, a 'statistic' was devised to summarize the meaning of a collection of data, yet with a Hollerith it *has* happened that the number of 'statistics' produced has exceeded the number of original data. Such is the nature of progress.

"In conclusion I would thank Dr. Hartley for his most interesting and stimulating paper."

Dr. Comrie then addressed a few words to the Section on his own account. He was glad that no one had got up that evening to talk about making special machines, because, before they made special machines to do these things, it was well to explore the possibilities of existing machines. The work about which they had been told was carried out with ordinary commercial machines, not particularly meant for statisticians, although suitable for their work. The Hollerith and the other machines that had been mentioned could perform, with the right technique, practically all that was wanted; if a person said that he was designing a machine he ought to be asked whether he had thoroughly explored those already existing.

Mr. Boss desired to thank Dr. Hartley for writing up in a convenient form several techniques which were not very well known. He wished to put it on record that Dr. Hartley had carried out his quite considerable work on punch-cards in the face of great difficulties, with machines which had been borrowed, and often in inaccessible places. The fact that these matters were not well known led him to another point—that very little work had been done, by very few people, in this country on such methods at all, subsequent to Dr. Comrie's pioneer work in the early 'thirties. Since the Hollerith machines were improved substantially in the middle 'thirties, very little use had been made of them, relatively speaking, for mathematical and statistical work. At the outbreak of war there was no modern installation working full time on mathematical or statistical work (he meant the type of calculation that Dr. Hartley mentioned in his paper). During the war two

installations had been set up, the second of which, at the National Physical Laboratory, was less than a year old. The explanation of why some of the questions had been asked that evening, and why the methods were evidently not known, might lie in that fact.

He wished to make three general comments on the paper. First of all, he thought that Dr. Hartley had understated the value of the digital multiplication which he had described. There were certain cautions, however, which he would like to mention in a moment. A point which Dr. Hartley did not make, and which should be stressed, was that punch-card techniques were very different indeed from other methods, and to attempt to argue from the one to the other was most dangerous. Furthermore, there was a fundamental difference between commercial technique in punch-card work and mathematical technique. As regards details, he did not think it was Dr. Hartley's intention that his remarks on digital accuracy should be applied to punch-card work. It helped enormously to work with small digits, but that did not necessarily apply to punch-cards, because one very soon got into negative quantities. Dr. Hartley had not stressed the fact that most methods talked about in the early part of the paper were concerned primarily with positive integers.

The method of summary multiplication was very simple, but he thought that certain points should be mentioned, including particularly the description given of the tabulator. The standard tabulator employed for commercial work had six counters of eleven wheels each. Unfortunately, from the statistical angle, the majority of commercial machines had to handle pounds, shillings and pence, and therefore the maximum decimal capacity of a counter was sometimes only six wheels and in certain cases eight. That did not detract from the value of the method: it only made it a little slower.

He had only one comment to make about the Hey tabulator. It was not, unfortunately, as young as it used to be, and it gave a certain amount of trouble. It had certain attractive features, but the method of linking counters again applied only to positive quantities.

He was sorry that Dr. Hartley did not round off his picture by saying a little more about the Hollerith (multiplier). The machine was certainly slow, but it was a machine which got through an immense amount of work in a quiet way. Furthermore, it was the only machine which would handle multiplication by many digits satisfactorily, in the sense that it was an 8×8 machine. It was not generally known that by sacrificing one digit, a Hollerith multiplier would also deal with the question of signs, and by the addition of a single relay would deal with all 8×8 multiplications and take account of the signs.

Mr. H. L. Seal, after thanking Dr. Hartley for his paper, said that he echoed Dr. Fieller in disagreeing with the opening statement that statisticians are not necessarily good at arithmetic. In his opinion, unless a man had worn out at least one calculating machine he was not a true statistician! He supposed that when Dr. Hartley described the Hollerith method of "summary multiplication," everyone realized that in fact the summation method of calculating an arithmetic average was being applied. It was of historical interest to mention that this was the procedure first utilized by Tetens, a Danish actuarial mathematician, in 1785, when calculating the value of an annuity whose successive payments were the cardinal numbers 1, 2, 3, . . . This was merely another illustration of the fact that throughout history efficient computers had again and again rediscovered useful dodges to help them in their work.

It might be mentioned that the method of quadrature devised for use with a National machine seemed to be suitable, with possibly slight modifications, for a Hollerith Rolling Total Tabulator in much the same way as the National differencing method (described by Comrie in the *Supplement*) could be modified for use with a Hollerith Tabulator.

In his reference to numerical quadrature the author had mentioned his application of the method described to the calculation of tables of the probability integral of the mean deviation of normal samples of n. The highly complicated formula used was derived by Godwin in *Biometrika*, XXXIII, 1945, 254, but the speaker wondered whether the work of the Italian actuary Tricomi would not have been preferable from the computational standpoint. Briefly, Tricomi had shown (*Giornale dell'Istituto Italiano degli Attuari*, VII, 1936, 280, and VIII, 1937, 68, 127) that (in Godwin's notation)

$$f_{n+1}(m) = (n+1)\sqrt{\frac{2}{\pi}}\int_0^{m(1+1/n)} e^{-\frac{1}{2}n^2[m(1+1/n)-t^2]} f_n(t)dt \quad . \quad . \quad . \quad . \quad (1)$$

and that the characteristic function of m for a sample of n is given by $\phi_n(-it)$ where

$$\phi_n(t) = e^{t^2/2n}\left[1 - \frac{2}{\sqrt{\pi}}\int_0^{t\sqrt{2}/n} e^{-x^2}dx\right]^n$$

Erdélyi (*l.c.*, VIII, 1937, 328) pointed out that $f_n(m)$ could be obtained from $\phi_n(t)$ by means of Doetsch's real inversion formula

$$f_n(m) = \lim_{k\to\infty}\frac{1}{\pi}\int_0^{\infty}\phi_n(t)E(mt, k)\,dt \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where
$$E(w,k) = \int_0^k \boldsymbol{R} \frac{w^{-\frac{1}{2}+iy}}{\Gamma(\frac{1}{2}+iy)} dy$$

and is a function whose numerical tabulation would prove of considerable value to statisticians. It would be interesting to ask Dr. Hartley to estimate by how much his calculations of the tables of $f_n(m)$ would have been shortened by the use of (1) or (2).

Dr. E. T. Goodwin said that the work which he had been organizing and carrying out had included few statistical calculations, and he could not add much of value to the discussion. He wished only to ask two questions. With regard to the application of the National machine mentioned on p. 168, could the author give some idea of the speed of this finite difference integration process? Its time-saving nature was most apparent when the integral was required for the tabular points at which the integrand was tabulated. When the integral was required for only a few tabular points it might be preferable to employ some alternative method. He was thinking in particular of the method whereby the first sum of the function is formed and simultaneously differenced on the National machine, the values of the integral then being given by the usual central difference formula.

He also desired to make a comment which Dr. Hartley might well regard as Joadish. On p. 158 he spoke of multiplication on an electric machine when the motor was controlled either by the operator through a motor-bar or by relays, and he called the first of these controls semi-automatic multiplication and the second fully-automatic multiplication. The former seemed to be, in essence, no more automatic than multiplication on a hand machine of the Brunsviga type, and he would prefer to reserve the term "semi-automatic" for the Marchant type of machine, where one strikes in succession the keys of a ten-key keyboard, as distinct from "fully-automatic multiplication," when multiplier and multiplicand are both set on keyboards and the operation of multiplication induced by striking a single key.

The following contributions were received in writing after the meeting:

Mr. J. L. Ineson: Dr. Hartley in Part II of his paper has given a selection of calculations involving the use of different types of machines. As a user of all of these types of machines, with the exception of the National accounting machine, I can well appreciate his difficulty in making a selection. During the past seven or eight years I have had the advantage of using an installation of Hollerith machines, including all the machines mentioned except the multiplier, for performing statistical calculations in connection with the loading of generating stations, and am convinced that a much greater use could be made of this type of machine in the solution of vastly differing kinds of problems. While it is true that punched-card equipment thrives on work involving very large numbers of cards and that, generally speaking, it is not a commercial proposition to install such complicated and expensive machinery to handle only small quantities of cards; nevertheless, there must be many installations in this country where punched-card equipment has already been installed for other purposes and where, if the users were alive to its possibilities in dealing with other types of work, a considerable amount of brain-fag and heart-breaking work could be avoided. For this reason, if for no other, Dr. Hartley's paper is particularly welcome, as it certainly does show what can be done with standard accounting machinery.

Quite apart from this aspect of the matter, it appears to me that there are advantages to be gained from the use of punched-card equipment, in that most of the operations are to a great extent automatic, if not entirely so, and can, under supervision, be carried out by junior staff. If, therefore, sums of square and products, for example, are to be worked out, it may be advantageous in the long run to use punched cards, even though the number of multiplications to be performed is relatively small.

As an example of the way in which much of the work becomes automatic when performed by the Hollerith equipment, consider the problem solved in Part II (i) of the paper by means of an adding and listing machine—namely, the calculation of moving totals and averages. Using Dr. Hartley's illustration, the figures of Table V would be punched into one field of a card, together with information indicative of the number of the observations in the series. By means of the reproducing punch this same series would then be reproduced into a second field on the cards, starting seven cards later, so that the seventh card contains, for example, the figures 0·312, originally punched, and 0·361 now introduced from the first card, and so on. A single run of these cards through the tabulator would then automatically give the 19 moving totals or moving averages, or both, as may be desired, and of course these results could simultaneously be punched into a new set of cards. The process by which the machine produces the results is identical with that carried out on an adding and listing machine described in the paper. Plugging diagrams have actually been worked out to punch by direct tabulation 21, 35, 53 and 365 cycle sliding averages correct to 4 significant figures, and there would seem to be little difficulty in arranging the plugging of the machine to give the averages over any other number of items. If sliding totals only are required, then the tabulating time is much shorter, but, apart from this, much laborious work is avoided if

the series is at all long, and, in any case, once the tabulator has been started no further attention is required, except to add further cards to the tabulator hopper and remove used and punched cards from time to time. Staff can therefore be employed on other work almost throughout the time taken to work out the figures on an adding and listing machine and calculator. The real bugbear, as with all Hollerith work, lies in the initial punching of the cards, and whether a Hollerith method is used or not depends for the most part on whether the trouble and time taken in punching the cards originally are worth while when weighed against the other advantages resulting from the use of the system. Often much depends on whether the cards have already been punched for another purpose or could be used for other purposes if they were punched, and a decision on which method to use is dependent on local circumstances.

There is one function of the Hollerith machines which Dr. Hartley has not mentioned in his paper, and which, to me at any rate, is one of its most important functions. This is the capacity of the tabulator to select one of a number of possible alternative answers to a calculation according to certain predetermined conditions communicated to the machines via the plug-boards. To give an example. The corresponding values of four variables—U_1, U_2, x and y—are given over a period of time, the number of sets of values normally being considered at one time being of the order of 18,000. It is desired to calculate for each set, the value of S determined by the following conditions, and to find the sum of the values of S over the whole range of values. The conditions are:

$$\begin{aligned} \text{If } U_1 + y - x = \Delta < 0 \text{ then } S &= U_2 - y \text{ if } U_2 - y > 0 \\ &= 0 \text{ otherwise} \\ \Delta \geqslant 0 \text{ then } S &= U_1 + U_2 - x \text{ if } U_1 + U_2 - x > 0 \\ &= 0 \text{ otherwise} \end{aligned}$$

Thus the machine has to calculate $(U_1 + y - x)$, $(U_2 - y)$ and $(U_1 + U_2 - x)$, examine the signs of each term, select the required answer, print or punch it, and transfer it to a counter where it can be stored up to give the required total. This is done merely by punching one card for each set of variables with the four values of these variables and passing the cards into the tabulator. The tabulator runs and produces the required results without any attention save the feeding and removal of cards. As a matter of interest, S is the minimum output of an inefficient generating station situated at the end of a circuit of capacity y which connects it to a more efficient generating station having available generating plant of capacity x, the requirements which have to be supplied at each end of the line being respectively U_2 and U_1.

There is just one other point which I think should be emphasized more fully, and that is the necessity for providing as many checks as possible on machine work. It is true that attention has been drawn in the paper to the checks which may be applied, but in my view—and I have no doubt that Dr. Hartley will agree—these checks are not optional, but obligatory. The fact that an answer to a problem is machine-made is in itself no guarantee of the accuracy of that answer, and even if it involves additional work, as in the example of the S-check, it is imperative that such a check be made.

In conclusion, I should like to record my pleasure at being allowed a preview of Dr. Hartley's paper and at having had an opportunity of joining in the discussion, if only at second hand. The literature on this subject published in this country is painfully sparse, and on that account, as well as on account of the excellence of the paper itself, my only regret is that the paper was not longer. I do sincerely hope, however, that it will be quickly followed by other papers on this or related subjects.

Dr. A. D. Booth: It was with considerable pleasure that I received an advance copy of Dr. Hartley's paper, and my chief feeling, after reading it, was one of regret at my inability to be present at the discussion.

I am sure that everyone present is full of admiration for the virtuosity with which commercially available machines have been adapted to uses completely foreign to those for which they were originally designed. I would, however, like to express the views of a confirmed "gadgeteer." As Dr. Hartley knows, I tend to the school of thought which considers that, when a sufficiently large number of any one type of calculation is to be made, a specially designed machine is always justified.

After considerable experience with calculations of the type

$$\rho(x, y, z) = \sum_{h, k, l} F(h, k, l) \cos 2\pi \left(h\frac{x}{a} + k\frac{y}{b} + l\frac{z}{c}\right)$$

where the summation extends over all values of (h, kl) in integer steps, positive and negative, and $\left(\frac{x}{a}, \frac{y}{b}, \frac{z}{c}\right)$ are numbers given to three or four decimal places), I have come to the conclusion that the point at which standard equipment breaks down is in the taking of the cosine. Although by a piece of sheer virtuosity Dr. Hartley and his colleagues have used Hollerith equipment for this purpose, I still have the feeling that it is rather a case of "steamroller and nut." I have recently designed, and have now under construction, a machine for this purpose. Its chief virtue lies in its

using only standard telephone equipment; and at a conservative estimate it should effect a saving of time by a factor of 5. In addition to the gain in speed, the new machine will be comparatively inexpensive (*ca.* £50, excluding labour), and when this total cost is compared with the cost of *hire* of Hollerith equipment the advantage is apparent. The relay machine has an exact electronic analogue and, although this would be rather more expensive, it would be possible to form the terms of the above summation at the rate of about 1000 per second.

Whilst this description applies only to the type of calculation specified, it is fairly obvious that the machine could be applied to any problem where it is necessary to enter mathematical tables and, in these cases, the advantages of an *ad hoc* machine would be equally great.

In conclusion, may I thank Dr. Hartley for a most interesting paper, and for "exposing" several of the tricks which must have been as much a mystery to other non-professional computers as they were to myself.

DR. H. O. HARTLEY said that perhaps he might be allowed to reply briefly to some of the points now, leaving further details to the reply in writing.

In the first place, he wished to thank all speakers for the constructive suggestions and criticisms made.

A number of questions had been asked about his method of quadrature on the "National" machine. Mr. Sadler asked why he had not used the formula giving the integral at arguments half-way between the tabular arguments of the integrand function. He agreed that this formula was better convergent, in fact he had used it in his original attempts at mechanized quadrature. However, experience had shown that the necessity for changing the interval of quadrature frequently arises. When narrowing down the interval of integration with the "half-way-point" formula it is often necessary to find a new starting value of the integral by interpolating between the values in the preceding wide interval panel. Moreover, when tabulating an integral, the interval must normally be taken sufficiently fine to make the final table linear, and there is no gain in using a wide-interval formula in the first place. Indeed, the requirement is usually for a series of quadratures, each to be taken over a short range, with the National method worth while only at a fine interval. It was because of these reasons that the method described in the paper had actually proved more useful in practice, although Mr. Sadler's formula had most of the theoretical advantages. Sometimes, with the range of quadrature long, one could use a method of quadrature with simultaneous sub-tabulation of the integral for which he had worked out details based on the half-way-point formula.

The question was closely linked with Dr. Goodwin's question whether the method should be used for the evaluation of an integral at isolated points or for a definite integral. For the latter the answer was in the negative, and the ordinary method of differencing with simultaneous summation of integrand values mentioned by Dr. Goodwin was faster, in the former case the decision depended on details.

In answer to Mr. Fieller, further terms could be added to the formula of quadrature, but the necessity seldom arose.

A number of speakers had taken exception to his remark "some statisticians loathe figures." He had been thinking of industrial statisticians who often preferred the graphical method and approach.

The Chairman and Dr. Hudson felt that more should have been given about the analysis of variance and more importance attached to flexibility of mechanized analysis rather than speed. The trouble with mechanizing small analysis of a varying character was that the overhead time, of setting the mechanism up becomes such a large proportion of the useful working time, and he would refer these speakers to his introduction. Mr. Kempthorne at Rothamsted had worked out details for doing 2^n analysis on a Hollerith tabulator, but he understood that the method was worth while only if quite a number of such experiments had to be analysed simultaneously. To the best of his knowledge there was no Hollerith tabulator specially adapted to analysis of variance work, the reason, no doubt, being that requirements were too scattered and varied.

He was interested in Mr. Seal's references to Italian work on the mean deviation, and would certainly follow up these references. His own contribution to the work referred to was confined to evaluating Mr. Godwin's formula numerically. He agreed that this had been quite a task, but it had been a case of "theirs not to reason why, theirs but to do and die."

Mr. Todd had raised the question as to general rules when Hollerith equipment should be used in place of desk calculators. No general rules could be given, as the decision would depend on a multitude of details of organization. As a very rough-and-ready rule the department concerned should first compare the existing method with the possibilities offered by Hollerith (using expert advice). Unless the Hollerith equipment would do the job in something like one-third of the original time it would not be worth while pursuing the matter further unless the time factor was all-important. Further, it should be the aim of the department to keep the Hollerith installation busy *all the time*. It would be seen therefore that the incitement would come from an increase in the computing programme of a department or from a reduction in staff.

Finally he would like to thank all speakers for the sympathetic reception his paper had received,

and in particular the Chairman, Dr. Comrie, Mr. Hey and Mr. Mandeville for the kind personal references.

Dr. Hartley subsequently wrote as follows:

I have already replied to some of the points at the meeting, and will therefore confine myself to dealing with the outstanding points and with the contributions in writing:—

Mr. Todd seemed to feel that in the scheme for solving simultaneous equations the capabilities of the punched-card system had not been fully exploited, and that other methods should be tried. I would say that Matrix Multiplication has been tried on Hollerith with rather less success, and that a machine specially built for scientific work (the Automatic Sequence Controlled Calculator, of which details have just been published in a Manual) also uses the Gauss-Doolittle elimination process with speeds not unlike those achieved here on ordinary commercial models very much smaller than the "American Monster." The elimination process has, in fact, been chosen *because* there are in it certain features suited to the punched-card system. Iteration, if known to converge, is actually better suited to using punched-card equipment, but, more often than not, conditions guaranteeing its convergence are not satisfied.

Dr. Comrie's principle of investigating the possibilities of existing machines before building a special one I regard as one of the most convincing lessons that I learned from him.

Mr. Hey raises a number of technical points: The coloured X, Y, space cards have given every satisfaction for *single* groups of as few as 50 cards working, sometimes, with duplicate packs (where N is small k has to be large for the Hollerith method to be worth while). In the case of several groups quoted by Mr. Hey his space cards alone would increase sorting and tabulating time by about 30 per cent., and I am not convinced that sorting of separate groups should add more delay. Also, it may not be possible to adjust all answers by standard amounts if the size of the fields vary and counter capacity is restricted.

Mr. Hey's suggested scheme of producing moving averages necessitates recopying the time series in a $6 \times N/6$ pattern—also, when the data consist of four-figure numbers or even more, people (like myself) somewhat weak at mental arithmetic might find the mental forming of differences tiring.

I am not convinced that punching 108 cards, each in one two-figure field, with a run of 72 cards through the Reproducer saved, should be more efficient than the punching of three (72-column) cards. I estimate the theoretical saving as about 1 minute Reproducer time set against a loss of about 7 minutes punch operators' time. Mr. Hey's suggestion was deliberately discarded in favour of the scheme given in the paper.

The scheme for getting multiples of 10, 20, 40, 80 and -160 for all counter contents does not require any special re-wiring, but necessitates breaking control five times with space cards.

I agree that the scheme for checking should be made an inherent part of the computing scheme, but the former is more dependent on the efficiency of the computing staff, and generalizations are therefore more difficult.

Mr. Boss has kindly mentioned the difficulties of carrying out Hollerith work on "borrowed" machines at "inaccessible" places. It was difficulties of this kind that Miss Gittus of the Scientific Computing Service had to face when she brought the experiment of solving 28 equations to a completely successful conclusion. It is due to her perseverance and patience that, since the paper was written, the method has been found to work in practice.

Mr. Seal suggested that the tabulation of the Mean Deviation tables could have been shortened by using formulæ derived by the Italian actuary Tricomi. I find, however (unless my limited knowledge of Italian deceives me), that Tricomi is concerned with the mean deviation about the known population mean (assumed to be 0) whilst Godwin's formula gives the distribution of the mean deviation about the sample mean which, naturally, turns out to be more complicated.

In reply to Dr. Goodwin, the speed of the method of quadrature on the National varies between 100 to 400 values per hour according to the length of runs, size and number of input differences and efficiency of operator. This time does not include any of the preliminary operations (*e.g.*, differencing). The usage of terms "fully automatic" and "semi-automatic" follows long-standing trade literature practice.

I was most interested in Mr. Ineson's contribution. I agree that Hollerith work is more automatic than calculations carried out on desk calculators, and that the actual operating can be performed by junior staff. However, the testing of plugging, elimination of faults, control and final checks are skilful jobs, and require staff with good qualifications and the right temperament.

I can visualize Mr. Ineson's method for calculating moving totals on Hollerith: as he says, it is a question of whether the punching of the cards is worth while. I imagine that his method of conversion into averages is multiplication of totals by the reciprocal ($1/6 = 0{\cdot}1667$) by successive rolling, and that it will take a large number of cycles to achieve this (I am counting that six cycles would be required). It might be better to summary punch moving totals, and to convert into averages on the Reproducer, using a master-card table (see Part I (v)), particularly if the series moving totals cover a limited range and, say, a month's output of moving totals can be "averaged" simultaneously.

The feature of distribution on a Hollerith tabulator has perhaps been wrongly neglected in the paper. Mr. Mandeville has, somehow, made up for this by giving his example of weighted averaging. Mr. Ineson has given another equally ingenious example involving the automatic sensing of the sign of a counter content. To make the plug-charts for either of these examples would, I am sure, have made Mr. Hey's heart jump with joy! But I think we are all agreed that when distribution is extensively used in a scheme there is, by its very definition, a large consumption of counter capacity. Such schemes are therefore excellent where counter capacity is available, but where not, distribution may seriously reduce output.

I was interested to hear about Dr. Booth's planned machine for doing Fourier synthesis. It is, however, not for me to comment on his comparison with the Hollerith method; firstly, because I was not associated with this development (which was carried out by Mr. Hey, and has been in successful use for three-dimensional synthesis, a calculation previously shirked), secondly, because Dr. Booth does not state under which conditions his quoted figures of cost and speed are applicable, and thirdly, because it is somewhat premature to compare the speed of a working machine with that of a planned construction. I would, however, remind Dr. Booth that in the case of serial correlation an assembly of Post Office relays resulted in a machine slightly slower than the method on a somewhat larger standard Hollerith installation. Here, as with Fourier synthesis, all depends on the scale of the work.

The "Effective" Number of Independent Observations in an Autocorrelated Time Series

By G. V. Bayley and J. M. Hammersley

Introduction

In the Symposium on Auto-Correlation in Time Series reported in the preceding part of this *Journal*, we made some reference (pp. 91–93) to our own work on the subject. This work was embodied in reports not generally available. The object of the present paper is to give in condensed form some of the results obtained. The paper should be read in conjunction with our own remarks and those of the other contributors to the Symposium.

Theoretical treatment

1. For a set of n independent readings of variance σ^2 we may write *:—

$$\text{var}\,(\bar{x}) = \sigma^2/n \quad \ldots \quad (1)$$

$$\text{var}\,(m_2') = 2\sigma^4/n \quad \ldots \quad (2)$$

$$\text{var}\,(s^2) = 2\sigma^4/(n-1) \quad \ldots \quad (3)$$

where

$$\bar{x} = \frac{1}{n}\sum_{j=1}^{n} x_j, \quad m_2' = \frac{1}{n}\sum_{j=1}^{n}\{x_j - E(x)\}^2, \quad s^2 = \frac{1}{n-1}\sum_{j=1}^{n}(x_j - \bar{x})^2.$$

Formulæ (1), (2), (3) will not hold if there is autocorrelation between the successive readings $x_1, x_2, \ldots x_n$. For such a case however, we may *define* numbers n_b^*, n_a^*, and n_v^* (which may be considered in a certain sense to be the "effective number of independent observations") so that

$$\text{var}\,(\bar{x}) = \sigma^2/n_b^* \quad \ldots \quad (4)$$

$$\text{var}\,(m_2') = 2\sigma^4/n_a^* \quad \ldots \quad (5)$$

$$\text{var}\,(s^2) = 2\sigma^4/(n_v^* - 1) \quad \ldots \quad (6)$$

In definitions (4), (5), (6), $\bar{x}$, m_2', and s^2 are defined to be unbiased estimates, *scil.*

$$E(\bar{x}) = E(x), \quad E(m_2') = E(s^2) = E\{x - E(x)\}^2 = \sigma^2 \quad \ldots \quad (7)$$

It follows that

$$\bar{x} = \frac{1}{n}\sum_{j=1}^{n} x_j \quad \ldots \quad (8)$$

$$m_2' = \frac{1}{n}\sum_{j=1}^{n}\{x_j - E(x)\}^2 \quad \ldots \quad (9)$$

$$s^2 = \left\{\frac{n_b^*(n-1)}{n(n_b^* - 1)}\right\}\left\{\frac{1}{n-1}\sum_{j=1}^{n}(x_j - \bar{x})^2\right\} \quad \ldots \quad (10)$$

2. If τ is the time interval between successive observations, so that $\sigma^2\rho(j\tau) = E(x_a x_{a+j})$ then

$$\text{var}\,(\bar{x}) = \frac{\sigma^2}{n} + \frac{2\sigma^2}{n^2}\sum_{j=1}^{n-1}(n-j)\rho(j\tau) \quad \ldots \quad (11)$$

$$\text{var}\,(m_2') = \frac{2\sigma^4}{n} + \frac{4\sigma^4}{n^2}\sum_{j=1}^{n-1}(n-j)\rho^2(j\tau) \quad \ldots \quad (12)$$

$$\text{var}\,(s^2) = \frac{2\sigma^4\{n^2(n-1) - 4n\Sigma_1 + 2n\Sigma_3 - 8\Sigma_4 - 4n\Sigma_6 - 8n\Sigma_7\}}{n^2(n-1)^2 - 4n(n-1)\Sigma_1 + 2\Sigma_2 - 2\Sigma_3 + 8\Sigma_4 + 8\Sigma_5} \quad \ldots \quad (13)$$

* In equations (2) and (3) it is assumed that $\mu_4 = 3\sigma^4$, *i.e.*, the distribution of the readings is mesokurtic.

where

$$\Sigma_1 = \sum_{j=1}^{n-1}(n-j)\rho(j\tau), \quad \Sigma_2 = \sum_{j=1}^{n-1}(n-j)(n^2+2n-4j)\rho^2(j\tau),$$

$$\Sigma_3 = \sum_{j=1}^{n-1}(n-j)(n^2-2j)\rho^2(j\tau), \quad \Sigma_4 = \sum_{j=2}^{n-1}\sum_{k=1}^{j-1}(n-j)k\rho(j\tau)\rho(k\tau),$$

$$\Sigma_5 = \sum_{=2}^{n-1}\sum_{k=1}^{j-1}(n-j)(n-k)\rho(j\tau)\rho(k\tau), \quad \Sigma_6 = \sum_{j=1}^{[\frac{1}{2}(n-1)]}(n-2j)\rho^2(j\tau),$$

$$\Sigma_7 = \sum_{j=2}^{n-1}\sum_{k=1}^{K}(n-j-k)\rho(j\tau)\rho(k\tau),$$

where the symbol $[\frac{1}{2}(n-1)]$ denotes the largest possible integer not greater than $\frac{1}{2}(n-1)$, and K is equal to $n-j-1$ or $j-1$ whichever is the less.

3. Alternatively we may express n^* in terms of n and ρ as follows:—

$$\frac{1}{n_b^*} = \frac{1}{n} + \frac{2}{n^2}\sum_{j=1}^{n-1}(n-j)\rho(j\tau) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$\frac{1}{n_d^*} = \frac{1}{n} + \frac{2}{n^2}\sum_{j=1}^{n-1}(n-j)\rho^2(j\tau) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

$$\frac{1}{n_v^*} = \frac{n^2(n-1) - 4n\Sigma_1 + 2\Sigma_3 - 8\Sigma_4 - 4n\Sigma_6 - 8n\Sigma_7}{n^3(n-1) - 4n^2\Sigma_1 + 2\Sigma_2 + 8\Sigma_5 - 4n\Sigma_6 - 8n\Sigma_7} \quad . \quad . \quad . \quad . \quad (16)$$

4. The above formulae (14), (15), (16) reduce to those given by Bartlett if certain approximations are made—*e.g.*, by writing n for $2(n-j)$ in (14) we obtain

$$n_b^* = n\Big/\sum_{j=0}^{n-1}\rho(j\tau)$$

and (15) can be reduced in a similar manner; or if in (16) we omit Σ_1, Σ_4, Σ_5, Σ_6, and Σ_7 and write n^3 for $2(n^2-j)(n^2+2n-4j)$ in Σ_2, and n^3 for $2(n-j)(n^2-2j)$ in Σ_3

$$n_v^* = n\left\{\frac{1 + \dfrac{1}{n-1}\sum_{j=1}^{n-1}\rho^2(j\tau)}{1 + \dfrac{n}{n-1}\sum_{j=1}^{n-1}\rho^2(j\tau)}\right\} \sim n\Big/\sum_{j=0}^{n-1}\rho^2(j\tau)$$

5. Before proceeding further it is appropriate to note the conditions for which relations (8) to (16) inclusive are valid. (8), (9), (10), (11), and (14) are perfectly general and hold whatever the distribution of the x's. (12), (13), (15), and (16) are obtained on the assumption that

$$\frac{1}{\sigma^4}E(x_r x_s x_u x_v) = \rho\{(r-s)\tau\}\rho\{(u-v)\tau\} + \rho\{(v-u)\tau\}\rho\{(s-v)\tau\} + \rho\{(r-v)\tau\}\rho\{(s-u)\tau\}$$

which is true if the x's are defined by a linear process whose random impulse function has a mesokurtic distribution. As a particular case they are true when the x's are normally distributed. It is further assumed in (13) and (16) that the parametric values of the ρ's are known; so that given n, n_b^* is known from (14). The evaluation of var (s^2) when allowance has to be made for the sampling errors of n_b^* presents a problem much harder than that discussed here.

6. Returning now to the general argument of the problem, let us suppose that our recordings are evenly spaced in time along a continuous process extending for a time T. Then

$$n \simeq T/\tau \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and the labour of computation will be directly proportional to n. We may define the efficiency of the computation by the percentage ratios

$$E_b = 100n_b^*/n, \text{ or } E_d = 100n_d^*/n, \text{ or } E_v = 100n_v^*/n \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

as the case may be. Thus by inserting trial values of τ in (17) and calculating E_b, E_d, or E_v we can assess the value of τ most suited to our purposes.

TABLE I

Time interval τ	No. of actual observations	Effective number of observations			Efficiency, %		
		n_b^*	n_d^*	n_v^*	E_b	E_d	E_v
0	∞	150	66	65	0	0	0
1	150	145	63	62	96	42	41
2	75	128	56	55	171	75	74
3	50	103	43	43	205	87	86
4	38	78	31	31	204	82	82
5	30	53	25	25	177	82	82
6	25	35	23	23	141	93	93
7	22	24	22	22	108	99	99
8	19	17	19	19	89	99	98
9	17	14	17	17	82	97	97
10	15	13	15	15	86	98	98

Practical application

7. Equations (14), (15), (16) are difficult to apply as they stand in practice, because of the labour required to evaluate them successively for a number of different trial values of τ. We modify them accordingly by assuming the functional form of the autocorrelation is

$$\rho(s) = e^{-\mu|s|} \cos \lambda s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

where λ and μ are constants peculiar to the autoregressive process under consideration. On substituting (19) in (14), (15), and (16), and neglecting $0(\exp - \mu n\tau)$ after the various summations have been effected, we obtain

$$\frac{1}{n_b^*} = \frac{1}{n}\left\{\frac{\sinh \mu\tau}{\cosh \mu\tau - \cos \lambda\tau}\right\} + \frac{1}{n^2}\left\{\frac{1 - \cosh \mu\tau \cos \lambda\tau}{(\cosh \mu\tau - \cos \mu\tau)^2}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

$$\frac{1}{n_d^*} = \frac{1}{2n}\left\{\frac{\sinh 2\mu\tau}{\cosh 2\mu\tau - \cos 2\lambda\tau} + \coth \mu\tau\right\} + \frac{1}{2n^2}\left\{\frac{-1}{\cosh 2\mu\tau - 1} - \frac{\cosh 2\mu\tau \cos 2\lambda\tau - 1}{(\cosh 2\mu\tau - \cos 2\lambda\tau)^2}\right\} \quad . \quad (21)$$

$$\frac{1}{n_v^*} = \frac{1}{2n}\left\{\frac{\sinh 2\mu\tau}{\cosh 2\mu\tau - \cos 2\lambda\tau} + \coth \mu\tau\right\} + \frac{1}{n^2}\left\{\frac{\sinh 2\mu\tau - 1}{\cosh 2\mu\tau - 1} - \frac{\sinh \mu\tau - \cos \lambda\tau}{\cosh \mu\tau - \cos \lambda\tau} - \frac{\frac{1}{4}(\cos 2\lambda\tau + 1)(\cos 2\lambda\tau - 9) + \frac{1}{2}(\cosh 2\mu\tau + 1)(3 \cos 2\lambda\tau + 1) - 2 \cosh \mu\tau \sin \lambda\tau \sin 2\lambda\tau}{(\cosh 2\mu\tau - \cos 2\lambda\tau)^2}\right\} + 0(n^{-3}) \quad . \quad . \quad (22)$$

For purposes of computation we write these equations as

$$n_b^* = nf_b(F, D)\Big/\left\{1 + \frac{1}{n}f_b'(F, D)\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$n_d^* = nf_d(F, D)\Big/\left\{1 - \frac{1}{n}f_d'(F, D)\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

$$n_v^* = nf_d(F, D)\Big/\left\{1 - \frac{1}{n}f_v'(F, D)\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

where
$$F = \lambda\tau/2\pi, \; D = \mu\tau \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

and $f_b, f_b', f_d, f_d', f_v'$ are functions which we have tabulated in Tables II to VI.

8. In practice we first estimate the quantities p and h from the sample correlogram, where p is the average period (in seconds) of the correlogram, and h is the average decrement per period of the autocorrelation coefficient which can be found by taking the average ratio $r(s + p)/r(s)$ for a number of values of s. Effect can be given to the greater reliability of $r(s)$ when s is small by weighting the ratios empirically according to the value of s. It is clear that

$$F = \tau/p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

$$D = - F. \log h \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

TABLE II

Effective number of independent observations (%) for bias (n *large*)

$100 f_b(F, D)$

D	F = 0, or any integer, +						
	·00 1·00	·05 ·95	·10 ·90	·20 ·80	·30 ·70	·40 ·60	·50
(L) $D \to O$	$50 \times D$	$4{\cdot}89/D$	$19{\cdot}1/D$	$69{\cdot}1/D$	$130{\cdot}9/D$	$180{\cdot}9/D$	$200{\cdot}0/D$
0·1	5	54	196	695	1311	1810	2001
0·2	10	34	104	353	660	909	1004
0·3	15	31	78	242	445	609	672
0·4	20	32	66	188	338	460	507
0·5	24	34	61	157	276	372	408
0·6	29	37	59	138	235	313	343
0·7	34	40	59	125	206	272	297
0·8	38	44	59	116	185	242	263
0·9	42	47	61	110	170	218	237
1·0	46	50	62	105	158	200	216
1·1	50	54	64	102	148	186	200
1·2	54	57	66	100	140	174	186
1·3	57	60	68	98	134	164	175
1·4	60	63	70	97	129	155	165
1·5	64	66	72	96	125	148	157
1·6	66	69	74	95	122	143	151
1·7	69	71	76	95	119	137	145
1·8	72	73	78	95	116	133	140
1·9	74	76	80	95	114	129	135
2·0	76	78	81	95	112	126	131
3·0	91	91	92	97	104	109	110
4·0	96	97	97	99	101	103	104
5·0	99	99	99	100	100	101	101
6·0	99	100	100	100	100	100	100

9. The limiting forms of (23), (24), (25) as $\tau \longrightarrow 0$ are of interest in that they give the maximum amount of information that can be extracted from a process of duration T. We obtain in this limiting case:—

$$\hat{n}_b{}^* = \frac{T}{p} A(h) \Big/ \left\{1 + \frac{p}{T} A'(h)\right\} \quad . \quad . \quad . \quad (29)$$

$$\hat{n}_d{}^* = \frac{T}{p} B(h) \Big/ \left\{1 - \frac{p}{T} B_d'(h)\right\} \quad . \quad . \quad . \quad (30)$$

$$\hat{n}_v{}^* = \frac{T}{p} B(h) \Big/ \left\{1 - \frac{p}{T} B_v'(h)\right\} \quad . \quad . \quad . \quad (31)$$

where

$$\left.\begin{aligned} A(h) &= (4\pi^2 + H^2)/2H, \quad A'(h) = (4\pi^2 - H^2)/H(4\pi^2 + H^2) \\ B(h) &= H(4\pi^2 + H^2)/(2\pi^2 + H^2), \quad B_d'(h) = (8\pi^4 + 2\pi^2H^2 + H^4)/2H(2\pi^2 + H^2)(4\pi^2 + H^2) \\ B_v'(h) &= (16\pi^4 - 22\pi^2H^2 - H^4)/2H(2\pi^2 + H^2)(4\pi^2 + H^2), \quad H = -\log h. \end{aligned}\right\} \quad (32)$$

A, A', B, B_d', B_v' and H are tabulated in Table 7.

10. The case of the Markoff process can be deduced from the foregoing results by taking $\lambda = 0$ (*i.e.*, $F = 0$ also). In the limiting case we have in place of (29), (30), (31)

$$\hat{n}_b{}^* = \frac{1}{2}\mu T \Big/ \left(1 - \frac{1}{\mu T}\right) \quad . \quad . \quad . \quad (33)$$

$$\hat{n}_d{}^* = \mu T \Big/ \left(1 - \frac{1}{2\mu T}\right) \quad . \quad . \quad . \quad (34)$$

$$\hat{n}_v{}^* = \mu T \Big/ \left(1 + \frac{1}{2\mu T}\right) \quad . \quad . \quad . \quad (35)$$

TABLE III

$f_b'(F, D)$

D	F = 0, or any integer, + ·00 / 1·00	·01 / ·99	·02 / ·98	·03 / ·97	·04 / ·96	·05 / ·95	·10 / ·90	·20 / ·80	·30 / ·70	·40 / ·60	·50
0·01	−100·0	+95·1	+97·5	+99·2	+99·7	+99·8	+100·0	+100·0	+100·0	+100·0	+100·0
0·02	− 50·0	+40·8	+47·5	+48·6	+49·4	+49·6	+ 50·0	+ 50·0	+ 50·0	+ 50·0	+ 50·0
0·03	− 33·3	+21·0	+29·8	+31·6	+32·5	+32·8	+ 33·2	+ 33·3	+ 33·3	+ 33·3	+ 33·3
0·04	− 25·0	+10·6	+20·3	+22·9	+23·8	+24·2	+ 24·8	+ 25·0	+ 25·0	+ 25·0	+ 25·0
0·05	− 20·0	+ 4·5	+14·5	+17·4	+18·5	+19·0	+ 19·8	+ 20·0	+ 20·0	+ 20·0	+ 20·0
0·06	− 16·7	+ 0·8	+10·5	+13·6	+14·9	+15·6	+ 16·4	+ 16·6	+ 16·6	+ 16·7	+ 16·7
0·07	− 14·3	− 1·5	+ 7·6	+10·9	+12·2	+13·0	+ 14·0	+ 14·2	+ 14·3	+ 14·3	+ 14·3
0·08	− 12·5	− 3·0	+ 5·3	+ 8·7	+10·2	+11·0	+ 12·1	+ 12·4	+ 12·5	+ 12·5	+ 12·5
0·09	− 11·1	− 3·8	+ 3·6	+ 7·0	+ 8·6	+ 9·4	+ 10·7	+ 11·0	+ 11·1	+ 11·1	+ 11·1
0·10	− 10·0	− 4·3	+ 2·2	+ 5·6	+ 7·3	+ 8·2	+ 9·5	+ 9·9	+ 10·0	+ 10·0	+ 10·0
0·11	− 9·1	− 4·6	+ 1·2	+ 4·5	+ 6·2	+ 7·1	+ 8·6	+ 9·0	+ 9·0	+ 9·1	+ 9·1
0·12	− 8·3	− 4·7	+ 0·4	+ 3·5	+ 5·3	+ 6·2	+ 7·8	+ 8·2	+ 8·3	+ 8·3	+ 8·3
0·13	− 7·7	− 4·7	− 0·2	+ 2·8	+ 4·5	+ 5·5	+ 7·1	+ 7·5	+ 7·6	+ 7·7	+ 7·7
0·14	− 7·1	− 4·7	− 0·7	+ 2·1	+ 3·8	+ 4·8	+ 6·5	+ 7·0	+ 7·1	+ 7·1	+ 7·1
0·15	− 6·6	− 4·6	− 1·1	+ 1·5	+ 3·2	+ 4·2	+ 6·0	+ 6·5	+ 6·6	+ 6·6	+ 6·6
0·16	− 6·2	− 4·5	− 1·4	+ 1·0	+ 2·7	+ 3·7	+ 5·5	+ 6·1	+ 6·2	+ 6·2	+ 6·2
0·17	− 5·9	− 4·4	− 1·7	+ 0·6	+ 2·2	+ 3·2	+ 5·1	+ 5·7	+ 5·8	+ 5·8	+ 5·9
0·18	− 5·5	− 4·3	− 1·9	+ 0·3	+ 1·8	+ 2·8	+ 4·7	+ 5·4	+ 5·5	+ 5·5	+ 5·5
0·19	− 5·2	− 4·2	− 2·0	− 0·0	+ 1·5	+ 2·5	+ 4·4	+ 5·1	+ 5·2	+ 5·2	+ 5·2
0·20	− 5·0	− 4·1	− 2·1	− 0·3	+ 1·1	+ 2·1	+ 4·1	+ 4·8	+ 4·9	+ 5·0	+ 5·0
0·30	− 3·3	− 3·0	− 2·3	− 1·4	− 0·5	+ 0·2	+ 2·1	+ 3·0	+ 3·2	+ 3·3	+ 3·3
0·40	− 2·4	− 2·3	− 2·0	− 1·5	− 1·0	− 0·5	+ 1·1	+ 2·1	+ 2·3	+ 2·4	+ 2·4
0·50	− 1·9	− 1·9	− 1·7	− 1·4	− 1·1	− 0·8	+ 0·5	+ 1·5	+ 1·8	+ 1·9	+ 1·9
0·60	− 1·6	− 1·5	− 1·4	− 1·3	− 1·1	− 0·9	+ 0·2	+ 1·1	+ 1·4	+ 1·5	+ 1·6
0·70	− 1·3	− 1·3	− 1·2	− 1·1	− 1·0	− 0·8	− 0·0	+ 0·9	+ 1·2	+ 1·3	+ 1·3
0·80	− 1·1	− 1·1	− 1·1	− 1·0	− 0·9	− 0·8	− 0·2	+ 0·6	+ 1·0	+ 1·1	+ 1·1
0·90	− 1·0	− 0·9	− 0·9	− 0·9	− 0·8	− 0·7	− 0·2	+ 0·5	+ 0·8	+ 0·9	+ 1·0
1·00	− 0·9	− 0·8	− 0·8	− 0·8	− 0·7	− 0·7	− 0·3	+ 0·4	+ 0·7	+ 0·8	+ 0·9
2·00	− 0·3	− 0·3	− 0·3	− 0·3	− 0·3	− 0·3	− 0·2	− 0·0	+ 0·1	+ 0·2	+ 0·3
3·00	− 0·1	− 0·1	− 0·1	− 0·1	− 0·1	− 0·1	− 0·1	− 0·0	+ 0·0	+ 0·1	+ 0·1
4·00	− 0·0	− 0·0	− 0·0	− 0·0	− 0·0	− 0·0	− 0·0	− 0·0	+ 0·0	+ 0·0	+ 0·0

$$n_b^* = \frac{n \, . \, f_b(F, D)}{1 + \frac{1}{n} f_b'(F, D)}$$

11. We can extend the treatment to the case where a random component has been superimposed on the autoregressive process, for which the autocorrelation function will then be

$$\left.\begin{aligned} \rho(s) &= 1, \; (s = 0) \\ \rho(s) &= a e^{-\mu |s|} \cos \lambda s, \; (s \neq 0) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

by first finding the values $n_b^*(1)$, $n_d^*(1)$, $n_v^*(1)$ which correspond to $a = 1$, using λ and μ appropriate to equation (36), and then applying the equations:—

$$\frac{1}{n_b^*} = \frac{1}{n} + a\left\{\frac{1}{n_b^*(1)} - \frac{1}{n}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

$$\frac{1}{n_d^*} = \frac{1}{n} + a^2\left\{\frac{1}{n_d^*(1)} - \frac{1}{n}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

$$\frac{1}{n_v^*} = \frac{1}{n} + a^2\left\{\frac{1}{n_v^*(1)} - \frac{1}{n}\right\} - \frac{a^2(1-a)}{n^2}\left\{2\left[\frac{1}{f_b(F, D)} - 1\right] + (1 + a)\left[\frac{1}{f_d(F, D)} - 1\right]\right\} \quad . \quad (39)$$

Typical cases of n* *and* E

12. We may consider firstly the case where the time series is very long and where n^* may be taken as equal to $nf(F, D)$. For a series T seconds long let $P = T/p$. Also for practical purposes $\tau = T/n$, *i.e.*, $F = \tau/p = P/n$, $D = FH = HP/n$.

TABLE IV

Effective number of independent observations (%) *for dispersion* (n *large*)

$100f_d(F, D)$

D	F = 0, or any integer, + ·00 ·50 1·00	·01 ·49 ·51 ·99	·02 ·48 ·52 ·98	·03 ·47 ·53 ·97	·04 ·46 ·54 ·96	·05 ·45 ·55 ·95	·10 ·40 ·60 ·90	·15 ·35 ·65 ·85	·20 ·30 ·70 ·80	·25 ·75
0·01	1·0	2·0	2·0	2·0	2·0	2·0	2·0	2·0	2·0	2·0
0·02	2·0	3·7	3·9	4·0	4·0	4·0	4·0	4·0	4·0	4·0
0·03	3·0	5·1	5·7	5·9	5·9	5·9	6·0	6·0	6·0	6·0
0·04	4·0	6·2	7·3	7·7	7·8	7·9	8·0	8·0	8·0	8·0
0·05	5·0	7·2	8·8	9·4	9·6	9·7	9·9	10·0	10·0	10·0
0·06	6·0	8·1	10·1	11·0	11·4	11·6	11·9	11·9	11·9	11·9
0·07	7·0	9·0	11·3	12·4	13·0	13·3	13·8	13·9	13·9	13·9
0·08	8·0	9·9	12·4	13·8	14·6	15·0	15·7	15·8	15·8	15·9
0·09	9·0	10·7	13·4	15·1	16·1	16·6	17·6	17·7	17·8	17·8
0·10	10·0	11·6	14·3	16·3	17·5	18·2	19·4	19·6	19·7	19·7
0·11	11·0	12·5	15·3	17·4	18·8	19·7	21·2	21·5	21·6	21·7
0·12	11·9	13·4	16·1	18·5	20·1	21·1	23·0	23·4	23·5	23·6
0·13	12·9	14·3	17·0	19·5	21·3	22·5	24·7	25·2	25·4	25·4
0·14	13·9	15·2	17·9	20·5	22·4	23·8	26·4	27·0	27·2	27·3
0·15	14·9	16·1	18·7	21·4	23·5	25·0	28·0	28·8	29·1	29·1
0·16	15·9	17·0	19·6	22·3	24·5	26·2	29·7	30·6	30·9	31·0
0·17	16·8	17·9	20·4	23·2	25·5	27·3	31·2	32·3	32·7	32·7
0·18	17·8	18·8	21·3	24·0	26·5	28·4	32·8	33·4	34·4	34·5
0·19	18·8	19·7	22·1	24·9	27·4	29·4	34·3	35·7	36·1	36·3
0·20	19·7	20·7	22·9	25·7	28·3	30·4	35·7	37·3	37·9	38·0
0·30	29·1	29·7	31·4	33·8	36·4	39·0	48·1	51·8	53·3	53·7
0·40	38·0	38·4	39·7	41·6	43·9	46·4	57·2	63·1	65·7	66·4
0·50	46·2	46·5	47·5	49·0	50·9	53·1	64·2	71·5	75·1	76·2
0·60	53·7	54·0	54·7	55·9	57·5	59·4	69·8	77·7	82·0	83·4
0·70	60·4	60·6	61·2	62·2	63·5	65·1	74·4	82·3	87·0	88·5
0·80	66·4	66·6	67·1	67·8	68·9	70·2	78·3	85·9	90·6	92·2
0·90	71·6	71·8	72·2	72·8	73·7	74·7	81·7	88·6	93·1	94·7
1·00	76·2	76·3	76·6	77·1	77·8	78·7	84·6	90·7	94·9	96·4
2·00	96·4	96·4	96·5	96·5	96·6	96·7	97·7	98·7	99·6	99·9
3·00	99·5	99·5	99·5	99·5	99·5	99·6	99·7	99·8	99·9	100·0
4·00	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0	100·0

We may therefore write:—

$$n_b^* = nf_b(F, D) = n\phi_b(n/P, h);\ \text{i.e., } n_b^*/P = \theta_b(n/P, h).$$
$$n_v^* = n_d^* \simeq nf_d(F, D) = n\phi_d(n/P\ h);\ \text{i.e., } n_v^*/P \simeq n_d^*/P = \theta_d(n/P, h).$$

Similarly E_b, E_d, E_v are functions of F and h whatever the value of P. We have calculated and graphed the expressions n_b^*/P, $n_v^*/P = n_d^*/P$, E_b and $E_v = E_d$ for the three values of $h = 0{\cdot}2$, 0·5 and 0·8. These values roughly cover the range of h often found in practice. n^*/P is shown as a function of n/P in Figures 1 and 2. E is shown as a function of F in Figures 3 and 4.

The gradients of the lines OA, OB, OC in Figure 1 are proportional to E_b at the selected points. C is the point of maximum efficiency in each case.

13. A number of deductions may be made from Figures 1 to 4, although it must be remembered that they are only valid for autocorrelations of the form $\rho(s) = e^{-\mu|s|} \cos \lambda s$. The efficiency of estimation of the mean has maximum values when $\tau \simeq 0{\cdot}375p$, $1{\cdot}45p$, $2{\cdot}5p$, $3{\cdot}5p$, $4{\cdot}5p$, . . . and minimum values occur when $\tau = p$, $2p$, $3p$, ... The efficiency of estimation of dispersion has maximum values when $\tau \simeq 0{\cdot}375p$, $0{\cdot}80p$, $1{\cdot}25p$, $1{\cdot}75p$, $2{\cdot}25p$, . . . and minimum values occur when $\tau = 0{\cdot}5p$, $1{\cdot}0p$, $1{\cdot}5p$, $2{\cdot}0p$, ... There is a negligible variation of these optimum efficiency points for the different values of h and we conclude that these rules hold true whatever the damping of the autocorrelation function.

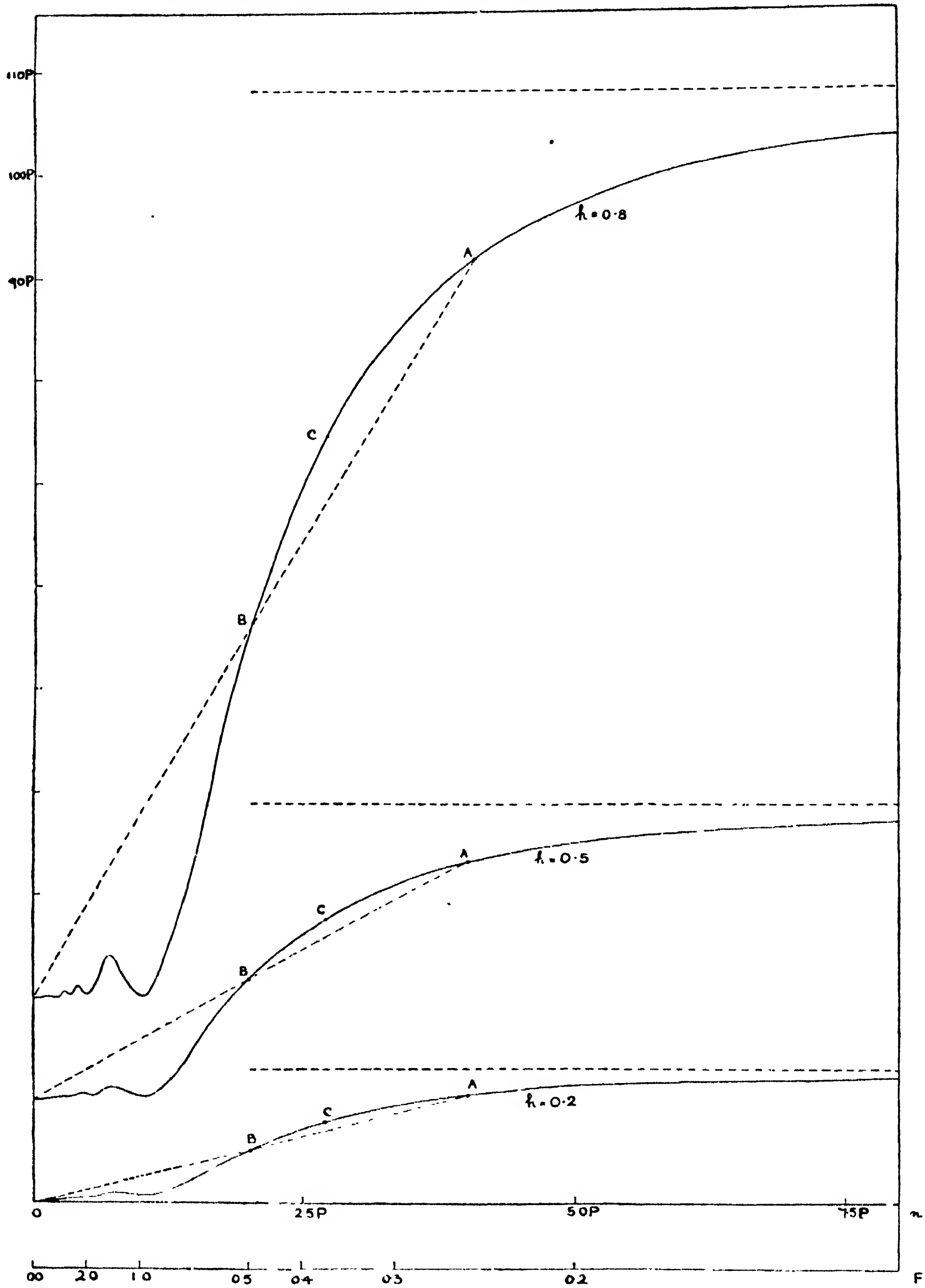

FIG. 1.

Effective number of observations for bias corresponding to n actual observations from a series of length P periods ($= T$ seconds). Graphs for $h = 0{\cdot}5$ and $h = 0{\cdot}8$ have been displaced vertically through $10P$ and $20P$ respectively.

14. Considerably greater efficiency than 100 per cent. can be obtained for the mean of the observations; the lighter the damping the greater the efficiency that can be achieved by a proper selection of τ. In general, the exact opposite applies to the estimation of dispersion. Here the lighter the damping the lower the efficiency that can be secured: we conclude that the reliability of the mean and standard deviation, respectively, of n observations from an autocorrelated time series, may depart considerably from their normal values—using n in the normal standard error formulae.

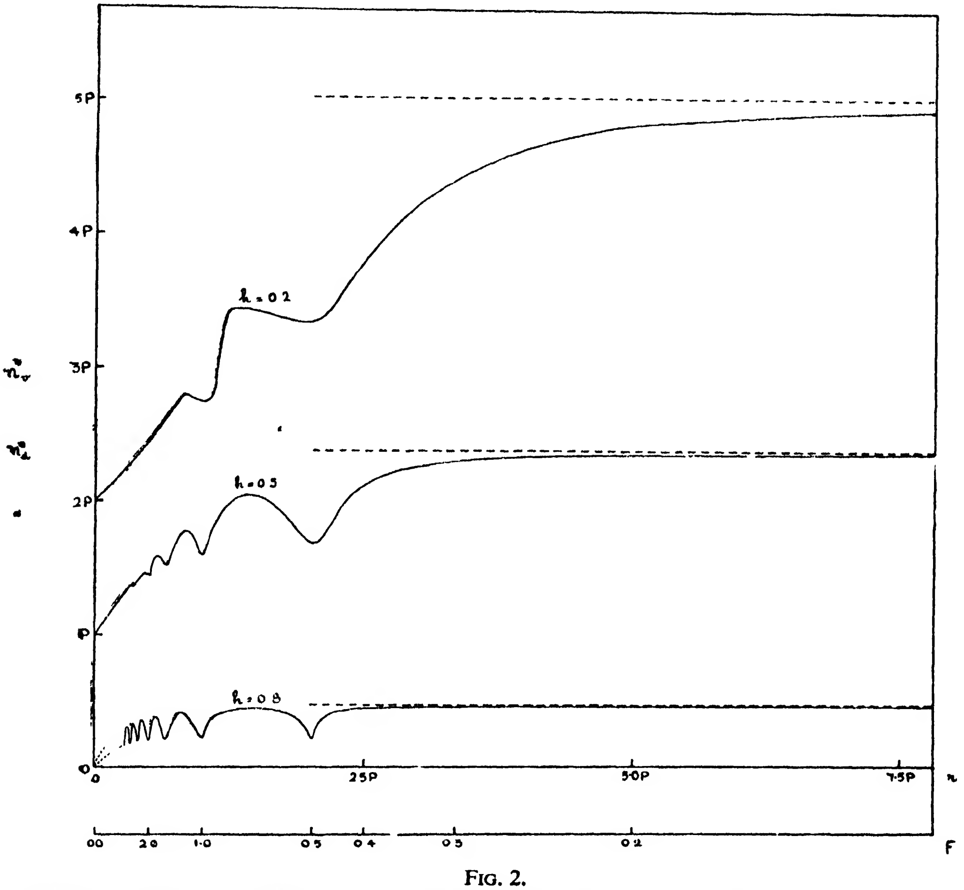

FIG. 2.

Effective number of independent observations for dispension, corresponding to n actual observations from a series of length P periods ($= T$ seconds). Graphs for $h = 0{\cdot}5$ and $h = 0{\cdot}8$ have been displaced vertically through P and $2P$ respectively.

15. Sometimes good estimates of the mean and dispersion are required from the same observations. The use of $\tau = 0{\cdot}375p$ has much to commend it. E_b and E_v have maximum values in this region and $n_b{}^*$ and $n_v{}^*$ are then reasonably close to their limiting values. There seems to be little advantage in ever reducing the time interval below about $p/5$.

16. Equation (12) may be written:

$$\text{var}\,(m_2') = \frac{2\sigma^4}{n}\left\{1 + \frac{2}{n}\sum_{j=1}^{n-1}(n-j)\rho^2(j\tau)\right\} \geqslant 2\sigma^4/n.$$

It follows that the standard error of the mean square error about the parametric mean is always greater than that given by the normal formula assuming the observations to be independent. We

do not know whether the same applies to the variance of the observations. The relatively low values of n_v^* that can sometimes arise emphasize the importance of selecting a few observations per series from as many separate series as possible, if an accurate assessment of dispersion is required.

17. The formula $n^* = nf(F, D)$ may be considered as applying to an infinite series. It is therefore important to observe the effect of introducing the correction to n^*, in the practical case

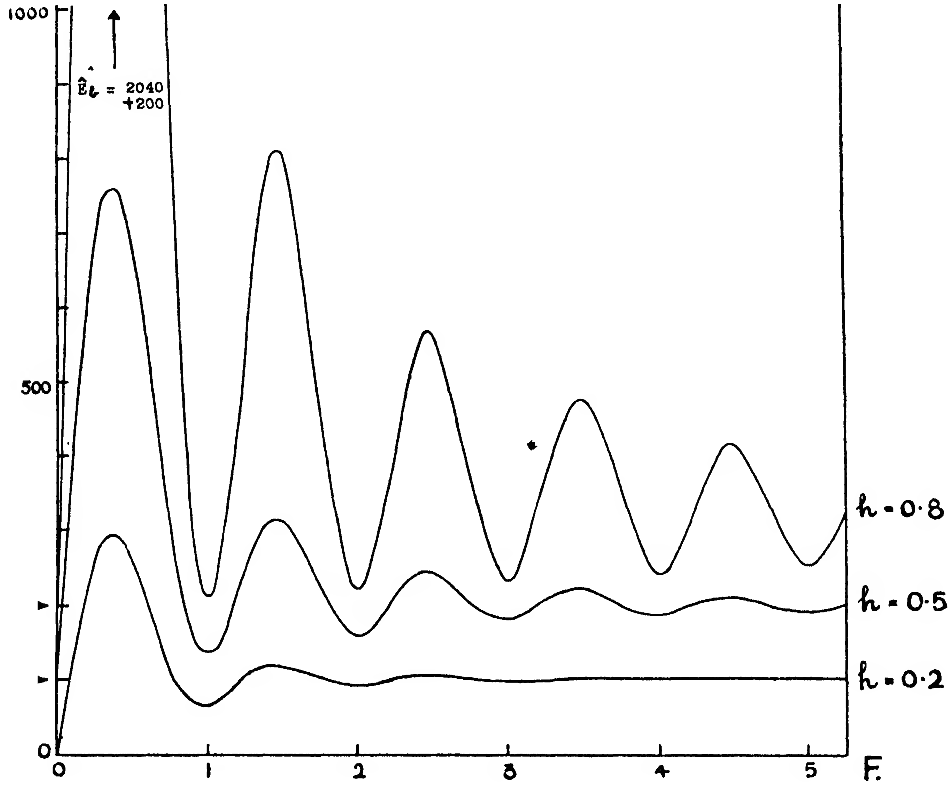

FIG. 3.

Bias Efficiency graphs showing the effective number of observations of (E_j), in a long series. Graphs for $h = 0{\cdot}5$ and $h = 0{\cdot}8$ have been displaced vertically through 100 and 200 respectively.

of series of limited length. It will be assumed however that the series is long enough to justify the assumption in para 7 that $0(e^{-\mu\tau})$ can be ignored.

(i) n_b^* and E_b. The peaks of the curves in Figs. 1 and 3 are depressed and the troughs are elevated by the term $\left\{1 + \frac{1}{n}f_b'(F, D)\right\}^{-1}$. The general shapes of the curves are not altered, and therefore the conclusions to be drawn are not altered.

(ii) n_d^* and E_d. Values of n_d^* are increased by the factor $\left\{1 - \frac{1}{n}f_d'(F, D)\right\}^{-1}$, which is always greater than 1. As τ is reduced in any given series this factor tends to the constant value $\left\{1 - \frac{p}{T}B_d'(h)\right\}^{-1}$. The general shapes of the curves in Figures 2 and 4 are not altered and the conclusions are unaffected.

(iii) n_v^* and E_v. The effect of limiting the series length depends upon the time interval chosen. The incorporation of the factor $\left\{1 - \frac{1}{n}f_v'(F, D)\right\}^{-1}$ reflects the reliability of the mean

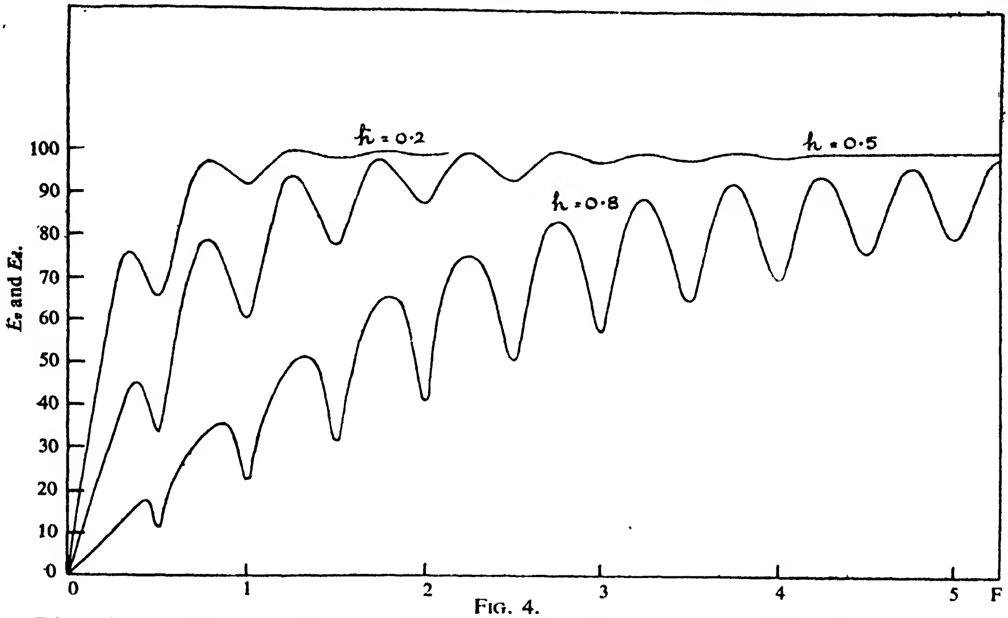

FIG. 4.
Dispersion "Efficiency" graphs showing the effective number of observations per cent. (E_v and E_d), in a long series.

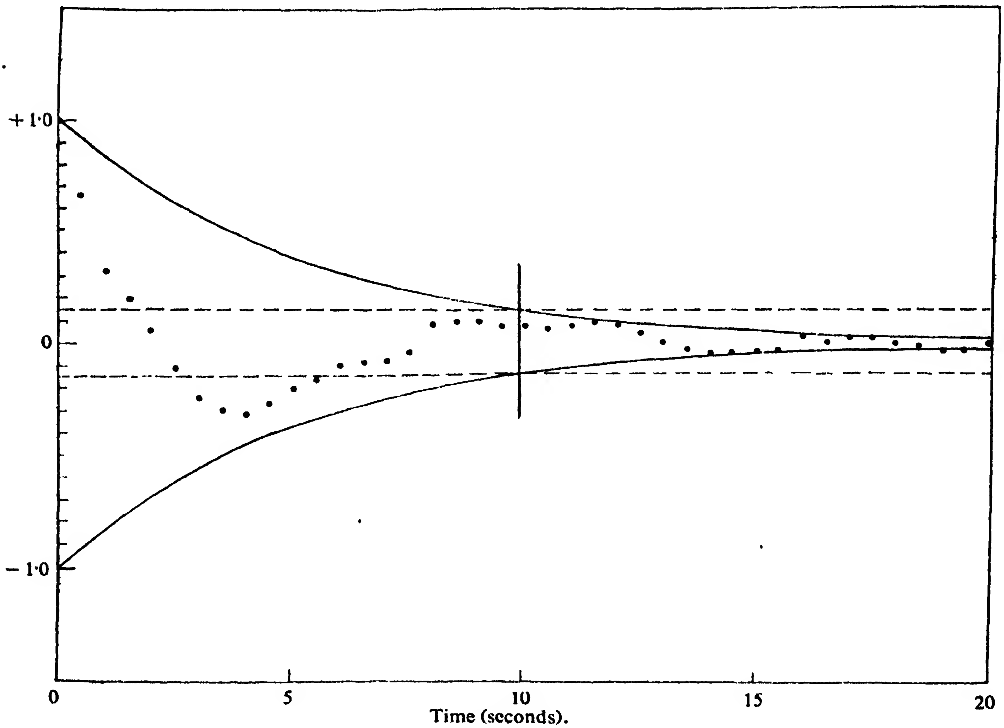

FIG. 5.
Correlogram for the lateral errors of a typical anti-aircraft radar set, and exponentially damped curves ($\pm e^{-0.2t}$).

obtained from the observations. Consequently where τ has values in the region of 0·375p, 1·45p, 2·5p, . . . the effect of the correction factor is to increase n_v^* appreciably. Conversely for $\tau = p, 2p, 3p, \ldots$ n_v^* is diminished. The shape of the curves in Figures 2 and 3 alter correspondingly, but the general conclusions reached are unaffected, unless the series is very short. In this event the fundamental assumption that $0(e^{-\mu\tau})$ can be ignored will probably break down also. Thus for very short series the use of the formulae can produce negative values of n_b^*, n_d^*, n_v^* due to the neglect of $0(e^{-\mu\tau})$.

TABLE V

$f_d'(F, D)$

D	F = 0, or any integer, +									
	·00 ·50 1·00	·01 ·49 ·51 ·99	·02 ·48 ·52 ·98	·03 ·47 ·53 ·97	·04 ·46 ·54 ·96	·05 ·45 ·55 ·95	·10 ·40 ·60 ·90	·15 ·35 ·65 ·85	·20 ·30 ·70 ·80	·25 ·75
0·01	50·0	47·6	49·4	49·7	49·8	49·9	50·0	50·0	50·0	50·0
0·02	25·0	21·2	23·8	24·4	24·7	24·8	24·9	25·0	25·0	25·0
0·03	16·7	12·4	15·0	15·9	16·2	16·3	16·6	16·6	16·6	16·6
0·04	12·5	8·5	10·6	11·5	11·9	12·1	12·4	12·4	12·4	12·4
0·05	10·0	6·6	7·9	8·8	9·3	9·5	9·9	9·9	9·9	9·9
0·06	8·3	5·5	6·2	7·0	7·5	7·8	8·2	8·2	8·3	8·2
0·07	7·1	4·8	5·0	5·8	6·2	6·5	6·9	7·0	7·1	7·0
0·08	6·2	4·4	4·2	4·8	5·3	5·5	6·0	6·1	6·1	6·1
0·09	5·5	4·1	3·7	4·1	4·5	4·8	5·3	5·4	5·4	5·4
0·10	5·0	3·7	3·2	3·5	3·9	4·2	4·7	4·8	4·9	4·9
0·11	4·5	3·5	2·9	3·1	3·4	3·7	4·2	4·3	4·4	4·4
0·12	4·1	3·3	2·7	2·8	3·0	3·3	3·8	4·0	4·0	4·0
0·13	3·8	3·1	2·5	2·5	2·7	2·9	3·5	3·6	3·7	3·7
0·14	3·5	3·0	2·4	2·3	2·5	2·7	3·2	3·3	3·4	3·4
0·15	3·3	2·8	2·3	2·1	2·2	2·4	2·9	3·1	3·1	3·1
0·16	3·1	2·7	2·2	2·0	2·1	2·2	2·7	2·9	2·9	2·9
0·17	2·9	2·6	2·1	1·9	1·9	2·0	2·5	2·7	2·7	2·7
0·18	2·7	2·4	2·0	1·8	1·8	1·9	2·3	2·4	2·5	2·6
0·19	2·6	2·3	1·9	1·7	1·7	1·7	2·1	2·3	2·4	2·4
0·20	2·4	2·2	1·8	1·6	1·6	1·6	2·0	2·2	2·2	2·3
0·30	1·6	1·5	1·3	1·2	1·1	1·0	1·1	1·2	1·3	1·3
0·40	1·2	1·1	1·0	0·9	0·8	0·8	0·7	0·8	0·8	0·8
0·50	0·9	0·8	0·8	0·7	0·7	0·6	0·5	0·5	0·5	0·6
0·60	0·7	0·7	0·7	0·6	0·5	0·5	0·4	0·3	0·4	0·4
0·70	0·5	0·6	0·5	0·5	0·4	0·4	0·3	0·2	0·2	0·2
0·80	0·4	0·5	0·4	0·4	0·4	0·3	0·2	0·2	0·2	0·2
0·90	0·3	0·3	0·3	0·3	0·3	0·3	0·2	0·1	0·1	0·1
1·00	0·3	0·3	0·3	0·3	0·2	0·2	0·2	0·1	0·1	0·1
2·00	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0

$$n_d^* = \frac{n \,.\, f_d(F, D)}{1 - \frac{1}{n} f_d'(F, D)}$$

Time interval necessary for virtually independent observations

18. As the time interval is increased n^* approaches n and the efficiency approaches 100 per cent. The rapidity of approach depends upon the rapidity of damping of the autocorrelation coefficients. It is often convenient to have readings virtually independent, and Tables II and IV suggest that D should not be less than 4·0 and 2·0 respectively. Clearly it is desirable to have as large a margin as possible over this minimum requirement. Since $D = \mu\tau$, the condition is that $e^{-\mu\tau}$ should not be less than 0·018 and 0·135 respectively. This principle is illustrated in Fig. 5.

Fig. 1 in the paper by Cunningham and Hynd, in the preceding part of this *Journal*, relates to the elevation errors of a typical anti-aircraft radar set. The correlogram given here relates to the lateral errors of the same radar. Both correlograms were obtained by taking for each value of s, the average of ten values of $r(s)$ obtained from ten separate series. The curves $\pm e^{-0\cdot 2s}$ intersect

the lines $\pm$ 0·135 at $s =$ 10 seconds. On the assumption that the correlogram damps within the limits $\pm e^{-0 \cdot 2s}$ we obtain 10 seconds as a minimum time interval (τ') to give approximate independence of observations for an estimate of dispersion. It can also be seen that for purposes of estimating the mean, τ' would be about 20 seconds. This approach is only justified of course when there is evidence that the correlogram damps according to the hypothesis. In these cases when $\tau > \tau'$ it seems reasonable to treat the observations as independent and to apply normal significance tests as necessary.

Practical example

19. We take as an example the particular radar set already discussed. Employing the correlogram in Fig. 1 of the paper by Cunningham and Hynd we estimate the constants

$$h = 0 \cdot 1, \; p = 9 \cdot 5 \text{ seconds.}$$

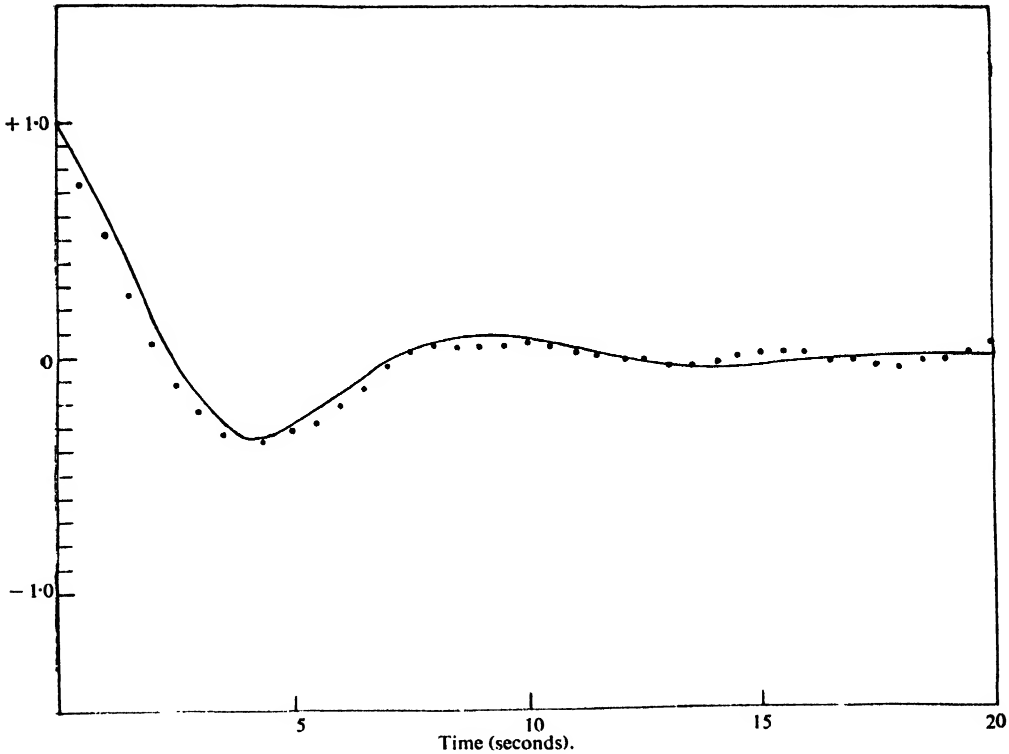

FIG. 6.
Correlogram for the elevation errors of a typical anti-aircraft radar set, and a fitted curve.
($h = 0 \cdot 1, p = 9 \cdot 5$ secs.)

The fitted curve on these assumptions is shown in Fig. 6. The average series length $T =$ 150 seconds and from this information we may readily prepare Table I.

From a table of this nature it is possible to weigh up the relative merits of any particular time interval for subsequent trials.

Tables

20. Although extensive tables would be necessary to permit linear interpolation, we have found Tables II–VI sufficient for most practical purposes. In some parts of the tables (*e.g.*, Table II for low values of D) it is sometimes as well to determine the values of the functions required directly from the formulae which define them.

TABLE VI

$f_v'(F, D)$

F = 0, or any integer, +

D	·00 1·00	·01 ·99	02 ·98	·03 ·97	·04 ·96	·05 ·95	·10 ·90	·15 ·85	·20 ·80	·25 ·75	·30 ·70	·35 ·65	·40 ·60	·45 ·55	·46 ·54	·47 ·53	·48 ·52	·49 ·51	·50
0·01	−49·0	+78·0	+92·7	+95·6	+96·7	+97·2	+97·8	+97·9	+98·0	+98·0	+98·0	+98·0	+98·0	+97·9	+97·9	+97·9	+97·7	+96·9	+149·0
0·02	−24·0	+14·9	+38·1	+43·4	+45·4	+46·3	+47·6	+47·8	+47·9	+48·0	+48·0	+48·0	+48·0	+47·9	+47·9	+47·8	+47·5	+46·6	+74·0
0·03	−15·7	−6·7	+17·7	+24·8	+27·6	+28·9	+30·7	+31·1	+31·2	+31·3	+31·3	+31·3	+31·3	+31·2	+31·1	+31·0	+30·7	+30·5	+49·0
0·04	−11·5	−14·3	+6·7	+14·8	+18·2	+19·8	+22·2	+22·7	+22·8	+22·9	+22·9	+23·0	+23·0	+22·8	+22·8	+22·6	+22·4	+23·1	+36·5
0·05	−9·0	−15·9	+0·2	+8·4	+12·2	+14·2	+17·0	+17·6	+17·8	+17·9	+17·9	+17·9	+17·9	+17·8	+17·7	+17·6	+17·5	+19·0	+29·0
0·06	−7·3	−15·0	−3·8	+4·0	+8·1	+10·2	+13·5	+14·2	+14·4	+14·5	+14·6	+14·6	+14·6	+14·5	+14·4	+14·3	+14·4	+16·4	+24·0
0·07	−6·1	−13·3	−6·2	+0·9	+5·0	+7·3	+11·0	+11·7	+12·0	+12·1	+12·2	+12·2	+12·2	+12·1	+12·0	+12·0	+12·3	+14·7	+20·4
0·08	−5·3	−11·5	−7·3	−1·2	+2·7	+5·1	+9·0	+9·9	+10·2	+10·3	+10·4	+10·4	+10·4	+10·3	+10·3	+10·3	+10·7	+13·3	+17·7
0·09	−4·6	−9·9	−7·9	−2·8	+0·9	+3·3	+7·5	+8·4	+8·8	+8·9	+9·0	+9·0	+9·0	+8·9	+8·9	+9·0	+9·6	+12·2	+15·6
0·10	−4·0	−8·5	−8·0	−3·9	−0·5	+1·9	+6·2	+7·2	+7·6	+7·8	+7·9	+7·9	+7·9	+7·8	+7·9	+8·1	+8·8	+11·2	+14·0
0·11	−3·6	−7·3	−7·8	−4·6	−1·5	+0·7	+5·2	+6·3	+6·7	+6·9	+6·9	+7·0	+7·0	+7·0	+7·0	+7·3	+8·1	+10·4	+12·6
0·12	−3·2	−6·3	−7·5	−5·1	−2·3	−0·2	+4·3	+5·5	+5·9	+6·1	+6·2	+6·2	+6·2	+6·3	+6·3	+6·6	+7·5	+9·6	+11·5
0·13	−2·9	−5·5	−7·0	−5·3	−3·0	−0·9	+3·6	+4·8	+5·2	+5·4	+5·5	+5·6	+5·6	+5·6	+5·8	+6·1	+7·1	+9·0	+10·5
0·14	−2·6	−4·8	−6·5	−5·4	−3·4	−1·6	+2·9	+4·2	+4·6	+4·9	+5·0	+5·0	+5·0	+5·1	+5·3	+5·7	+6·7	+8·4	+9·6
0·15	−2·4	−4·2	−6·0	−5·4	−3·7	−2·0	+2·4	+3·6	+4·1	+4·4	+4·5	+4·5	+4·6	+4·7	+4·9	+5·3	+6·3	+7·9	+8·9
0·16	−2·2	−3·8	−5·6	−5·3	−3·9	−2·4	+1·9	+3·2	+3·7	+3·9	+4·1	+4·1	+4·2	+4·4	+4·6	+5·0	+6·0	+7·4	+8·3
0·17	−2·0	−3·4	−5·1	−5·1	−4·0	−2·7	+1·4	+2·8	+3·3	+3·6	+3·7	+3·7	+3·8	+4·0	+4·3	+4·8	+5·7	+7·0	+7·8
0·18	−1·8	−3·0	−4·7	−4·9	−4·1	−2·9	+1·0	+2·4	+3·0	+3·2	+3·4	+3·4	+3·5	+3·8	+4·0	+4·5	+5·4	+6·6	+7·3
0·19	−1·7	−2·7	−4·3	−4·7	−4·1	−3·1	+0·7	+2·1	+2·6	+2·9	+3·1	+3·1	+3·2	+3·5	+3·8	+4·3	+5·2	+6·3	+6·8
0·20	−1·6	−2·5	−3·9	−4·5	−4·0	−3·2	+0·4	+1·8	+2·4	+2·7	+2·8	+2·9	+3·0	+3·3	+3·6	+4·2	+5·0	+5·9	+6·4
0·30	−0·8	−1·1	−1·8	−2·4	−2·8	−2·8	−1·3	−0·0	+0·6	+1·0	+1·2	+1·3	+1·5	+2·2	+2·5	+2·9	+3·4	+3·7	+3·9
0·40	−0·4	−0·5	−0·9	−1·3	−1·7	−1·9	−1·6	−0·8	−0·2	+0·2	+0·4	+0·6	+0·9	+1·6	+1·9	+2·1	+2·4	+2·6	+2·6
0·50	−0·2	−0·2	−0·4	−0·7	−1·0	−1·2	−1·5	−1·0	−0·5	−0·2	+0·1	+0·3	+0·6	+1·3	+1·4	+1·6	+1·7	+1·8	+1·9
0·60	−0·1	−0·1	−0·2	−0·4	−0·6	−0·8	−1·2	−1·0	−0·7	−0·4	−0·1	+0·1	+0·4	+1·0	+1·1	+1·2	+1·3	+1·4	+1·4
0·70	+0·0	−0·0	−0·1	−0·2	−0·3	−0·5	−0·9	−0·9	−0·7	−0·4	−0·2	+0·0	+0·3	+0·8	+0·9	+0·9	+1·0	+1·0	+1·0
0·80	+0·0	+0·0	−0·0	−0·1	−0·2	−0·3	−0·7	−0·8	−0·6	−0·4	−0·3	−0·0	+0·3	+0·6	+0·7	+0·7	+0·8	+0·8	+0·8
0·90	+0·0	+0·1	+0·0	−0·0	−0·1	−0·2	−0·5	−0·6	−0·6	−0·4	−0·3	−0·1	+0·2	+0·5	+0·5	+0·6	+0·6	+0·6	+0·6
1·00	+0·1	+0·1	+0·0	+0·0	−0·0	−0·1	−0·4	−0·5	−0·5	−0·4	−0·2	−0·1	+0·2	+0·4	+0·4	+0·4	+0·5	+0·5	+0·5
2·00	+0·0	+0·0	+0·0	+0·0	+0·0	+0·0	−0·0	−0·0	−0·1	−0·1	−0·1	−0·1	+0·0	+0·0	+0·0	+0·0	+0·0	+0·0	+0·0

$$n_v^* = \frac{n \, . \, f_d(F, D)}{1 - \frac{1}{n} . f_v'(F, D)}$$

TABLE VII

Calculation of the maximum number of independent observations in a series of T *seconds duration*

h	$H = -\log_e h$	$A(h)$	$A'(h)$	$B(h)$	$B_d'(h)$	$B_v'(h)$
0·1	2·303	9·7	0·331	4·118	0·177	0·073
0·2	1·609	13·1	0·545	3·032	0·277	0·327
0·3	1·204	17·0	0·772	2·326	0·388	0·594
0·4	0·916	22·0	1·046	1·795	0·524	0·905
0·5	0·693	28·8	1·408	1·370	0·704	1·298
0·6	0·511	38·9	1·932	1·015	0·966	1·850
0·7	0·357	55·5	2·785	0·711	1·393	2·727
0·8	0·223	88·5	4·469	0·446	2·235	4·432
0·9	0·105	187·3	9·482	0·211	4·743	9·465

$$n_b^* \text{ (max.)} = \frac{T}{p} \cdot \frac{A(h)}{1 + \frac{p}{T} \cdot A'(h)}$$

$$n_d^* \text{ (max.)} = \frac{T}{p} \cdot \frac{B(h)}{1 - \frac{p}{T} \cdot B_d'(h)}$$

$$n_v^* \text{ (max.)} = \frac{T}{p} \cdot \frac{B(h)}{1 - \frac{p}{T} \cdot B_v'(h)}$$

Average Sampling Numbers from Finite Lots

By S. Vajda

The following investigation is a by-product of some work connected with quality control, and it may be found useful to record the simple results.

Let a lot of N items be given. We choose single items and classify them as "effective" or "defective" in accordance with well-defined and objective principles. The items are not replaced. We decide to stop at the latest when $n(\leqslant N)$ items have been picked out, and earlier if we either find $c+1(\leqslant n)$ defective items (this mode of termination of sampling will be called "rejection"), or if we see that it is impossible to collect $c+1$ defectives even if we carry on testing up to n items (this case, and also that of reaching n without finding $c+1$ defectives, will be called "acceptance").

It will be obvious that these assumptions are descriptive of a form of sequential single sampling from a finite population. We will here only deal with the derivation of the average number it will be necessary to sample—the average sampling number.

This latter is clearly dependent on the number m of defectives in the lot, since the probability of finding a defective at any stage in the sampling process depends on m as well as on N. It will be obvious that if $m=0$, then the sampling must terminate by acceptance after $n-c$ items have been chosen, because these will not contain any defective. On the other hand, if $m=N$, we shall stop after the first $c+1$ items have all been found defective. But it would be a mistake to conclude that the average sampling number will always lie between $c+1$ and $n-c$ (inclusive). We will return to this question in due course.

1. Let us, then, investigate the position for any value of $m(O\leqslant m\leqslant N)$. The sampling may terminate by *rejection* after $c+1+i\ (i=0, 1, 2, \ldots, n-c-1)$ steps. This happens if exactly c defectives are found among the first $c+i$ items and if the last item is also defective. The probability for such an occurrence is

$$\cdot {}^{m}C_{i}\frac{m-c}{N-c-i}=\frac{1}{{}^{N}C_{m}}{}^{c+i}C_{c}\,.\,{}^{N-c-i-1}C_{m-c-1}.$$

${}^{a}C_{b}$ being zero if either $a<b$ or $b<0$ we find that rejection is impossible if $m\leqslant c$. This is also obvious from first principles.

Sampling may also end by *acceptance* after $n-c+j(j=0, 1, \ldots, c)$ steps. This occurs when there are exactly j defectives among the $n-c+j-1$ first items and when the last one is effective. In such a case, with only $c-j$ items left for sampling, it is obviously impossible to find the further $c+1-j$ defectives which would be necessary for rejection. The probability of such a sample is

$$\frac{1}{{}^{N}C_{n-c+j-1}}\,.\,{}^{N-m}C_{n-c-1}\,.\,{}^{m}C_{j}\frac{N-m-n+c+1}{N-m+c-j+1}=\frac{1}{{}^{N}C_{m}}\,.\,{}^{N-n+c-j}C_{m-j}\,.\,{}^{n-c-1+j}C_{j}.$$

Here we find that acceptance is not possible if $m\geqslant N-n+c+1$. This is again as it must be, because in this case even if all $N-n$ unsampled items are defective, there will still be at least $c+1$ left among the sampled ones. Nevertheless, we can formally use the above expressions even if they vanish, and we will return to this point when dealing with the final formulæ.

2. In what follows we shall use a lemma concerning the expression

$$\sum_{i=a}^{u-v}{}^{x+i}C_{y}\,.\,{}^{u-i}C_{v}\quad\ldots\ldots\ldots\ldots\quad(1)$$

and we will prove it at this stage in order to avoid interruptions in the main development.

We have obviously

$$\sum_{i=a}s_{i}t_{i}=s_{a}\sum_{i=a}t_{i}+(s_{a+1}-s_{a})\sum_{i=a+1}t_{i}+(s_{a+2}-s_{a+1})\sum_{i=a+2}t_{i}+\ldots$$

Applying this to expression (1) we have:

$$\sum_{i=a}^{u-v} {}^{x+i}C_y \,.\, {}^{u-i}C_v = {}^{x+a}C_y \sum_{i=a} {}^{u-i}C_v + {}^{x+a+1}C_y - {}^{x+a}C_y \sum_{i=a+1} {}^{u-i}C_v + [{}^{x+a+2}C_y - {}^{x+a+1}C_y] \sum_{i=a+2} {}^{u-i}C_v + \dots$$

$$= {}^{x+a}C_y \,.\, {}^{u-a+1}C_{v+1} + {}^{x+a}C_{y-1} \,.\, {}^{u-a}C_{v+1} + {}^{x+a+1}C_{y-1} \,.\, {}^{u-a-1}C_{v+1} + \dots$$

$$= {}^{x+a}C_y \,.\, {}^{u-a+1}C_{v+1} + \sum_{i=a} {}^{x+i}C_{y-1} \,.\, {}^{u-i}C_{v+1}.$$

The second term of this expression is seen to reduce to (1) on substituting $y-1$ and $v+1$ for y and v respectively. Applying this procedure once more we obtain:

$$ {}^{x+a}C_y \,.\, {}^{u-a+1}C_{v+1} + {}^{x+a}C_{y-1} \,.\, {}^{u-a+1}C_{v+2} + \sum {}^{x+i}C_{y-2} \,.\, {}^{u-i}C_{v+2}$$

etc., and finally

$$\sum_{i=a}^{u-v} {}^{x+i}C_y \,.\, {}^{u-i}C_v = \sum_{i=0}^{u-v-a} {}^{x+a}C_{y-i} \,.\, {}^{u-a+1}C_{v+1+i} \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

which is the lemma mentioned above. The upper limits could arbitrarily be increased without altering the values of the expressions, but it is essential that the upper limit should be at least the value up to which the binomial coefficients have values different from zero.

3. We proceed to calculate the average sampling number, S_n, say, for given N, n, m and c. We have:

$$ {}^NC_m \,.\, S_n = \sum_{i=0}^{n-c-1} {}^{c+i}C_c \,.\, {}^{N-c-i-1}C_{m-c-1}(c+i+1) + \sum_{j=0} {}^{N-n+c-j}C_{m-j} \,.\, {}^{n-c+j-1}C_j(n-c+j)$$

$$= (c+1)\sum_{i=0}^{n-c-1} {}^{N-c-i-1}C_{m-c-1} \,.\, {}^{c+i+1}C_{c+1} + (n-c)\sum_{j=0} {}^{N-n+c-j}C_{m-j} \,.\, {}^{n-c+j}C_{n-c}$$

$$= (c+1)\sum_{i=0}^{n-c-1} {}^{N-c-i-1}C_{m-c-1} \,.\, {}^{c+i+1}C_{c+1} + (n-c)\sum_{i=0}^{c} {}^{N-n+i}C_{m-c+i} \,.\, {}^{n-i}C_{n-c}$$

on writing $j = c - i$. . . . . (3)

If $m \leqslant c$, the first term disappears. If, in particular, $m = 0$, then we have $S_n = n - c$, because only the term for $i = 0$ remains in the second sum. If $m \geqslant N - n + c + 1$, the second term disappears, and if, in particular, $m = N$, then only the term for $i = 0$ remains in the first sum, so that then $S_n = c + 1$. The first sum can be transformed into a more convenient form, however, by applying our lemma (2). For this purpose we must write $\sum_{i=0}^{n-c-1}$ in the form

$$\sum_{0}^{N-c-1} - \sum_{n-c}^{N-c-1}.$$

We then obtain immediately

$$\sum_{i=0}^{c+1} {}^{c+1}C_{c+1-i} \,.\, {}^{N-c}C_{m-c+i} - \sum_{i=0}^{c+1} {}^{n+1}C_{c+1-i} \,.\, {}^{N-n}C_{m-c+i} \qquad . \quad . \quad . \quad . \quad (4)$$

The first term is seen to be ${}^{N+1}C_{m+1}$, so that we have

$$ {}^NC_m \,.\, S_n = (c+1)\left[{}^{N+1}C_{m+1} - \sum_{i=0}^{c+1} {}^{n+1}C_{c+1-i} \,.\, {}^{N-n}C_{m-c+i}\right] + (n-c)\sum_{i=0}^{c} {}^{N-n+i}C_{m-c+i} \,.\, {}^{n-i}C_{n-c} \qquad . \quad . \quad . \quad . \quad (5)$$

We find thus that if $m \geqslant N - n + c + 1$, then all the binomial coefficients in the sums disappear and S_n becomes

$$(c+1) \,.\, \frac{{}^{N+1}C_{m+1}}{{}^NC_m} = \frac{(c+1)(N+1)}{m+1}$$

which is independent of n. This fact was to be expected, because if m is sufficiently large, then a decision (by rejecting the lot) is bound to be reached at a stage before n items were investigated. For $m = N$, we have, of course, again $S_n = c + 1$.

For $c = 0$ or 1 the formula for S_n reduces to the following expressions which are convenient for computation:

$$c = 0 \qquad S_n = \frac{1}{{}^{N}C_m}\left[{}^{N+1}C_{m+1} - {}^{N-n+1}C_{m+1}\right] = \frac{N+1}{m+1} - \frac{{}^{N-n+1}C_{m+1}}{{}^{N}C_m}$$

$$c = 1 \qquad S_n = \frac{2}{{}^{N}C_m}\left[{}^{N+1}C_{m+1} - {}^{N-n+1}C_{m+1}\right] - \frac{n+1}{{}^{N}C_m}{}^{N-n+1}C_m.$$

By remembering that

$${}^{N+1}C_{m+1} - {}^{N-n+1}C_{m+1} = {}^{N-n+1}C_m + {}^{N-n+2}C_m + \ldots + {}^{N}C_m$$

it can easily be shown that for $c = 0$, S_n decreases (or remains equal) as m increases. However, this does not hold for higher values of c, as will be seen from the attached tables, which have been worked out for $N = 10$, and $c = 0$ and $c = 1$.

It is also easy to prove that for $c = 0$ and $n = 1$ the value of S_1 is always 1, whatever N. Clearly this result follows also from first principles.

Average sampling numbers

$$\frac{1}{{}^{N}C_m}\left[{}^{N+1}C_{m+1} - {}^{N-n+1}C_{m+1}\right]$$

$n = 10$, $c = 0$

m	$n = 1$	2	3	4	5	6	7	8	9	10
0	1	2	3	4	5	6	7	8	9	10
1	1	1·9	2·7	3·4	4	4·5	4·9	5·2	5·4	5·5
2	1	1·8	2·42	2·89	3·22	3·44	3·58	3·64	3·67	3·67
3	1	1·7	2·17	2·46	2·625	2·71	2·74	2·75	2·75	2·75
4	1	1·6	1·93	2·1	2·17	2·20	2·2	2·2	2·2	2·2
5	1	1·5	1·72	1·81	1·83	1·83	1·83	1·83	1·83	1·83
6	1	1·4	1·53	1·57	1·57	1·57	1·57	1·57	1·57	1·57
7	1	1·3	1·37	1·375	1·375	1·375	1·375	1·375	1·375	1·375
8	1	1·2	1·22	1·22	1·22	1·22	1·22	1·22	1·22	1·22
9	1	1·1	1·1	1·1	1·1	1·1	1·1	1·1	1·1	1·1
10	1	1	1	1	1	1	1	1	1	1

Average sampling numbers

$$\frac{2}{{}^{N}C_m}\left[{}^{N+1}C_{m+1} - {}^{N-n+1}C_{m+1}\right] - \frac{n+1}{{}^{N}C_m}{}^{N-n+1}C_m$$

$N = 10$, $c = 1$

m	$n = 2$	3	4	5	6	7	8	9	10
0	1	2	3	4	5	6	7	8	9
1	1·1	2·2	3·3	4·4	5·5	6·6	7·7	8·8	9·9
2	1·2	2·36	3·44	4·44	5·33	6·22	6·69	7·11	7·33
3	1·3	2·47	3·46	4·25	4·83	5·22	5·425	5·5	5·5
4	1·4	2·53	3·53	3·91	4·23	4·36	4·4	4·4	4·4
5	1·5	2·56	3·57	3·515	3·64	3·67	3·67	3·67	3·67
6	1·6	2·53	2·97	3·11	3·14	3·14	3·14	3·14	3·14
7	1·7	2·47	2·71	2·75	2·75	2·75	2·75	2·75	2·75
8	1·8	2·36	2·44	2·44	2·44	2·44	2·44	2·44	2·44
9	1·9	2·2	2·2	2·2	2·2	2·2	2·2	2·2	2·2
10	2	2	2	2	2	2	2	2	2

In both tables:

N = number in lot
m = number of defectives in lot
c = maximum number of defectives in a sample leading to "acceptance"
n = maximum number in sample.

The functions tabulated assume, of course, the values $c + 1$ (= 1 or 2) when $m = 10$, and $n - c$ when $m = 0$. We see further that in the table corresponding to $c = 0$ the values in all columns decrease with increasing m except for $n = 1$, when $c + 1 = n - c$, and therefore the two marginal—and extreme—values are equal.

Summary.

The purpose of the preceding note is to derive formulæ for the average sampling number necessary in sequential single sampling from a finite population or lot of N items. The maximum number of items in the sample is fixed at n, and sampling terminates earlier than this if either $c + 1$ defectives are found or if it becomes obvious that they could not materialize even if sampling were to continue until all n items had been drawn.

The probabilities for termination of sampling after any given number of items sampled are derived. The average number thus sampled is then calculated, and tables are computed for $c = 0$ and for $c = 1$ corresponding to a lot of 10 items.

The Use of the Negative Binomial Distribution in an Industrial Sampling Problem

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1. *Introduction*

In the binomial distribution with negative index, the probability of r occurrences is the coefficient of t^r in

$$(1 + p - pt)^{-K} \quad \text{. . . . .} \quad (1.1)$$

or

$$(1 - \xi)^K (1 - t\xi)^{-K} \quad \text{. . . . .} \quad (1.2)$$

where

$$\xi = \frac{p}{1 + p}.$$

The mean

$$\mu = Kp = \frac{K\xi}{1 - \xi}.$$

Thus the Poisson distribution whose mean is μ is a limiting case, with K infinite.

Kendall (1943) gives several examples of frequency distributions to which it gives a much better fit than the Poisson distribution. It should do so in particular when individual numbers are taken from Poisson distributions with varying means.* This case frequently occurs in practice, and the negative binomial should therefore be used more widely. It is hoped to show in this paper that it may be readily applied numerically.

This is mostly done with the aid of the Incomplete Beta function, for which two results are deduced in the appendix that appear to be new.

2. *Details of the industrial problem*

Batches of reels of enamelled wire are being tested for pinholes in the enamel. A batch is a consignment from one supplier, and usually has more than 50 reels each about 1,200 yards long. The test is on a reel over a standard length which is 50 yards to accord with a British Standard Specification and is drawn through a bath of electrically conducting liquid so that the insulation breaks down at every pinhole in the enamel.

Previously a 50-yard length had been tested on each of one third of the reels. Distributions of numbers of pinholes were then recorded. If any length had more than $\widehat{R}$ pinholes, every reel in the batch was tested. $\widehat{R}$ was 10 for the thickest gauges of wire (28 to 32 S.W.G.), and above 32 S.W.G. it was graded continuously to rise to 22 for the finest wire (47 S.W.G.). The problem was to find a more reliable criterion for accepting or rejecting the batches, which should, if possible, also require fewer reels to be tested.

3. *Relating the problem to the negative binomial*

The user of modern statistics often performs as valuable a service in defining a problem as in solving it. It is so in this case, where the quality of the product can vary continuously.

We first consider the method of sampling. Several miles of wire are made at one time, and it is afterwards divided into reels. These will be thoroughly mixed by the time they reach the testing department. Tests on successive lengths of one wire show little significant variation compared with that from one reel to another, so it may be assumed that within one wire the numbers of holes in 50-yard lengths have a Poisson distribution with a constant mean. But there is an inevitable and continuous change in conditions of manufacture leading to fluctuations, that are negligible over 50 yards, in the mean number of pinholes.

Therefore if the lengths tested are all on different reels, the result should be slightly more precise than that from a random sample from all the wire in the batch.

The difference should be negligible when only a small proportion of the reels are tested, for then the number of reels having more than one length on them tested would be too small to affect the

* The distribution is binomial with negative index if the means have a χ^2 distribution.

results, and it is this case for which the sampling should be most useful in saving labour. We shall, therefore, assume that we are taking 50-yard lengths at random from a population in which the number of pinholes is distributed as in (1.1).

The wire is to be wound on a coil, with many turns in 50 yards. If two pinholes come together, the insulation could break down. The points in contact are at a distance measured along the wire that is small compared with 50 yards. From these facts we shall derive a simple approximation to the quality of a batch, measured as a probability of leakage, in terms of the parameters of r.

We have to assume that there is a leak if any two pinholes overlap. If the area of each pinhole is δ, an equivalent condition is that the centre of one pinhole should be over a circle concentric to another pinhole, with twice its radius.

Let h_r be the mean number of leaks per unit length when there are r pinholes in the standard length l.

Starting from the centre of a pinhole, we take a point at an assigned distance along the wire on its surface, that depends on the coil winding. Then the probability that this point is within a circle of area 4δ concentric with another pinhole is

$$\frac{(r-1)4\delta}{2\pi a l},$$

if a is the radius of the wire. We can similarly start from each of the r pinholes in the length l, but this gives every leak twice, so that:

$$h_r l = \frac{r}{2}\frac{(r-1)4\delta}{2\pi a l} = \frac{r(r-1)\delta}{\pi a l} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.1)$$

This assumes, of course, that the leaks are distributed independently of each other over the standard length l, which is justified according to the discussion above.

Then over the whole length of wire the mean number of leaks h per unit length is given by

$$h = \sum_{r=0}^{\infty} h_r P_r = \frac{2\delta}{\pi a l^2}\sum_{r=2}^{\infty}\frac{r(r-1)}{2}P_r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.2)$$

Now $\sum_{r=0}^{\infty}\frac{r(r-1)}{2}P_r$ is half the second factorial moment of the distribution, and is, therefore, the coefficient of u^2 in the factorial moment generating function obtained by putting $t = 1 + u$ in the generating function $(1 + p - pt)^{-K}$, $\mu = Kp$, viz.:

$$(1 - pu)^{-K} = 1 + Kpu + \frac{K(K+1)}{2}p^2u^2 + \ldots \qquad \text{\{see Aitken (1939)\}}$$

Then
$$\sum_{r=2}^{\infty}\frac{r(r-1)}{2}P_r = \frac{K(K+1)}{2}p^2 = \tfrac{1}{2}\mu^2\left(1 + \frac{1}{K}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.3)$$

so that
$$h = \frac{\delta\mu^2}{\pi a l^2}\left(1 + \frac{1}{K}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.4)$$

The mean is, therefore, more important than the index in determining the quality of a batch. This is fortunate, because it is much more difficult to devise a simple but efficient test for controlling K than to find the mean, of which the distribution in a sample of N is known exactly for a given K.

In the results analysed K varied from batch to batch much less than μ. The best values for K were found for many samples by the method of maximum likelihood (appendix A), they were always fairly near to 1·5. When K was considerably smaller, corresponding to a more variable batch, μ was higher. If a particular reel had a very large number of leaks, it was usually evident from a visual inspection that it was unsatisfactory. A similar situation arises in many quality-control problems, where, in solving them, it is assumed that the mean of the quantity being sampled changes from batch to batch, whilst its variance remains constant.

For a given population mean μ, and $K = 1{\cdot}5$ we could find the distribution of the maximum R in a sample of N, and of the number of zeros. If either are larger than could be reasonably expected from sample fluctuations, the batch could be rejected even if its mean was satisfactory. This is not a very sensitive test for K, but should be a reasonable safeguard in practice against passing an unusually variable distribution.

4. *Principle of an acceptance test for controlling the mean*

For each gauge batches were defined to be " bad " if their mean number of pinholes per 50 yards exceeded a certain μ_b. (The method of assigning values to μ_b will be discussed later.) The criterion was that the probability of accepting on its sample mean a bad batch should be not more than 0·05 for any given number N in the sample.

However, if N was fixed in advance, there would be needless testing on good batches. Therefore 10 reels were usually tested at first, and if the resulting sample mean was too great for acceptance, N was increased to 15, and if necessary to 20, 30, and 60. As discussed more fully in paragraph 5, such a sequential system must increase the overall risk of accepting a bad batch, but this risk was kept reasonably small by limiting the number of permissible values of N.

Obviously the probability of acceptance is greatest for a batch that is just bad, with mean μ_b exactly. As discussed in § 3, we assume that $K = 1{\cdot}5$ all the time, and then the highest sample mean $\widehat{m}$ that will be accepted in a sample of N is the 5 per cent. point in the distribution of m from a population with mean μ_b.

To calculate $\widehat{m}$ it is most convenient to work with sample totals Nm. The probability of obtaining Nm in a sample is from (1.2), the coefficient of t^{Nm} in

$$(1-\xi)^{NK}(1-t\xi)^{-NK} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.1)$$

As pointed out in the introduction to Pearson's (1934) tables, the sum of a finite number of terms of this series is an Incomplete Beta Function. The relation is

$$\frac{K(K+1)\ldots(K+r+1)}{r!}\xi^r\left\{1+\frac{K+r}{r+1}\xi+\frac{(K+r)(K+r+1)}{(r+1)(r+2)}\xi^2+\ldots\right\}$$

$$= I_\xi(r,K) = \frac{\int_0^\xi t^{r-1}(1-t)^{K-1}dt}{\int_0^1 t^{r-1}(1-t)^{K-1}dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.2)$$

so that the probability of r or more in this distribution is

$$\sum_r^\infty P_r = I_\xi(r,K) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.3)$$

and the probability in a sample of N that the total is Nm or more is

$$I_\xi(Nm, NK) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.4)$$

corresponding to μ_b is a value of ξ, ξ_b given by

$$\mu_b = \frac{K\xi_b}{1-\xi_b} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.5)$$

The values of $\widehat{m}$ therefore satisfied the equation

$$I_{\xi_b}(N\widehat{m}, NK) = 0{\cdot}95 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.6)$$

with $K = 1{\cdot}5$.

The method of solving (4.6) is given in appendix B. Solutions (Table II), were obtained for six different values of ξ_b corresponding to various gauges (Table I).

It remains to say how the values of ξ_b were assigned. They were chosen so that about 6 per cent. of a batch with mean μ_b ("just bad") had more than $\hat{R}$ leaks in 50 yards, with $K = 1{\cdot}5$ and R having the same meaning as in § 2, and varying with the gauge number between 10 and 22 inclusive.

TABLE I

Distributions with K $= 1{\cdot}5$ *for various means* μ_b *assigned to correspond to batches that are just bad.*

Gauge, S.W.G.	ξ_b	$\mu_b = \frac{1\cdot5\xi_b}{1-\xi_b}$	Percentage equal to or exceeding r — r: 2	5	10	15	20
28·32	0·70	3·5	66·3	29·2	6·3	1·2	0·2
34	0·72	3·857	69·2	32·9	8·1	1·8	0·4
38	0·77	5·022	76·2	43·4	14·8	4·7	1·4
42	0·81	6·395	81·7	53·1	22·9	9·3	3·6
44	0·82	6·833	83·0	55·6	25·5	10·9	4·5
47	0·85	8·50	86·8	63·6	34·4	17·5	8·7

Table I, obtained directly from Pearson's Tables, gives batches that were "just bad" with the new definition for various gauges. Table II gives corresponding values of $\hat{m}$.

To find how the new criterion for acceptance with the above values of ξ_b is related to the old one, we require the distribution of the maximum in a sample of N, and this is found in the next section.

TABLE II

Solutions for $N\hat{m}$ of $I_{\xi_b}(N\hat{m}, 1{\cdot}5) = {\cdot}95$.

(Values of $N\hat{m}$ not to be exceeded in a sample of N, for gauges in Table I corresponding to ξ_b's.)

N.B.—For the practical application, obviously we only require the nearest whole number below $N\hat{m}$, and we do not guarantee that the decimal place is exactly right.

Values of $N\hat{m}$ for various values of N and ξ_b:

ξ_b	N: 5	10	15	20	30	60
0·70	7·75	19·5	32·9	47·1	76·5	168·7
0·72	8·1	21·7	36·5	52·2	84·7	186·6
0·77	10·9	28·8	48·6	69·7	111·5	244·6
0·81	14·1	37·3	62·4	89·0	143·7	313·3
0·82	15·2	39·9	66·9	95·0	153·2	335·0
0·85	19·2	50·1	83·9	119·0	191·9	418·3

5. *Distribution of the maximum* R *in a sample of* N

Let $Q_R = I_x(R,K)$ be the probability of getting at least R. Then the probability that R will not be attained in a sample of N is

$$(1 - Q_R)^N \quad . \quad . \quad . \quad (5.1)$$

We have obtained solutions of

$$(1 - Q_R)^N = 0{\cdot}5 \quad . \quad . \quad . \quad (5.2)$$

$$(1 - Q_R)^N = 0{\cdot}9 \quad . \quad . \quad . \quad (5.3)$$

for various values of N. (Table III.)

TABLE III

Solutions of $(1 - Q_R)^N = 0{\cdot}5, 0{\cdot}9$.

N	5	10	15	20	30	60
Q_R (satisfying (5·2)	0·12947	0·06697	0·04516	0·03406	0·02734	0·02284
Q_R ,, (5·3)	0·02100	0·01048	0·00700	0·00525	0·00420	0·00351

The corresponding percentage points of R are solutions of

$$I_x(R,K) = Q_R \quad . \quad . \quad . \quad (5.4)$$

where m is the sample mean and $x = \dfrac{m}{m+K}$.

Table IV gives these solutions for $K = 1{\cdot}5$ and various values of Nm, including in particular the nearest whole numbers below the values of $N\hat{m}$ in Table II. It is actually an inverse table obtained by interpolation from probabilities that a sample will have a maximum of R or more—since only integral values of R are physically possible.

It will be noticed that the 50 per cent. values of R for samples of 30 which just pass on the new test correspond approximately to the values of $\hat{R}$ and would just have been rejected on the old system with a batch size of about 100. However, since the testing is stopped as soon as a sample mean satisfies Table II, there can be a considerable saving in the amount of testing of good batches.

As discussed in § 3, we can now give a simple criterion for rejecting a sample, even if its mean value is satisfactory, for being too variable. If we take the highest integral value of Nm below $N\hat{m}$ and reject any sample whose maximum exceeds its 90% R of Table IV, one sample in 10 that just passes the test for sample means will be rejected wrongly, and this probability increases as the population value of K decreases. It is possible that it does not increase rapidly enough, and in that case the 75 per cent. point (say) should be chosen rather than the 90 per cent. point.

The question of a more sensitive test for K is another problem. Also there seems to be no published work on the problem of a joint variation in μ and K assuming no previous knowledge of K.

TABLE IV

50% and 90% points of the distribution of the maximum R in samples of N from populations with various means m *and* K = 1·5. (*N.B.* 50% *or* 90% *of the sample* R*'s are less than the values tabulated.*)

$N = 5$

Nm	15	12	10	9	8	7	6	
50 p.c. value of R … …	6·75	5·7	4·9	4·5	4·1	3·7	3·2	
90 „ „ … …	11·8	9·8	8·4	7·9	7·2	6·4	5·8	

$N = 10$

Nm	39	35	29	25	21	19	15	
50 p.c. value of R … …	10·7	9·8	8·2	7·3	6·4	5·9	4·9	
90 „ „ … …	17·0	15·5	13·4	11·8	10·5	9·5	7·9	

$N = 15$

Nm	66	60	54	48	45	42	32	27·5
50 p.c. value of R … …	13·5	12·3	11·3	10·4	9·7	9·1	7·5	6·5
90 „ „ … …	20·4	18·9	17·0	15·7	14·7	13·8	11·3	9·8

$N = 20$

Nm	99	85	69	66	63	47	0·42	
50 p.c. value of R … …	15·5	14·1	11·8	11·3	10·8	8·4	7·8	
90 „ „ … …	22·0	21·0	17·5	16·85	16·2	12·7	11·6	

$N = 30$

Nm	335	300	244	224	168	160		
50 p.c. value of R … …	23·0	20·9	17·3	16·1	12·7	12·1		
90 „ „ … …	31·5	28·5	23·9	22·0	17·4	16·7		

An obvious extension of the calculations is to have values of $N\hat{m}$ for every value of N. Dr. Bartlett has pointed out that we would than have a complete sequential system (Barnard, 1946, Wald 1945) for acceptance only. For example, such a system would be obtained by solving (4·6) for every value of N above, say, $N = 10$. The overall risk of accepting a batch that is just "bad" would then, of course, be much larger than 0·05, for it is the sum of probabilities of acceptance at $N = N_1$, and of non-acceptance at $N = N_1$ but acceptance at $N = N_2$, for $N_1 = 10, 11, 12, 13 \ldots$ and $N_2 = N_1 + 1$. With our system $N_1 = 10, 15, 20$, etc., and the corresponding values of N_2 are 15, 20, 30, etc., respectively, and of course a much smaller additional risk results. We did not consider the lengthy problem of evaluating this risk, especially as the definition of a bad batch is itself empirical.

Meanwhile it seems clear that there has been a considerable improvement on the previous system for accepting batches, and that the new one was fairly satisfactory under war-time conditions, in which it was necessary to test every reel in a batch which failed on the sample tests.

MATHEMATICAL APPENDIX

A.—*Fitting negative binomials to observed distributions by the method of maximum likelihood, with examples*

This section is included to show what we believe from experience to be the most convenient form of the maximum likelihood equations for μ and K for solving them numerically.

Fisher (1941) calculates the efficiency of the estimate of K obtained from the sample mean and variance, viz. :—

$$K = \frac{\bar{r}^2}{v - \bar{r}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.1)$$

where $\bar{r}$ is now used to denote the sample mean and v the sample variance. A rough test applying this formula shows that the estimate would not be very efficient in our case.

Writing $\xi = \frac{\mu}{\mu + K} = \frac{p}{1 + p}$ as before, Fisher's expression for the reciprocal of the efficiency is :

$$\frac{1}{E} = 1 + 2\left\{\tfrac{1}{3}\xi\frac{2}{K+2} + \tfrac{1}{4}\xi^2\frac{2.3}{(K+2)(K+3)} + \tfrac{1}{5}\xi^3\frac{2.3.4}{(K+2)(K+3)(K+4)} \cdots\right\}$$

Thus $\frac{1}{E} \doteqdot 1{\cdot}5$ when $\xi = 0{\cdot}75$ and $K = 1{\cdot}5$.

Haldane (1941) derives the equations of maximum likelihood and shows that we can easily solve them for p and K. It is more convenient in our case to introduce μ for the population mean, so that $p = \frac{\mu}{K}$.

Then from (1.1) and (1.2), if r occurs n_r times in the sample of N

$$\log P_r = -(r+K)\log(\mu+K) + r\log\mu + K\log K + \log(K+r-1)! - \log(K-1)! - \log r! \quad . \quad . \quad . \quad . \quad (A.2)$$

$$L = \sum_{r=0}^{R} n_r \log P_r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.3)$$

Write m and k for the maximum likelihood estimate of μ and K. Then

$$\frac{\partial L}{\partial \mu}(\text{at } \mu = m) = 0 = \frac{K}{m(m+K)}\left\{\sum_r rn_r - Nm\right\} = \frac{NK}{m(m+K)}(\bar{r} - m) \quad . \quad . \quad (A.4)$$

so, as is well known, $m = \bar{r}$.

The expression for $\frac{\partial L}{\partial K}$ involves the sum s_K defined by

$$s_K = \sum_{r=1}^{R} n_r F(K+r-1) - (N-n_0)F(K-1) \quad . \quad . \quad . \quad . \quad . \quad (A.5)$$

where the digamma function

$$F(x) = \frac{d}{dx}\log\Gamma(x+1).$$

Then

$$\frac{\partial L}{\partial K} = s_K + N\left\{1 + \log\frac{K}{K+\mu} - \frac{\bar{r}+K}{\mu+K}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.6)$$

Haldane (1941) prefers to avoid using the digamma function. His formula is equivalent to putting

$$s_K = \frac{1}{K}(n_1 + n_2 + \ldots n_R) + \frac{1}{K+1}(n_2 + n_3 + \ldots n_R) + \ldots + \frac{n_R}{K+R-1} \quad (A.5)'$$

This is no more laborious if the number of frequency groups is small, but much more so for computing if s_K is large.

At

$$K = k,\ \mu = m = \bar{r},\ \frac{\partial L}{\partial K} = 0,$$

and from (A.6):

$$m + k = ke^{\frac{s_k}{N}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.7)$$

which probably corresponds to the form which Haldane (1941) finds best for solving for k, according to his last paragraph.*

We have often found solutions near the minimum of $ke^{\frac{s_k}{N}}$ as a function of k. The estimates for k and m are independent, for

$$\frac{\partial^2 L}{\partial K \partial \mu} = \frac{N(m-\mu)}{(\mu+K)^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.8)$$

In calculating s_k we require values of $F(r+0)$, where $0 \leqslant 0 < 1$, for $r = 0, 1, 2, 3 \ldots$ in turn. For this Pairman's (1919) tables are arranged most usefully, but only go to $r = 20$. The B.A. Tables (1931) and those of Davis (1933) give values up to $r = 60$ and 50 respectively, but are arranged less conveniently. It was decided to copy all the values of $F(r+K)$ for values of r from 1 to 25, with $K = 0{\cdot}6, 0{\cdot}7, 0{\cdot}8 \ldots 1{\cdot}5$, so that all the entries for the same K were in one column. Values of s_K were then quickly completed on the machine.

m was between 2 and 7 and about a tenth of the samples had $k < 1$, but nearly always it was between 1 and 2. Much of the statistical analysis was on " bad " batches in which a 50-yard length on every reel had been tested, thus providing a large sample.

We append three of the distributions with a summary of the numerical work.

* There may be an error in this paragraph. Haldane's equation (2.1) is, with our notation, $N\{\log(m+k) - \log k\} = s_k$. He states that if this is multiplied by k one side increases with k and the other decreases. To obtain the form which we have found most convenient, one must first divide by N take the exponentials of both sides, and then multiply them by k.

TABLE Va

Observed frequencies in a batch of reels of 30 *S.W.G. wire*

r	0	1	2	3	4	5	6	7	8	9	10	11	15	16	17	19	25	30	43
n_r	9	12	10	20	11	12	6	10	5	7	6	2	3	1	1	2	2	1	1
$\bar{n}_r$ (calculated)	10·8	13·1	12·9	12·1	10·9	9·7	8·3	13·2			22·2					7·7			

Numerical values of:

N	Nm	m	$s_{1\cdot 1}$	$s_{1\cdot 5}$	$1{\cdot}5e^{\frac{s_{1\cdot 5}}{N}}$	k just above	$x = \frac{m}{m+k}$	χ^2	Degrees of freedom	P
121	726	6·01	238·7	195·1	7·53	1·5	0·8	8·07	7	0·32

More than half of χ^2 is in the discrepant value of n_3. On the other hand, some other isolated irregularities in the frequencies of the larger numbers are removed by the grouping necessary to make all the theoretical frequencies large enough for the χ^2 test.

TABLE Vb

Observed frequencies in a batch of reels of 36 *S.W.G. wire*

r	0	1	2	3	4	5	6	7	8	9	10
n_r	9	6	8	1	3	5	2	0	1	0	1
$\bar{n}$	9·2	8·8	8·0		5·0				5·0		

Numerical values of:

N	Nm	m	$s_{1\cdot 4}$	$1{\cdot}4e^{\frac{s_{1\cdot 4}}{N}}$	$s_{1\cdot 3}$	$1{\cdot}3e^{\frac{s_{1\cdot 3}}{N}}$	k	x	χ^2	Degrees of freedom	P (from χ^2)
36	520	2·67	38·05	4·03	40·15	3·965	1·28	0·676	3·1	2	0·22

TABLE Vc

Observed frequencies in a batch of reels of 44 *S.W.G. wire*

r	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
n_r	5	2	5	4	4	1	0	1	1	2	2	0	2	1	0	1
$\bar{n}_r$	9·0		7·1			6·8			4·7					3·4		

Numerical values of :

N	Nm	m	$s_{1\cdot 0}$	$e^{\frac{s_{1\cdot 0}}{N}}$	$s_{1\cdot 2}$	$1{\cdot}2e^{\frac{s_{1\cdot 2}}{N}}$	$s_{1\cdot 4}$	$1{\cdot}4e^{N}$	k	x	χ^2	Degrees of freedom	P
31	150	4·84	56·98	6·284	50·73	6·161	45·98	6·168	1·32	0·786	1·69	1	0·22

χ^2 was obtained by grouping all the frequencies from 7 upwards.

Since the distributions are of whole numbers, the values of χ^2 are possibly overestimated, but in any case the fits are as good as is needed for quality control. It is clear also how inaccurate it would be to assume that these are Poisson distributions.

B.—*Method of solving for* $\widehat{m}$ *the equation* $I_{1-\xi}(NK, N\widehat{m}) = 0$ *where* θ *is a small fraction*

Since

$$I_{\xi_\theta}(N\widehat{m}, NK) = 1 - I_{1-\xi_\theta}(NK, N\widehat{m}) \quad \text{(B.1)}$$

putting $\theta = 0{\cdot}05$ leads at once to the equation (4.6) in the form

$$I_{1-\xi}(1{\cdot}5N, N\widehat{m}) = 0{\cdot}05 \quad \text{(B.2)}$$

Since NK and $N\widehat{m}$ are both moderately large, Cochran's formula may be used. (Comrie and Hartley, 1941; Cochran, 1940). This is for ξ as an unknown, but we can adapt it to solve for $\widehat{m}$ by working with his equations in the following order:

$$A = \frac{4N\widehat{m}K}{K + \widehat{m}} \quad \text{(B.3)}$$

$$z = \frac{y}{\sqrt{A - \lambda_1}} + \frac{\lambda_2(\widehat{m} - K)}{N\widehat{m}K} \quad \text{(B.4)}$$

$$m = Kpe^{-2z} \quad \text{(B.5)}$$

where $p = \frac{\xi}{1-\xi}$ as before, $y =$ unit normal deviate exceeded by the proportion θ of the normal distribution ($y = 1{\cdot}6449$ at $\theta = 0{\cdot}05$)

$$\lambda_1 = \tfrac{1}{6}(y^2 + 3) = 0{\cdot}9509 \quad \text{for} \quad \theta = 0{\cdot}05$$

$$\lambda_2 = \frac{\lambda_1 - \frac{1}{6}}{2} = 0{\cdot}39215 \quad \text{for} \quad \theta = 0{\cdot}05.$$

From (B.5)
$$\frac{\partial z}{\partial \bar{m}} = -\frac{1}{2\bar{m}} \qquad \text{(B.6)}$$

From (B.4) and (B.3)
$$\frac{\partial z}{\partial \bar{m}} = \frac{-y\left(1 - \frac{\lambda_1}{A}\right)^{-\frac{3}{2}}}{4K(\bar{m} + K)^3\sqrt{N}} + \frac{\lambda_2}{N\bar{m}^2} \qquad \text{(B.7)}$$

So starting with an approximation to $\bar{m}$ say $\bar{m}_{(1)}$, it is substituted in (B.3), and z in (B.4) and $\bar{m}_{(2)}$ (say) in (B.5) are calculated. Then

$$\frac{\bar{m} - \bar{m}_{(2)}}{\bar{m} - \bar{m}_{(1)}} \doteqdot \frac{\dfrac{\partial z}{\partial \bar{m}_{(1)}}}{\dfrac{\partial z}{\partial \bar{m}_{(2)}}} \qquad \text{(B.8)}$$

In the cases worked out numerically, the ratio in (B.8) was easily estimated and was often c. $\frac{1}{10}$.

When a series of m's are known for a particular ξ, the calculation for a neighbouring value of ξ may be greatly simplified when N is moderate or large.

For if m_1 corresponds to p_1 and z_1, and m_2 to p_2 or z_2, from (B.5)

$$\frac{m_2}{m_1} \doteqdot \frac{p_2}{p_1} e^{-2(z_2 - z_1)} \qquad \text{(B.9)}$$

and if $p_2 - p_1$ is small

$z_2 - z_1 \doteqdot (m_2 - m_1)\dfrac{\partial z}{\partial m_1}$ which, from (B.7), is very small when m or N are not small, so that:

$$\frac{m_2}{m_1} \doteqdot \frac{p_2}{p_1} \qquad \text{(B.10)}$$

This is most accurate, of course, for large N, but once $m_1(60)$ and $m_2(60)$ had been obtained, the following approximation was still better, viz.:

$$\frac{m_2(N_2)}{m_1(N_2)} \doteqdot \frac{m_2(N_1)}{m_1(N_1)} \qquad \text{(B.11)}$$

C.—*An expansion for the Incomplete Beta function in terms of Incomplete Gamma functions*

We require both percentage points and percentages when $2K$ is not a whole number. Interpolation is not easy when K is small, in either Pearson's or Thompson's tables. The expansion which will now be derived is very convenient in the range we need, and apparently is not in any of the published work on the function.

We write
$$I_\xi(K,r) = \frac{\int_0^\xi t^\epsilon(1-t)^{r-1}dt}{\beta(K,r)} \quad \text{where} \quad \epsilon = K - 1$$

$$= \frac{\Gamma(K+r)}{\Gamma(K)\Gamma(r)} I \qquad \text{(C.1)}$$

Put $1 - t = e^{-u}$. Then

$$I = \int_0^v (1 - e^{-u})^\epsilon e^{-ur} du \qquad \text{(C.2)}$$

where
$$v = -\log(1 - \xi) \qquad \text{(C.3)}$$

r is to be large and ϵ moderate or small. So we write

$$I = \int_0^v (2\sinh \tfrac{1}{2}u)^\epsilon e^{-(r + \frac{1}{2}\epsilon)u} du \qquad \text{(C.4)}$$

$(2 \sinh \frac{1}{2}u)^\epsilon$ converges rapidly as a power series in u. We obtain a Borel expansion by integrating (C.4) term by term in descending powers of $(r + \frac{1}{2}\epsilon)^2$. The series is

$$\frac{(2\sinh\frac{1}{2}u)^\epsilon}{u^\epsilon} = 1 + \frac{\epsilon u^2}{24} + \frac{u^4}{5760}(5\epsilon^2 - 2\epsilon) + \frac{u^6}{2903040}(35\epsilon^3 - 42\epsilon^2 + 16\epsilon) \quad . \quad . \quad \text{(C.5)}$$

Since

$$\int_0^v u^\alpha e^{-xu} du = \frac{1}{x^{1+\alpha}}\Gamma_{xv}(\alpha + 1) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C.6)}$$

$$I = \frac{1}{(r+\frac{1}{2}\epsilon)^{1+\epsilon}}\Gamma_{v(r+\frac{1}{2}\epsilon)}(1 + \epsilon) + \frac{\epsilon}{24(r+\frac{1}{2}\epsilon)^{3+\epsilon}}\Gamma_{v(r+\frac{1}{2}\epsilon)}(3 + \epsilon) \quad . \quad \text{(C.7)}$$

We can write this as a series of incomplete Γ function ratios. Put $P = r + \frac{1}{2}\epsilon$ then

$$I_\xi(1 + \epsilon, P - \tfrac{1}{2}\epsilon) = \frac{\Gamma(P + 1 + \frac{1}{2}\epsilon)}{P^{1+\epsilon}\Gamma(P - \frac{1}{2}\epsilon)}\left\{\frac{\Gamma_{Pv}(1+\epsilon)}{\Gamma(1+\epsilon)} + \frac{\epsilon(1+\epsilon)(2+\epsilon)}{24P^2}\frac{\Gamma_{Pv}(3+\epsilon)}{\Gamma(3+\epsilon)}\right.$$

$$\left. + \frac{(5\epsilon^2 - 2\epsilon)(1+\epsilon)(2+\epsilon)(3+\epsilon)(4+\epsilon)}{5760P^4}\frac{\Gamma_{Pv}(5+\epsilon)}{\Gamma(5+\epsilon)} + \ldots\right\}. \quad . \quad \text{(C.8)}$$

This converges rapidly when ϵ^3 is not large compared with P^2 or when ξ is very small, since the incomplete Γ function ratios then converge rapidly. Pearson's tables (1922) give values of $\frac{\Gamma_x(1+\epsilon)}{\Gamma(1+\epsilon)}$ for tabulated values of ϵ and $\frac{x}{\sqrt{\epsilon}}$ or the ratio in terms of ϵ and x can be read directly from Campbell's (1923) chart.

When ϵ and P are known, the first term by itself gives a good approximation to one of ξ or I when the other is known. It is easy to find an unknown P also, for we can obtain an even simpler approximation by expanding the factor outside the brackets, which is almost 1, in (C.8)

Using Stirling's expansion

$$\log \Gamma(x) = \tfrac{1}{2}\log 2\pi + (x - \tfrac{1}{2})\log x - x + \frac{1}{12x} - \frac{1}{360x^3} + \quad . \quad . \quad . \quad . \quad \text{(C.9)}$$

$$\log\Gamma(P + \tfrac{1}{2}\epsilon + 1) - (1+\epsilon)\log P - \log(P - \tfrac{1}{2}\epsilon) = \log\frac{P+\frac{1}{2}\epsilon}{P^{1+\epsilon}}\frac{\Gamma(P+\frac{1}{2}\epsilon)}{\Gamma(P-\frac{1}{2}\epsilon)}$$

$$= P\left\{\log\left(1 + \frac{\epsilon}{2P}\right) - \log\left(1 - \frac{\epsilon}{2P}\right)\right\} + (\tfrac{1}{2}\epsilon + \tfrac{1}{2})\log\left(1 - \frac{\epsilon^2}{4P^2}\right) - \epsilon$$

$$- \frac{\epsilon}{12P^2}\left\{1 - \frac{\epsilon^2}{4P^2}\right\}^{-1} + \frac{\epsilon}{120P^4}\left\{1 - \frac{\epsilon^2}{4P^2}\right\}^{-3} + 0(P^{-6}) \quad . \quad . \quad \text{(C.10)}$$

The right-hand side of (C.10) simplifies to

$$\frac{K - K^3}{24P^2}\left\{1 + \frac{3K^2 - 7}{40P^2} + 0(P^{-4})\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C.11)}$$

Substituting in (C.8)

$$I_\xi(K,r) = \frac{\Gamma_{Pv}(K)}{\Gamma(K)} + \frac{K - K^3}{24P^2}\left\{\frac{\Gamma_{Pv}(K)}{\Gamma(K)} - \frac{\Gamma_{Pv}(K+2)}{\Gamma(K+2)}\right\} + \text{terms * in } P^{-4}, P^{-6} \quad . \quad . \quad \text{(C.12)}$$

$$(P = r + \tfrac{1}{2}K - \tfrac{1}{2}, \ v = -\log_e(1 - \xi))$$

Thus from the first term of (C.12) we easily obtain either I, ξ or r if the other two and K are known.

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* The next term is

$$\frac{K^3 - K}{5760P^4}\left\{(K-3)(K-2)(5K+7)\frac{\Gamma_{Pv}(K)}{\Gamma(K)} - 10(K^3 - K)\frac{\Gamma_{Pv}(K+2)}{\Gamma(K+2)} + (K+2)(K+3)(5K-7)\frac{\Gamma_{Pv}(K+4)}{\Gamma(K+4)}\right\}.$$

Summary

From a batch of reels of enamelled wire, a standard length in a sample of the reels is tested for pinholes. The rule for accepting or rejecting the batch depended on the maximum in the sample: by assuming that the distribution of numbers of pinholes is binomial with a negative index that does not vary much from batch to batch, we deduce a more reliable criterion for accepting the batch on results from as small a sample as possible. An approximate formula for the quality of the batch in terms of statistical parameters is also obtained, and the relation of the new to the old test is discussed.

In the appendix are some results that can be applied generally:

A. The method, with examples, which was found most convenient for fitting the negative binomial to observed distributions by solving the maximum likelihood equations.

B. An adaptation of Cochran's method for solving the equation in the Incomplete Beta function :

$I_\xi\,(Nm,\, Nk) = \theta$ when θ is a small fraction and m is the unknown.

C. A good approximation to the Incomplete Beta function in terms of Incomplete Gamma functions that may be new.

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A Table of Lagrangian Coefficients for Logarithmic Interpolation of Standard Statistical Tables to Obtain other Probability Levels

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In statistical analysis it is sometimes desirable to obtain the values of statistical functions for probability levels other than those given in the standard tables. This problem has been discussed by Simaika (1942), who tested several scales of interpolation. His results show that quadratic interpolation on the logarithmic scale gave reasonably accurate results, although more accurate results could usually be obtained by other methods. These other methods, however, are less direct and usually involve heavier computation than the logarithmic interpolation. Furthermore,

Table of Lagrangian Coefficients for Logarithmic Interpolation

(Column headings are the probability levels of tabular values and row headings are probability levels of the interpolate)

x	100	50	25	x	250	100	50	x	250	100	50	x	500	250	100
	−	+	+		−	+	+		+	+	−		−	+	+
35	125	735	390	70	81	674	407	110	51	1,019	70	175	179	925	254
36	124	775	349	71	81	695	386	112	62	1,020	82	180	173	945	228
37	123	811	312	72	81	715	366	114	73	1,019	92	185	166	963	203
38	119	843	276	73	81	734	347	116	85	1,017	102	190	158	978	180
39	116	872	243	74	80	751	329	118	96	1,015	111	195	149	990	159
40	109	896	213	75	79	769	310	120	108	1,012	120	200	139	1,000	139
41	102	918	184	76	78	785	293	122	120	1,009	129	205	128	1,008	120
42	94	937	157	77	77	801	276	124	132	1,005	137	210	116	1,013	103
43	85	953	132	78	75	816	259	126	145	998	143	215	103	1,016	87
44	75	966	109	79	73	830	243	128	157	992	149	220	90	1,019	71
45	64	977	87	80	71	843	228	130	170	984	154	225	76	1,019	57
46	53	986	67	81	69	856	213	132	183	976	159	230	62	1,018	44
47	41	992	49	82	66	868	198	134	196	968	164	235	47	1,015	32
48	28	997	31	83	64	880	184	136	209	959	168	240	32	1,012	20
49	14	999	15	84	61	891	170	138	222	950	172	245	16	1,007	9
	+	+	−										+	+	−
50	0	1,000	0	85	58	901	157	140	235	940	175	250	0	1,000	0
51	15	999	14	86	55	911	144	142	248	930	178	255	17	992	9
52	30	997	27	87	52	920	132	144	261	920	181	260	33	984	17
53	46	993	39	88	49	929	120	146	274	909	183	265	51	974	25
54	62	987	49	89	46	938	108	148	288	897	185	270	68	964	32
55	78	981	59	90	42	945	97	150	302	884	186	275	87	952	39
56	95	973	68	91	38	953	85	152	315	872	187	280	105	940	45
57	112	964	76	92	34	959	75	154	329	859	188	285	123	927	50
58	130	954	84	93	30	966	64	156	343	845	188	290	142	913	55
59	148	943	91	94	26	972	54	158	357	831	188	295	161	898	59
60	166	931	97	95	22	978	44	160	371	817	188	300	180	883	63
61	184	918	102	96	18	983	35	162	384	803	187	305	199	868	67
62	203	904	107	97	14	988	26	164	398	789	187	310	218	852	70
63	222	889	111	98	9	992	17	166	412	774	186	315	238	835	73
64	241	873	114	99	5	997	8	168	426	759	185	320	257	818	75
					+	+	−								
65	261	857	118	100	0	1,000	0	170	440	743	183	325	277	800	77
66	280	840	120	101	5	1,003	8	172	454	727	181	330	297	781	78
67	300	822	122	102	10	1,006	16	174	468	712	180	335	317	762	79
68	320	803	123	103	14	1,009	23	176	482	696	178	340	337	743	80
69	340	784	124	104	20	1,011	31	178	496	679	175	345	357	723	80
70	361	764	125	105	25	1,013	38	180	511	662	173	350	378	703	81
71	381	744	125	106	30	1,015	45	182	525	645	170	355	398	683	81
72	401	723	124	107	35	1,017	52	184	539	628	167	360	419	662	81
73	422	702	124	108	40	1,018	58	186	553	611	164	365	440	641	81
74	443	680	123	109	45	1,019	64	188	567	594	161	370	460	620	80
75	464	657	121	110	51	1,019	70	190	581	577	158	375	480	599	79

Note.—The decimal point has been omitted from the table for convenience in printing. After the actual process of multiplication and addition has been carried out, the result is to be divided by 1,000 to give the interpolate.

if a set of coefficients be prepared for logarithmic interpolation in the range 1 to 10 these same coefficients can be used for any corresponding range such as 0·1 to 1·0, etc. This makes the logarithmic method of greater utility than other methods.

The table given constitutes such a set of coefficients for use with three standard levels of probability. The standard levels chosen are 5, 2·5, 1. These levels were chosen, since they and submultiples of them are coming into general use either wholly (Thompson 1941) or in part (Fisher and Yates, 1938) as standard levels. The decimal point has for convenience been omitted from the table. This was necessary in the headings, since its presence there would lead to confusion—for instance, a column headed 0·025 would equally well apply to 0·25 and to 0·0025. The decimal is to be inserted according to the level required; thus to obtain the coefficients for the case P (0·02) look up 200 in the table and use tabulated values for P (0·05, 0·025, 0·01), and so on. The sizes of the coefficients are indicated at the head of the column or part column, and italics are used for negative coefficients as a further reminder to the user. If intermediate values are required, linear interpolation of this table will be found satisfactory. Since the coefficients must in each case total unity, it will be seen that omission of decimal points simply means that results of calculations are to be divided by 1,000 to bring the value obtained for the function to the correct order. By arranging the three levels used so that the required level is as near as possible to the centre of the range the error is reduced to a minimum. It will be seen that for certain values of x coefficients are given for more than one set of standard levels. These alternatives occur at the end of chosen ranges, and either may be used to suit the user's convenience. Since the aim has been to retain simplicity and speed with reasonable accuracy, rather than to obtain high accuracy at any cost, the coefficients have been given correct to three figures. That three-figure accuracy is satisfactory will be seen from the following table, where errors arising when interpolating in the scale of the normal deviate using the tabulated coefficients are compared with the corresponding errors when coefficients correct to five figures are used.

Probability level	0·015	0·020	0·035	0·15
Tabled value of normal deviate	2·17009	2·05735	1·81191	1·03643
Error of interpolate (*a*)	−0·00073	+0·00070	−0·00083	−0·00570
Error of interpolate (*b*)	−0·00079	+0·00094	−0·00074	−0·00579

(*a*) Interpolation with coefficients correct to five figures.
(*b*) Interpolation with coefficients correct to three figures.

Two examples of their use are given, the examples being taken so that the results can be compared with values given in existing tables.

Example 1. Find the value of χ^2 for 11 degrees of freedom with $P = 0·02$.

Referring to the table, we find the coefficients for 2 given under the row heading 200, and column headings 500, 250, 100. The levels required in the standard tables are therefore 0·05, 0·025 and 0·01. The values of χ^2 for these levels and 11 degrees of freedom are 19·675, 21·920, 24·725 respectively (Thompson, 1941).

The coefficients are −139, +1,000, +139.

The calculation is therefore $-139 \times 19·675 + 1{,}000 \times 21·920 + 139 \times 24·726$
$= 22{,}622$

i.e., $\chi^2_{\cdot} = 22·622$ for $P = 0·02$

True value $= 22·618$ (Fisher and Yates, 1938)

The value obtained by ordinary linear interpolation is 22·855.

Example 2. Find the value of t for 15 degrees of freedom with $P = 0·3$.

The required coefficients are given under the row heading 300 and column headings 500, 250, 100. The levels required in the standard tables are therefore 0·50, 0·25, 0·10. The values of t for these levels and 15 degrees of freedom are 0·691, 1·197, 1·753 respectively (Merrington 1942).

The coefficients are +180, +883, −63
The calculation is $180 \times 0{\cdot}691 + 883 \times 1{\cdot}197 - 63 \times 1{\cdot}753$
$= 1071$
$t = 1{\cdot}071$ when $P = 0{\cdot}3$
True value $= 1{\cdot}074$ (Fisher and Yates, 1938)
The value obtained by ordinary linear interpolation is 1·096.

INVERSE INTERPOLATION

Although the table was designed for use in direct interpolation, a method has been devised for its use in inverse interpolation. The method consists of obtaining an approximate result, say by inspection, followed by successive approximations. Usually two approximations will suffice.

Derivation

Suppose a function is known to have a value " h," and the probability of this value is required.

Denote by a, b and c the values given by the standard tables. These values will be those nearest to the above value " h." Since, however, P is not known, the coefficients cannot be obtained from the table. Denote the required coefficients by l, m, n.

[Then] $$la + mb + nc = h$$
[but] $$l + m + n = 1$$

(In these equations it is assumed that the decimal point has been inserted.)

$$\therefore lb + mb + nb = b$$

Eliminating the second term gives

$$l(b - a) - n(c - b) = b - h$$

i.e., $Al - Cn = H$. Where A, B, H are the differences from the middle standard value.

This equation can then be used to obtain the required probability by successive approximations. A rough value can be obtained by inspection; the coefficients for this value being obtained from the table, a new value of H is calculated, say H_1. A first approximation to P is then estimated, using linear interpolation with the known value H, H_1 and H_2, where H_2 is the value of H at one of the standard levels. Using this value of P to obtain coefficients from the table, the process is repeated. It will usually be found unnecessary to proceed to a third approximation.

In practice it will be found that the procedure can be carried out very rapidly.

Example. Find the significance of $\chi^2 = 33{\cdot}2$ for 20 degrees of freedom.

From the standard tables the following values are obtained:

P	0·05	0·025	0·01
χ^2	31·4104	34·1696	37·5662

Taking differences from 34·1696 gives

$$A = 2{\cdot}7592 \quad C = 3{\cdot}3966 \quad H = 0{\cdot}9696$$

From inspection the expected value is approximately 0·033.
From the table for $P = 0{\cdot}033$ and inserting decimal point

$$l = 0{\cdot}297 \quad n = -0{\cdot}078 \quad \therefore H_1 = 1{\cdot}0844.$$

Also when $P = 0{\cdot}05$, $l = 1{\cdot}0$, $n = 0{\cdot}0$ and $H = 2{\cdot}7592$.

For first approximation

$$P = 0{\cdot}033 + \frac{0{\cdot}9696 - 1{\cdot}0844}{2{\cdot}7592 - 1{\cdot}0844} \times (0{\cdot}05 - 0{\cdot}033) \quad \text{using linear interpolation}$$

$$= 0{\cdot}033 - \frac{0{\cdot}1148}{1{\cdot}6748} \times (0{\cdot}017)$$

$$= 0{\cdot}033 - 0{\cdot}0012 = 0{\cdot}0318$$

When $P = 0{\cdot}0318$, $l = 0{\cdot}249$, $n = -0{\cdot}074$, $H_2 = 0{\cdot}9384$.

For second approximation

$$P = 0{\cdot}0318 - \frac{0{\cdot}9384 - 0{\cdot}9696}{0{\cdot}9384 - 1{\cdot}0844} \times (0{\cdot}0318 - 0{\cdot}033) \quad \text{using linear interpolation.}$$

$$= 0{\cdot}0318 - \frac{0{\cdot}0312}{0{\cdot}1460}(\times\ 0{\cdot}0012)$$

$$= 0{\cdot}0318 - 0{\cdot}00026 = 0{\cdot}0321 \text{ rounding off to three figures.}$$

A third approximation should be unnecessary but is included for demonstration.

When $P = 0{\cdot}0321$, $l = 0{\cdot}261$, $n = -0{\cdot}075$, $H_3 = 0{\cdot}9749$

and for third approximation $P = 0{\cdot}0321 + \dfrac{0{\cdot}9696 - 0{\cdot}9749}{1{\cdot}0263 - 0{\cdot}9749} \times (0{\cdot}0318 - 0{\cdot}0321)$

$$= 0{\cdot}0321 + \frac{0{\cdot}0053}{0{\cdot}0514} \times 0{\cdot}0003$$

$$= 0{\cdot}0321 + 0{\cdot}00003 = 0{\cdot}0321 \text{ rounding off to three figures.}$$

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Linear Sequential Rectifying Inspection for Controlling Fraction Defective

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Introduction

Wald and Barnard (Refs. 5 and 1) have considered the sequential inspection of a batch of items to test whether the proportion of defectives is allowable. The batch is assumed to be large, and a relatively small number of items are inspected. After inspection the batch is either accepted or rejected. Any batch that is accepted is of the same quality after inspection as before—the effect of the inspection is simply to weed out batches of bad quality. This non-rectifying kind of inspection could appropriately be applied, for example, by a producer when purchasing batches of components from a subcontractor.

But the inspection of a batch of items may serve a quite different purpose, provided that the test is not destructive. The maker of the goods may wish to assure himself, by a final inspection, that the fraction defective in the batch when it leaves him will be low. If when he starts the inspection he finds that the fraction defective is too high he will continue the inspection until a considerable part or all of the batch has been examined, the defective items found being removed, rectified, or replaced by good ones. The quality of the batch may thus be considerably improved by the inspection. The batch is never rejected.

Such inspection may be termed rectifying inspection. Pioneer work has been done in this field by Dodge and Romig (Ref. 3). The present paper (which results from the author's wartime interest in inspection problems while a member of the Ministry of Supply Advisory Service on Statistical Method, S.R. 17) gives an adaptation of Wald's sequential methods to rectifying inspection, and formulæ from which a set of tables could be computed.

Dodge–Romig Inspection

The inspection procedure of Dodge and Romig is as follows. An initial sampling inspection is performed, which may be by either single or double sampling. (In single sampling a sample of n items is examined, and the quality is regarded as satisfactory if the number of defectives does not exceed c, n and c being given integers. In double sampling, a first sample of size n_1 is taken, and the quality is regarded as satisfactory if the number of defectives does not exceed c_1 and as unsatisfactory if it exceeds c_2 $(>c_1)$. If the number of defectives found lies between $c_1 + 1$ and c_2 inclusive, a second sample of size n_2 is examined and the quality is regarded as satisfactory if the total number of defectives found does not exceed c_2.) If this sampling indicates that the quality of the batch is satisfactory, the batch is passed as correct. If not, the batch is inspected 100 per cent. and all defective items found are rectified or replaced.

In deciding on the constants of the scheme two quantities are taken account of, the first the process average fraction defective and the second either the "average outgoing quality limit ($AOQL$)" or the "lot tolerance". The process average fraction defective is the average quality of a long run of production before it is inspected. The $AOQL$ is the average quality of batches after inspection for the most unfavourable incoming quality, *i.e.*, the maximum average fraction defective of outgoing material, whatever the incoming quality. The lot tolerance is that outgoing quality of a batch which, whatever the incoming quality, there is at most only a small (stated) chance of permitting. The Dodge–Romig sampling inspection tables are so constructed that batches whose incoming quality is at the process average will be inspected with minimum average amount of inspection (taking into account the possibility of occasional 100 per cent. inspection) consistent with the guarantee of a given $AOQL$ or lot tolerance.*

The lot tolerance is similar to what Barnard has called the "risk point" of a non-rectifying scheme. In practice it is necessary, or at least convenient, to express the guarantee to be given by an inspection scheme as a single simple condition, and for a rectifying scheme when we are con-

* This at least was the intention, but it may not have been exactly fulfilled, since approximations were made in the solution.

centrating attention on the batches individually the lot tolerance is the obvious condition to choose. Dodge and Romig state, in fact, that their tables based on the lot tolerance " have been found particularly useful in inspections made by the ultimate consumer or his purchasing agent for lots or shipments purchased more or less intermittently " (Ref. 3, p. 38).

The concept of *AOQL* was introduced to cater for the inspection of a long series of batches when it is important to control the average quality of output and not the quality of individual batches. It " has been found particularly helpful, for example, in consumer inspections of continuing purchases of large quantities of a product, and in manufacturing process inspections of parts where the inspection lots tend to lose their identity by merger in a common storeroom from which quantities are withdrawn on order as needed". However, since Dodge and Romig constructed their tables other schemes of rectifying inspection have been suggested (Refs. 2 and 6), in which the output is inspected continuously and not in batches of definite size. Such schemes would normally be more efficient, and we may surmise that they will largely replace batch-by-batch inspection as provided for in the Dodge–Romig *AOQL* tables. In this paper we shall be concerned with inspection of batches, and so the lot tolerance is the form of quality guarantee that will be considered, rather than the *AOQL*.

In practice the initial sampling inspection in a Dodge–Romig scheme is sometimes carried out by a different staff from the 100 per cent. inspection, and it may be more important to minimize the amount of sampling inspection than the total amount of inspection. In such cases the sampling inspection is not primarily rectifying, and should be considered by itself as non-rectifying. But if all the inspection is carried out by the same staff, then the principle of minimizing the average total amount of inspection may well be a sound one to adopt, and as an approximation we can if we like choose a scheme which minimizes the average amount of inspection when the incoming quality is at the process average.

Now that sequential methods have been developed, it is clear that the Dodge–Romig procedure can be made more efficient, as to the number of items inspected, by the use of a sequential scheme for the initial sampling inspection instead of single or double sampling, unless sequential sampling is impracticable.* But if all the inspection is done by the same staff there is no need for the scheme to consist of a sequential inspection followed by a possible distinct 100 per cent. inspection. Both parts can be combined into a sampling scheme that may include a large part or the whole of the batch. We have this state of affairs, in fact, if we use a Wald sequential scheme in which (in Barnard's notation) the acceptance-handicap H_1 is small but the rejection-handicap H_2 is so large that the decision to inspect 100 per cent. is never reached, the whole batch having been inspected first anyway during the sequential sampling—in other words, a scheme with a linear acceptance boundary, as in a Wald scheme, but no rejection boundary.

We are thus led to consider the inspection procedure defined below. The mathematical treatment will concentrate on the limiting expressions when the fractions defective are very small, and will lead to the formulæ needed for computing a set of tables similar to the Dodge–Romig tables. If quality is expressed in terms of the actual number of defectives in the batch instead of the fraction defective, schemes can be set out in double-entry tables of adequate accuracy for fairly low fractions defective (such as are normally acceptable), and the space saved in this way, as compared with the triple-entry Dodge–Romig tables, can be devoted to information on average sample size. It is hoped that it will be possible for such tables to be published shortly.

Dodge and Romig state that they have investigated multiple sampling methods, and consider that anything more elaborate than double sampling does not justify its complexity. In defence of the procedure treated here it may be remarked that

(1) Dodge and Romig were presumably unaware of it,

(2) it is specified by two constants only, H and b (or α and β), while for example Dodge–Romig double sampling needs four constants, two sample sizes, n_1 and n_2, and two defect-numbers, c_1 and c_2,

(3) whether the occasions when multiple sampling is physically convenient are frequent or rare, it is desirable that multiple sampling methods should be explored and made available.

* It is supposed in sequential sampling that the items are inspected one by one. With certain particular schemes there is only a slight loss in efficiency if the items are tested in sets of size $(b + 1)$ together, where b is the " penalty ", in Barnard's notation. If this is too small a number to be tested conveniently together, then a cruder method such as double sampling must be used.

A RECTIFYING SEQUENTIAL INSPECTION SCHEME

We suppose that we are given a batch of N items, of which an unknown number Y are defective, and the batch is sampled at random item by item. It is convenient to represent the course of the sampling by a path on an "inspection diagram" (Ref. 1), the point (x, y) being reached when $x + y$ items have been sampled, x of them found to be good, and y defective. If the whole batch is sampled, the path must lead to the point $(N - Y, Y)$. We consider the following

Sampling rule. Sampling will proceed, and defective items found will be replaced by good ones, until either the whole batch has been tested or the path reaches one of the *boundary points*:

$$(H, 0), (H + b, 1), (H + 2b, 2), \ldots (H + \mu b, \mu),$$

where H and b are given integers, and μ is the greatest integer less than $\dfrac{N - H}{b + 1}$. (See Fig. 1.)

We can put this alternatively by saying that an initial sample of size H is taken, and thereafter further samples of size $b + 1$, and sampling is discontinued if there are no defectives in the first sample, or one only in the first two samples, or two only in the first three samples, etc. If a score is kept by adding 1 for a good item and subtracting b for a defective, the score being initially zero, the inspection ceases if the score reaches H.

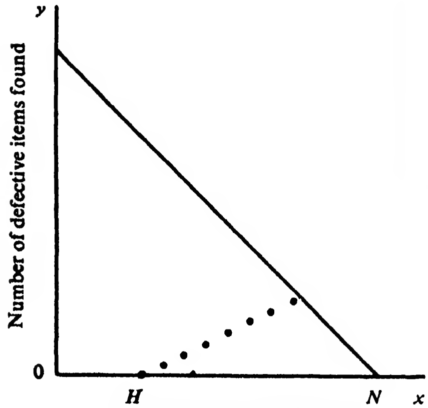

FIG. 1.

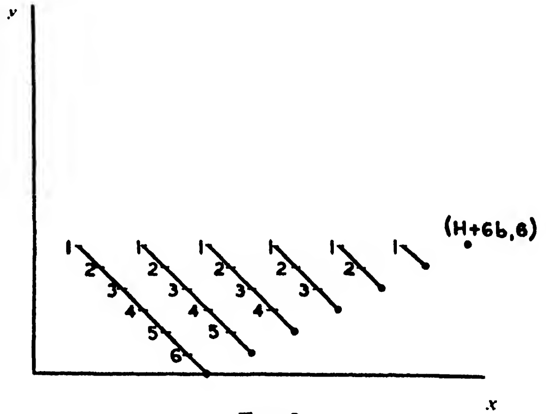

FIG. 2.

Given N, the problem is to decide on H and b so that the following conditions are satisfied:—

(1) Whatever value Y may have, there is at most a risk ε that after inspection the batch will contain Y' or more defectives (Y' given).

(2) For some range of values of Y, perhaps simply when Y has a given value Y_0, the average number of items tested is as small as possible.

PRINCIPAL FORMULÆ

Theorem 1. The chance of reaching the point (x, y) on the inspection diagram is equal to the number of possible paths from $(0, 0)$ to (x, y) that do not include a boundary point (other than (x, y) itself if that should be a boundary point) multiplied by

$$\frac{Y!\,(N - \overline{x + y})!\,(N - Y)!}{(Y - y)!\,N!\,(N - Y - x)!}$$

Here we must have $y \leq Y$, $x \leq N - Y$, since otherwise (x, y) is inaccessible.

Proof. The chance of reaching (x, y) by any one path is

$$\frac{Y(Y-1)(Y-2)\ldots(Y-y+1)(N-Y)(N-Y-1)\ldots(N-Y-x+1)}{N(N-1)(N-2)\ldots(N-y-x+1)}$$

Altering the order of the defectives (*i.e.*, the order in which they occur in the series of $x + y$ observations) merely alters the order of the factors in the numerator.

Theorem 2. The numbers of paths to the boundary points at which $y = 0, 1, 2, \ldots \mu$, are respectively

$$1, H, \frac{1}{2!}H(H + 2 . \overline{b+1} - 1), \frac{1}{3!}H(H + 3 . \overline{b+1} - 1)(H + 3 . b + 1 - 2), \ldots$$

the general term for the boundary point $(H + yb, y)$ being

$$\frac{H}{H + y(b+1)} \, {}^{H+y(b+1)}C_y.$$

Proof. The number of paths is easily verified for $y \leq 3$. We consider now $y \geq 1$. For a given value of y, say y_0, we consider points on the inspection diagram for which $x + y = H +$ a multiple of $b + 1$ and $0 \leq \quad < y_0$. In Fig. 2 $y_0 = 6$. The points on each constant-sample-size diagonal are numbered from the top, those on the first diagonal (through $(H, 0)$) being numbered $1, 2, \ldots y_0$, those on the second $1, 2, \ldots y_0 - 1$, etc., stopping short of the boundary point each time.

Following the method of Stockman and Armitage (Ref. 4), we write down the number of paths from (0, 0) to the points on the first diagonal, and the matrices of numbers of paths from the ith point on one diagonal to the jth point on the next, and multiply all together to obtain the number of paths from (0, 0) to $(H + y_0 b, y_0)$. We get, writing now y instead of y_0,

$$({}^HC_y, {}^HC_{y-1}, \ldots {}^HC_1). \begin{pmatrix} 1, & 0, & \ldots & 0 \\ b+1, & 1, & \ldots & 0 \\ {}^{b+1}C_2, & b+1 & \ldots & 0 \\ \ldots & \ldots & \ldots & \ldots \\ {}^{b+1}C_{y-1}, & {}^{b+1}C_{y-2}, & \ldots & b+1 \end{pmatrix} \begin{pmatrix} 1, & 0, & \ldots & 0 \\ b+1, & 1, & \ldots & 0 \\ \ldots & \ldots & \ldots & \ldots \\ {}^{b+1}C_{y-2}, & {}^{b+1}C_{y-3} & \ldots & b+1 \end{pmatrix} \cdots \begin{pmatrix} 1 \\ b+1 \end{pmatrix}$$

Sizes:— $1 \times y$ $\qquad y \times (y-1) \qquad (y-1) \times (y-2) \qquad \ldots \; 2 \times 1$

The product of all the matrices except the first is the following column:

$$\begin{pmatrix} {}^{y(b+1)}C_0 \\ {}^{y(b+1)}C_1 - (b+1) . {}^{y(b+1)-1}C_0 \\ {}^{y(b+1)}C_2 - (b+1) . {}^{y(b+1)-1}C_1 \\ \ldots\ldots\ldots\ldots \\ {}^{y(b+1)}C_{y-1} - (b+1) . {}^{y(b+1)-1}C_{y-2} \end{pmatrix}$$

This is easily proved by induction. We have to shew that (writing ξ for $b + 1$)

$${}^{\xi}C_r . {}^{\xi}C_0 + {}^{\xi}C_{r-1} . \{{}^{\nu\xi}C_1 - \xi . {}^{\nu\xi-1}C_0\} + \ldots + {}^{\xi}C_0 . \{{}^{\nu\xi}C_r - \xi . {}^{\nu\xi-1}C_{r-1}\} = {}^{(\nu+1)\xi}C_r - \xi . {}^{(\nu+1)\xi-1}C_{r-1}.$$

Now $LHS =$ coefficient of t^r in $(1 + t)^{\xi}(1 + t)^{\nu\xi} - \xi t(1 + t)^{\xi}(1 + t)^{\nu\xi-1} = RHS$.

The total number of paths is therefore

$${}^HC_y . {}^{y(b+1)}C_0 + {}^HC_{y-1} . \{{}^{y(b+1)}C_1 - (b+1) . {}^{y(b+1)-1}C_0\} + \ldots + {}^HC_0 . \{{}^{y(b+1)}C_y - (b+1) . {}^{y(b+1)-1}C_{y-1}\},$$

where we have added an extra zero term at the end. This expression is the coefficient of t^y in $(1 + t)^H(1 + t)^{y(b+1)} - (b + 1)t . (1 + t)^H(1 + t)^{y(b+1)-1}$,

i.e.,
$$\begin{aligned} & {}^{H+y(b+1)}C_y - (b+1) . {}^{H+y(b+1)-1}C_{y-1} \\ &= \frac{H}{H + y(b+1)} . {}^{H+y(b+1)}C_y. \quad \text{Q.E.D.} \end{aligned}$$

Combining theorems 1 and 2, we have the chance of reaching each of the boundary points. The most convenient form of the chance P_y of reaching the boundary point $(H + yb, y)$, for computing, is probably

$$\left.\begin{aligned} P_y &= \frac{H}{H + y(b+1)} \frac{{}^{H+y(b+1)}C_y . {}^{N-H-y(b+1)}C_{Y-y}}{{}^NC_Y} \quad \text{if } y \leq Y \text{ and } \leq \frac{1}{b}(N - H - Y) \\ P_y &= 0 \text{ otherwise.} \end{aligned}\right\}$$

Limit as $N \longrightarrow \infty$. From now on we consider the limiting case $N \longrightarrow \infty$, Y finite. H and b will be infinite also, but

$$\frac{H}{N} = \alpha, \quad \frac{b+1}{N} = \beta$$

will remain finite. μ is now the greatest integer less than $\frac{1-\alpha}{\beta}$. It has been found that with non-rectifying sequential schemes formulæ derived for the limiting case fraction defective $\longrightarrow 0$ are sufficiently accurate for practical purposes, provided that b is not less than 10 or so, and we shall expect that the analogous limiting case here considered will give formulæ for α and β equally useful.

Applying Stirling's formula to the expression in Theorem 1, and combining with Theorem 2, we get

Theorem 3. In the limit as $N \longrightarrow \infty$, the chances of reaching the boundary points $(\alpha N, 0)$, $(\overline{\alpha + \beta} \,.\, N, 1)$, $(\overline{\alpha + 2\beta} \,.\, N, 2)$, . . . $(\overline{\alpha + \mu\beta} \,.\, N, \mu)$, become $P_0, P_1, P_2, \ldots P_\mu$, where

$$P_0 = (1-\alpha)^Y$$

and for $y \geq 1$

$$\left.\begin{array}{l} P_y = {}^YC_y \,.\, \alpha(\alpha + y\beta)^{y-1} \,.\, (1 - \alpha - y\beta)^{Y-y} \quad \text{if } y \leq Y, \\ P_y = 0 \quad \text{if } y > Y. \end{array}\right\}$$

Note that the condition $x \leq N - Y$ becomes the trivial one $x \leq N$ as $N \longrightarrow \infty$ and Y remains finite. Thus if $\infty > Y \geq \mu$, all the boundary points are accessible, however large Y is. This is not true in general when N is finite.

The average sample size A, *i.e.*, the average number of items inspected, is given by the formula

$$\frac{A}{N} = \alpha + (P_1 + 2P_2 + \ldots + \mu P_\mu)\beta + (1 - \sum_{i=0}^{\mu} P_i)(1 - \alpha), \quad . \quad . \quad . \quad (1)$$

where, for a given scheme, α and β are constants and the P_i are functions of Y, the total number of defectives. For values of Y not exceeding μ this expression can be simplified, for we have then

$$\frac{A}{N} = \alpha\{1 + Y\beta(1 - \alpha - \beta)^{Y-1} + \ldots + r \,.\, {}^YC_r \,.\, \beta(\alpha + r\beta)^{r-1}(1 - \alpha - r\beta)^{Y-r} + \ldots + Y\beta(\alpha + Y\beta)^{Y-1}\}.$$

If the expression inside the curly brackets is expanded by the multinomial theorem and the terms collected, it will be found that all terms containing a positive power of α vanish, and the coefficient of β^n is

$${}^YC_n[n^n - {}^nC_1(n-1)^n + {}^nC_2(n-2)^n - \ldots + (-1)^{n-1} \,.\, {}^nC_{n-1}] = {}^YC_n \,.\, n!.$$

Thus for $Y \leq \mu$

$$\frac{A}{N} = \alpha\{1 + Y\beta + Y(Y-1)\beta^2 + Y(Y-1)(Y-2)\beta^3 + \ldots\} \quad . \quad . \quad (1')$$

If now we know the "process curve" of the incoming batches, *i.e.*, the relative frequencies with which different values of Y will be presented, we can obtain the actual average sample size that will be experienced, on suitable summation of the values of A calculated for individual values of Y.

The average outgoing quality (*AOQ*), *i.e.*, the average number of defectives in the batch after inspection, for any initial number of defectives Y, is

$$\left(\sum_{i=0}^{\mu} P_i\right) Y - (P_1 + 2P_2 + \ldots + \mu P_\mu),$$

which for $Y \leq \mu$ reduces to

$$Y\left\{1 - \text{the value of } \frac{A}{N} \text{ at } (Y-1)\right\}.$$

The *AOQL* is the maximum value of this as Y varies.

THE LOT TOLERANCE CONDITION

Given the "lot tolerance" Y' and the corresponding risk ε, we have to find schemes (*i.e.*, values of α and β) with this lot tolerance and with minimum average sample size for some specified value or values of Y. The following results are useful.

Theorem 4. With the notation of Theorem 3,

(1) the quantity $\sum_{i=0}^{y} P_i \; (0 \leq y \leq \mu)$ is equal to 1 for $Y \leq y$ and is a decreasing function of Y for $Y \geq y$;

(2) for any given integer $Y' \; (\geq 1)$, the sequence of quantities

$$\left.\begin{aligned} \theta_0 &= P_0 \text{ for } Y = Y', \\ \theta_1 &= P_0 + P_1 \text{ for } Y = Y' + 1, \\ \theta_2 &= P_0 + P_1 + P_2 \text{ for } Y = Y' + 2, \\ &\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot\;\cdot \\ \theta_\mu &= P_0 + P_1 + \ldots + P_\mu \text{ for } Y = Y' + \mu, \end{aligned}\right\}$$

has only one maximum, in the sense that we cannot have $\theta_r \geq \theta_s \leq \theta_t$ for integers, r, s, t satisfying $0 \leq r < s < t \leq \mu$. (We *can* have $\theta_i = \theta_{i+1}$.)

I have been unable to prove either of these statements, but proof is not strictly necessary, as they can always be verified in any particular instance.

Assuming the truth of the statements, it is clear that the lot tolerance condition is equivalent to saying that $\max_{0 \leq i \leq \mu} (\theta_i) \leq \varepsilon$, θ_i being defined as above, and that to make sure that this condition is satisfied (for any given α and β) it is merely necessary to calculate $\theta_0, \theta_1, \theta_2, \ldots$ in turn until a maximum is reached. To economize on sample size (this being an increasing function of both α and β) we shall choose α and β so that $\max. (\theta_i) = \varepsilon$, since θ_i is a decreasing function of both α and β (these statements are obvious intuitively if not algebraically). Thus a single infinity of schemes is defined. Among them there is a series of schemes in which the maximum is attained by two of the θ_i, namely the schemes for which respectively $\theta_0 = \theta_1 = \varepsilon$, $\theta_1 = \theta_2 = \varepsilon$, $\theta_2 = \theta_3 = \varepsilon$, &c. The scheme with $\theta_0 = \theta_1 = \varepsilon$ can be found directly, being

$$\left.\begin{aligned} \alpha &= 1 - \varepsilon^{1/Y'} \\ \beta &= \varepsilon^{1/Y'}\left\{1 - \left(\frac{1}{Y'+1}\right)^{1/Y'}\right\} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

As we pass to other schemes in the series α increases and β decreases.

Thus we see that the inspection procedure is defined by two quantities, α and β, and that the lot tolerance condition imposes a relation between them. For any α not less than that given in equations (2) there is a β such that $\max_{0 \leq i \leq \mu} \theta_i = \varepsilon$,* and the scheme so defined will yield average sample sizes for varying incoming defect numbers Y according to equations (1) and (1′), and an overall average sample size for a given process curve. If we consider the average sample size for a fixed value of Y or for a given process curve as a function of α, this function is continuous and consists of a series of smooth curves with corners wherever the order of the maximum θ changes, *i.e.*, corners where, as α increases, θ_2 first becomes greater than θ_1, θ_3 becomes greater than θ_2, &c. Some specimen calculations have suggested what seems *a priori* plausible that the minimum average sample size often but not invariably occurs at one of these corners. Since it is practicable to tabulate only a small selection of the infinitely many schemes with any given lot tolerance, we may reasonably tabulate only those schemes for which two consecutive θ's are equal to ε. It seems that usually one of these schemes will minimize the average sample size for a given process curve; if not, one of them will nearly do so. When the tolerance number Y' is large, however, the labour of finding the schemes (values of α and β) for which two consecutive θ's are equal becomes great, and it may be necessary to consider arbitrary equally spaced values of α.

* If α has the value given in (2), any value of β not less than that given in (2) will satisfy the condition $\max. \theta_i = \varepsilon$, but the value of β in (2) leads to the lowest sample sizes when $Y > 0$. For greater values of α the corresponding β is unique.

The following table is given as an example. It refers to $Y' = 10$, $\varepsilon = 0{\cdot}01$, and gives the first four schemes having two consecutive θ's equal (*i.e.*, $\theta_0 = \theta_1 = \varepsilon$, $\theta_1 = \theta_2 = \varepsilon$, $\theta_2 = \theta_3 = \varepsilon$, $\theta_3 = \theta_4 = \varepsilon$). The number of such schemes is unlimited; one would merely tabulate as many as seemed useful. Average sample sizes and *AOQL* are given for each scheme. Minimum average sample sizes for particular values of Y appear in heavy type.

Lot tolerance $Y' = 10$, *risk* $\varepsilon = 0{\cdot}01$

Scheme α	Scheme β	Average sample size (A/N) for Y equal to 0	1	2	3	4	5	6	8	10	12	*AOQL*	Corresponding Y
0·3690	0·1345	**0·369**	**0·419**	0·482	0·563	0·672	0·803	0·890	0·966	0·989	0·996	1·7	4
0·4010	0·0784	0·401	0·433	**0·469**	**0·511**	0·561	0·621	0·693	0·878	0·969	0·992	2·3	6
0·4354	0·0553	0·435	0·459	0·486	0·516	**0·550**	0·587	0·630	0·736	0·879	0·973	2·6	7
0·4679	0·0423	0·468	0·488	0·509	0·532	0·558	**0·586**	**0·617**	0·688	0·776	0·889	2·8	9

Schemes with a Curved Acceptance Boundary

No claim has been made for the inspection procedure considered above that it is in any sense the best possible, but merely that it involves the inspection of fewer items, on the average, than a Dodge–Romig scheme with the same quality guarantee, and it is specified very simply by two constants. More powerful schemes with a curved acceptance boundary would be more troublesome to specify (and bulkier to tabulate). But it may be noted that the above work suggests a particular curved-boundary procedure, defined by $\theta_0 = \theta_1 = \theta_2 = \ldots = \varepsilon$, where by θ_r is meant the chance that one of the first $r + 1$ boundary points should be reached when the total number Y of defectives initially is $Y' + r$. This procedure has in fact been studied by S. N. Collings (quite independently of the present investigation), and it is understood that a joint paper on it is being prepared by him and G. A. Barnard. The relation between the two procedures has not been fully explored, but it appears that a Collings scheme is in general appreciably more economical of sampling than a linear-boundary scheme with the same lot tolerance.

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On the Distribution of the Sum of n Sample Values drawn from a Truncated Normal Population

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Summary

This paper deals with the situation arising when a normal population has all members with values greater (or less) than a given value rejected, and where small samples are drawn from the remaining truncated population. Formal expressions are deduced from which can be calculated the distribution of the sum (or mean) of the sample values as a function of the point at which the original parent population is cut. Tables are given for the integral distribution function of the sum, standardised in an appropriate fashion, and their importance in solving a type of problem of quite frequent occurrence is shown.

1. *Introduction*

In the course of the author's work the problem arose with which this paper deals. The solution involved functions which—as far as could be discovered—had not previously been tabulated, and Dr. N. R. Campbell kindly undertook the laborious work involved in the calculation of a number of these functions. The tables which resulted are likely to prove useful in many allied problems, and as the matter does not seem to have been treated before—or, at any rate, has not been published—it has been thought worth while to place the work on record.

The problem in question is the following: A population of members exists of which the relevant variate is normal or Gaussian and of which the mean and variance are known. The members of small samples (each sample consisting of the same number of members) have to be used together in such a way that the sum or the mean of the values of the variate in the sample is less than a given number. The question is: at what value of the variate should the original population be cut in order that when members are taken from the truncated population, the desired result is achieved with an allowable probability of failure?

A simple example of such a case which might arise in practice is where the original population consists of a factory production of similar metal parts of which the distribution of the lengths (or of any other dimension) can be shown to be substantially Gaussian. Two of these parts have to be used together in such a way that the total length must be less than a given distance. What metal parts should be discarded from the production in order that apparatus containing the mounted components should have a reject percentage less than a certain amount? Alternatively, what part of the production should be discarded to achieve the minimum wastage in time and material when the cost of mounting the components is taken into account?

2. *The Analytical Solution*

Let the original population be

$$f_0(x) = \frac{1}{\sigma_0\sqrt{2\pi}} Exp\left[-\frac{(x-\bar{x})^2}{2\sigma_0^2}\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

that is, a Gaussian distribution with variable x, the mean of which is $\bar{x}$ and the standard deviation σ_0. There is no loss of generality in changing the units in the usual way so as to bring (1) into standard form

$$f(y) = \frac{1}{\sqrt{2\pi}} Exp\left(-\frac{y^2}{2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$y = \frac{x-\bar{x}}{\sigma_0}$$

so that the sum of the variates in a sample of n is

$$\sum_n y = \frac{\sum_n x - n\bar{x}}{\sigma_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Suppose that the distribution (2) is truncated at $y_0 = \frac{x_0 - \bar{x}}{\sigma_0}$ to produce the distribution

$$f_1(y) = \frac{1}{\sqrt{2\pi}} Exp\left(-\frac{y^2}{2}\right) \Big/ \int_{-\infty}^{y_0} \frac{1}{\sqrt{2\pi}} Exp\left(-\frac{y^2}{2}\right) dy \text{ for } y \leqslant y_0 \qquad (4)$$
$$= 0 \qquad \text{for } y > y_0$$

and let $f_n(y, y_0)$ represent the distribution of the sum of n random members of (4); f_n clearly vanishes for $y > ny_0$.

It is required that the sum of the n random members of (4) should be less than y_s (say) with a chance of failure F. We must therefore establish y_0 as a function of n, y_s and F. That is to say, we need the value of

$$F_n(y_0, y_s) = \int_{y_s}^{\infty} f_n(y, y_0) dy \qquad (5)$$

We may regard the sum of $(m + n)$ random members of (4) as obtained by adding the sum of a first group of m to that of a second group of n random members; thus *

$$f_{m+n}(y) = \int_{-\infty}^{+\infty} f_m(x) f_n(y - x) dx \qquad (6)$$

In the integrand here the first factor vanishes for $x > my_0$ and the second for $(y - x) > ny_0$; thus we may re-write (6) as

$$f_{m+n}(y) = \int_{y-ny_0}^{my_0} f_m(x) f_n(y - x) dx \qquad (7)$$

It is now necessary to compute the functions f_1, f_2, etc., from the relations (7) and obtain their integrals according to (5).

3. *Calculation of the Functions*

Certain relations are useful in order to present the tabulated functions in a convenient form. In the first place we require the mean, a_1, and the variance, σ_1^2, of the truncated population, f_1. If we write

$$g(y_0) = \frac{1}{2}\left[1 + Erf\left(\frac{y_0}{\sqrt{2}}\right)\right] \qquad (8)$$

where

$$Erf\, x = \frac{2}{\sqrt{\pi}} \int_0^x Exp(-y^2) dy \qquad (9)$$

we obtain for the mean of f_1

$$a_1 = \int_{-\infty}^{+\infty} y f_1(y) dy$$
$$= -\frac{1}{\sqrt{2\pi}} \cdot \frac{1}{g(y_0)} Exp\left(-\frac{y_0^2}{2}\right) \qquad (10)$$

and for its variance

$$\sigma_1^2 = \int_{-\infty}^{+\infty} (y - a_1)^2 f_1(y) dy = 1 + a_1 y_0 - a_1^2 \qquad (11)$$

Further it is a well-known result that the mean of f_n will be

$$a_n = na_1 \qquad (12)$$

and the standard deviation of f_n

$$\sigma_n = \sigma_1 \sqrt{n} \qquad (13)$$

For the actual computation of $f_2(y_0, y)$, (7) may be integrated giving

$$f_2(y, y_0) = \frac{1}{2\sqrt{\pi}} \cdot \frac{1}{g^2(y_0)} Exp\left(-\frac{y^2}{4}\right) Erf\left(y_0 - \frac{y}{2}\right) \qquad (14)$$

* For a formal proof see H. Cramer, "Random Variables and Probability Distributions." C.U.P., 1937, p. 36.

and this result was used to obtain the values of $f_2(y_0, y)$. For f_3, f_4, etc., the integrations cannot be carried out in the form of known functions, and numerical integration becomes necessary. f_2 could, of course, be obtained by numerical integration from f_1.

The values of

$$Exp\left(-\frac{y^2}{2}\right),\ Exp\left(-\frac{y^2}{4}\right),\ Erf\left(\frac{y}{\sqrt{2}}\right),\ Erf\left(y_0 - \frac{y}{2}\right)$$

appearing in (4), (8) and (14) were taken from the American Tables of Probability Functions * at intervals of 0·2 of y to 6 decimal places.

TABLE 1.

$y_0 = 0{\cdot}0.$

$a_1 = -0{\cdot}7979.\quad \sigma_1 = 0{\cdot}6028.$

n na_1 $\sigma_1\sqrt{n}$	1 −0·7979 0·6028	2 −1·5958 0·8525	4 −3·1915 1·2056	∞
$z = \dfrac{y - na_1}{\sigma_1\sqrt{n}}$	$F_1(z)$	$F_2(z)$	$F_4(z)$	$F_\infty(z)$
3·0				0·0013
2·9				0·0019
2·8			$F_4 = 0$ at	0·0026
2·7			$z = 2{\cdot}647$	0·0035
2·6				0·0047
2·5			0·0000	0·0062
2·4			0·0001	0·0082
2·3			0·0005	0·0107
2·2			0·0014	0·0139
2·1			0·0030	0·0179
		$F_2 = 0$ at		
2·0		$z = 1{\cdot}872$	0·0058	0·0228
1·9			0·0100	0·0287
1·8		0·0012	0·0160	0·0359
1·7		0·0068	0·0242	0·0446
1·6	$F_1 = 0$ at	0·0169	0·0349	0·0548
1·5	$z = 1{\cdot}324$	0·0314	0·0482	0·0668
1·4		0·0501	0·0644	0·0808
1·3	0·0114	0·0727	0·0837	0·0968
1·2	0·0594	0·0989	0·1060	0·1151
1·1	0·1072	0·1284	0·1313	0·1357
1·0	0·1547	0·1607	0·1595	0·1587
0·9	0·2016	0·1954	0·1904	0·1841
0·8	0·2477	0·2321	0·2237	0·2119
0·7	0·2930	0·2705	0·2591	0·2420
0·6	0·3373	0·3100	0·2963	0·2743
0·5	0·3804	0·3502	0·3348	0·3085
0·4	0·4223	0·3907	0·3743	0·3446
0·3	0·4628	0·4312	0·4143	0·3821
0·2	0·5018	0·4712	0·4545	0·4207
0·1	0·5392	0·5105	0·4944	0·4602
0·0	0·5751	0·5488	0·5337	0·5000
−0·1		0·5859	0·5721	0·5398
−0·2			0·6092	0·5793

y_0 is value at which original population is truncated.
a_1 is mean of truncated population.
σ_1 is standard deviation of truncated population.
$F_n(z)$ is the integral from z to ∞ of the frequency distribution of the sum of n sample values, z being the sum expressed as a standardised variable.

The calculation of g, a_1, σ_1^2, f_1, F_1, f_2 then requires only addition and multiplication. F_2 was derived from f_2 by numerical integration; for integration the formulae given in Interpolation

* Tables of Probability Functions, Vol. I, issued by the Federal Works Agency of New York.

and Allied Tables (Stationery Office), involving only odd-order differences were used. F_n for $n > 2$ involves two integrations, one of a product $f_n . f_{n-p}$ giving f_{2n-p}, and a second of f_{2n-p} giving F_{2n-p}. If $p = 0$, the product is symmetrical about a central value; the labour of calculating necessary products and of performing the first integration is then much less than if p has any other value. Accordingly f_4 was calculated from the formula

$$f_4(y_0, y) = \int_{y-2y_0}^{2y_0} f_2(u) f_2(y-u)\,du \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

and F_4 from this value of f_4. Since p cannot be 0 when $2n - p$ is odd, f_3 and F_3 were not calculated. In order to limit the labour, the values of y were limited to those for which F_n is not much greater than $\frac{1}{2}$; other values are not likely to be required in any problem of the kind considered.

TABLE 2.

$y_0 = 0{\cdot}5.$

$a_1 = -0{\cdot}5092. \quad \sigma_1 = 0{\cdot}6973.$

n na_1 $\sigma_1\sqrt{n}$	1 −0·5092 0·6973	2 −1·0183 0·9861	4 −2·0366 1·3945	∞
z	$F_1(z)$	$F_2(z)$	$F_4(z)$	$F_\infty(z)$
2·8			$F_4 = 0$ at	0·0026
2·7			$z = 2{\cdot}895$	0·0035
2·6				0·0047
2·5			0·0000	0·0062
2·4		$F_2 = 0$ at	0·0008	0·0082
2·3		$z = 2{\cdot}047$	0·0017	0·0107
2·2			0·0032	0·0139
2·1		0·0000	0·0056	0·0179
2·0		0·0003	0·0091	0·0228
1·9		0·0028	0·0140	0·0287
1·8	$F_1 = 0$ at	0·0082	0·0206	0·0359
1·7	$z = 1{\cdot}447$	0·0167	0·0292	0·0446
1·6		0·0282	0·0400	0·0548
1·5	0·0000	0·0430	0·0532	0·0668
1·4	0·0169	0·0610	0·0691	0·0808
1·3	0·0536	0·0822	0·0877	0·0968
1·2	0·0912	0·1064	0·1092	0·1151
1·1	0·1298	0·1335	0·1334	0·1357
1·0	0·1690	0·1634	0·1605	0·1587
0·9	0·2088	0·1956	0·1901	0·1841
0·8	0·2488	0·2300	0·2222	0·2119
0·7	0·2890	0·2662	0·2564	0·2420
0·6	0·3292	0·3038	0·2924	0·2743
0·5	0·3691	0·3424	0·3300	0·3085
0·4	0·4086	0·3818	0·3687	0·3446
0·3	0·4474	0·4214	0·4081	0·3821
0·2	0·4854	0·4610	0·4479	0·4207
0·1	0·5225	0·5001	0·4876	0·4602
0·0	0·5584	0·5386	0·5269	0·5000
−0·1		0·5761	0·5655	0·5398

For meaning of symbols see Table 1.

In the next stage, the tables with argument y were converted by interpolation to tables with argument z where

$$z = \frac{y - na_1}{\sigma_1\sqrt{n}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The advantage of this procedure is that the tables then show how, with increasing n, F_n approaches the limit

$$F_\infty = \frac{1}{2}\left\{1 - Erf\left(\frac{z}{\sqrt{2}}\right)\right\}$$

as f_n approaches the Gaussian form.

Up to this stage 6 decimal places were retained and the maximum error appeared to be 3 in the last place. Finally the values were cut down to 4 places; apart from slips, the values should therefore be accurate to ½ unit in the last place.

TABLE 3.

$y_0 = 1.$

$a_1 = -0{\cdot}2876.\quad \sigma_1 = 0{\cdot}7935.$

n na_1 $\sigma_1\sqrt{n}$	1 −0·2876 0·7935	2 −0·5752 1·1222	3 −1·1504 1·5871	∞
z	$F_1(z)$	$F_2(z)$	$F_4(z)$	$F_\infty(z)$
3·2				0·0007
3·1				0·0010
			$F_4 = 0$ at	
3·0			$z = 3{\cdot}246$	0·0013
2·9				0·0019
2·8			0·0001	0·0026
2·7			0·0003	0·0035
2·6			0·0006	0·0047
2·5		$F_2 = 0$ at	0·0012	0·0062
2·4		$z = 2{\cdot}295$	0·0021	0·0082
2·3			0·0036	0·0107
2·2		0·0000	0·0057	0·0139
2·1		0·0023	0·0087	0·0179
2·0		0·0056	0·0128	0·0228
1·9	$F_1 = 0$ at	0·0106	0·0182	0·0287
1·8	$z = 1{\cdot}623$	0·0176	0·0252	0·0359
1·7		0·0269	0·0340	0·0446
1·6	0·0052	0·0385	0·0447	0·0548
1·5	0·0293	0·0527	0·0578	0·0668
1·4	0·0553	0·0696	0·0732	0·0808
1·3	0·0829	0·0892	0·0912	0·0968
1·2	0·1123	0·1115	0·1118	0·1151
1·1	0·1433	0·1366	0·1350	0·1357
1·0	0·1757	0·1642	0·1609	0·1587
0·9	0·2094	0·1942	0·1893	0·1841
0·8	0·2443	0·2266	0·2202	0·2119
0·7	0·2802	0·2608	0·2532	0·2420
0·6	0·3169	0·2967	0·2881	0·2743
0·5	0·3541	0·3339	0·3247	0·3085
0·4	0·3916	0·3722	0·3626	0·3446
0·3	0·4292	0·4111	0·4014	0·3821
0·2	0·4667	0·4502	0·4408	0·4207
0·1	0·5037	0·4893	0·4804	0·4602
0·0	0·5402	0·5280	0·5198	0·5000
−0·1		0·5659	0·5586	0·5398

For meaning of symbols see Table 1.

Tables 1–5 give the values of F_1, F_2, F_3, F_∞ for $y_0 = 0{\cdot}0$, $0{\cdot}5$, $1{\cdot}0$, $1{\cdot}5$, $2{\cdot}0$; F_∞ is, of course, the same for all values of y_0. Each column is headed by the values of na_1, and $\sigma_1\sqrt{n}$, which are necessary to reconvert z into y according to (16). Values of F_3 and F_n for $n > 4$, sufficiently accurate for many purposes, can be obtained from the values of F_1, F_2, F_4 and F_∞. For $n > 4$ it may be desirable to use some function of n—such as the reciprocal—for the variable in the

interpolation. The six figure tables for the *F*'s and *f*'s and the intermediate steps are available on application to the author.

TABLE 4.

$y_0 = 1{\cdot}5.$

$a_1 = -0{\cdot}1388. \quad \sigma_1 = 0{\cdot}8789.$

n na_1 $\sigma_1\sqrt{n}$	1 −0·1388 0·8789	2 −0·2776 1·2430	4 −0·5552 1·7579	∞
z	$F_1(z)$	$F_2(z)$	$F_4(z)$	$F_\infty(z)$
3·5				0·0002
3·4			$F_4 = 0$ at	0·0003
3·3			$z = 3{\cdot}727$	0·0005
3·2				0·0007
3·1			0·0001	0·0010
3·0			0·0002	0·0013
2·9		$F_2 = 0$ at	0·0003	0·0019
2·8		$z = 2{\cdot}636$	0·0006	0·0026
2·7			0·0010	0·0035
2·6		0·0000	0·0016	0·0047
2·5		0·0003	0·0026	0·0062
2·4		0·0011	0·0039	0·0082
2·3		0·0025	0·0058	0·0107
2·2		0·0047	0·0084	0·0139
2·1		0·0079	0·0119	0·0179
	$F_1 = 0$ at			
2·0	$z = 1{\cdot}864$	0·0123	0·0163	0·0228
1·9		0·0181	0·0221	0·0287
1·8	0·0081	0·0255	0·0293	0·0359
1·7	0·0223	0·0347	0·0382	0·0446
1·6	0·0382	0·0460	0·0488	0·0548
1·5	0·0560	0·0595	0·0615	0·0668
1·4	0·0757	0·0753	0·0765	0·0808
1·3	0·0974	0·0936	0·0939	0·0968
1·2	0·1211	0·1144	0·1137	0·1151
1·1	0·1468	0·1378	0·1360	0·1357
1·0	0·1744	0·1637	0·1608	0·1587
0·9	0·2039	0·1920	0·1882	0·1841
0·8	0·2352	0·2225	0·2179	0·2119
0·7	0·2680	0·2552	0·2499	0·2420
0·6	0·3022	0·2897	0·2838	0·2743
0·5	0·3376	0·3257	0·3195	0·3085
0·4	0·3739	0·3630	0·3567	0·3446
0·3	0·4110	0·4013	0·3950	0·3821
0·2	0·4484	0·4400	0·4340	0·4207
0·1	0·4860	0·4790	0·4734	0·4602
0·0	0·5233	0·5179	0·5128	0·5000
−0·1	0·5603	0·5563	0·5519	0·5398

For meaning of symbols see Table 1.

The tables are unlikely to be used precisely in the form given for all problems; but a merit of that form is that it appears to be the most concise from which any information required may be most easily derived. Thus in one of the problems from which the investigation arose, it is necessary to know the relation between y and y_0 for given small values of F_n. This information can be derived by finding from the tables, by inverse linear interpolation, the values of z corresponding to a given value of F_n for the various values of y_0 and converting these values of z into y. The results for F_2, F_3 are shown graphically in Figs. 1 and 2, where each curve relates to the value of F marked against it. The curves give the value of y_0, *i.e.*, the value at which the

Table 5.

$y_0 = 2{\cdot}0.$

$a_1 = -0{\cdot}0552. \quad \sigma_1 = 0{\cdot}9415.$

n	1	2	4	∞
na_1	$-0{\cdot}0552$	$-0{\cdot}1105$	$-0{\cdot}2210$	
$\sigma_1\sqrt{n}$	0·9415	1·3315	1·8830	
z	$F_1(z)$	$F_2(z)$	$F_4(z)$	$F_\infty(z)$
3·9				0·0000
3·8			$F_4 = 0$ at	0·0001
3·7			$z = 4{\cdot}366$	0·0001
3·6				0·0002
3·5			0·0000	0·0002
3·4			0·0001	0·0003
3·3			0·0001	0·0005
3·2		$F_2 = 0$ at	0·0002	0·0007
3·1		$z = 3{\cdot}087$	0·0003	0·0010
3·0		0·0000	0·0005	0·0013
2·9		0·0001	0·0008	0·0019
2·8		0·0004	0·0012	0·0026
2·7		0·0008	0·0019	0·0035
2·6		0·0015	0·0028	0·0047
2·5		0·0025	0·0040	0·0062
2·4	$F_1 = 0$ at	0·0040	0·0057	0·0082
2·3	$z = 2{\cdot}183$	0·0061	0·0079	0·0107
2·2		0·0089	0·0107	0·0139
2·1	0·0047	0·0126	0·0145	0·0179
2·0	0·0113	0·0173	0·0192	0·0228
1·9	0·0192	0·0233	0·0251	0·0287
1·8	0·0284	0·0308	0·0324	0·0359
1·7	0·0393	0·0399	0·0412	0·0446
1·6	0·0518	0·0508	0·0518	0·0548
1·5	0·0661	0·0637	0·0643	0·0668
1·4	0·0824	0·0788	0·0788	0·0808
1·3	0·1008	0·0961	0·0956	0·0968
1·2	0·1213	0·1158	0·1148	0·1151
1·1	0·1440	0·1380	0·1364	0·1357
1·0	0·1688	0·1625	0·1604	0·1587
0·9	0·1958	0·1895	0·1869	0·1841
0·8	0·2250	0·2187	0·2157	0·2119
0·7	0·2561	0·2501	0·2468	0·2420
0·6	0·2890	0·2834	0·2799	0·2743
0·5	0·3235	0·3185	0·3149	0·3085
0·4	0·3594	0·2551	0·3515	0·3446
0·3	0·3964	0·3928	0·3894	0·3821
0·2	0·4342	0·4313	0·4282	0·4207
0·1	0·4725	0·4703	0·4675	0·4602
0·0	0·5109	0·5095	0·5070	0·5000
−0·1	0·5491	0·5484	0·5464	0·5398

For meaning of symbols see Table 1.

distribution must be cut, in order that the probability that the sum of the values of selected members will exceed an assigned value y_s, shall be the value of F_2 marked against the curve.

4. *Example of the Use of the Tables and Figures*

Consider the problem of the metal parts mentioned in Section 1. Suppose it is found that the parts (which are being manufactured to a nominal mean length of 0·5 inch) have, in fact, a mean value of 0·503 inch with a standard deviation of 0·01 inch. Two of the parts are used together and in at least 99 cases out of 100 the total length must not exceed 1·02 inches. What should be the upper limit of acceptance of the parts?

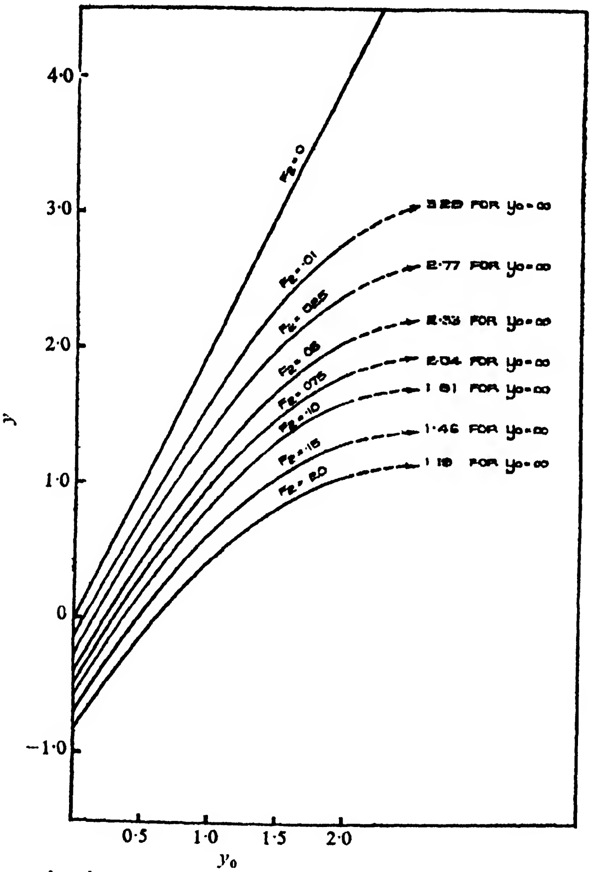
Here $\sigma_0 = 0{\cdot}01$; $\bar{x} = 0{\cdot}503$; $n = 2$.

From equation (3) we must have the sum of the lengths in a random sample of two less than

$$y_t = \frac{1{\cdot}02 - 2\bar{x}}{\sigma_0} = 1{\cdot}4$$

with a chance of failure F_2 of 0·01.

Reference to Fig. 1 shows that for these values of y_t and F_2 the value of y_0 is 0·9, so that all parts longer than 0·512 inch should be rejected.



F_2 gives the chance that the sum of the values of the variate in samples of two is greater than y_t.

FIG. 1.

Value (y_0) at which normal population should be truncated.

Now suppose that a test is applied to the mounted components and *all* that fall outside the limit of 1·02 inches are rejected. Suppose, further, that the nature of the process in which the metal parts are used makes it impracticable to select them in pairs. The number of mounted components which have to be rejected on final test will, of course, depend upon the acceptance limit of the individual components. The effective cost of mounting a component, we will say, is nine times the manufacturing cost, so that the value of a pair of mounted components is 20 times the cost of manufacture of a single component. What should be the acceptance limit for the parts in order to obtain a minimum cost of manufacture of the mounted components?

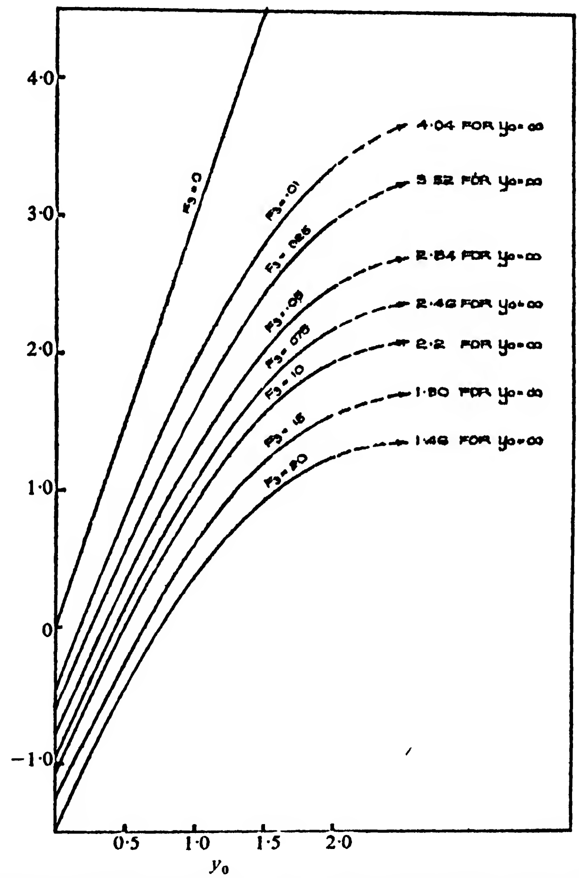
Let the acceptance limit (y_0) be 1·0, so that all parts of length greater than 0·513 inch are rejected. From the value of F_∞ in Table 2 we find that this involves the initial rejection of

16 per cent. of the components. To show the use of the Tables, we will find from Table 3 the percentage of the mounted components to be rejected. We find that $a_1 = -0{\cdot}288$, $\sqrt{2}\sigma_1 = +1{\cdot}12$ so that from equation (18)

$$z = \frac{1{\cdot}4 + 0{\cdot}576}{1{\cdot}12} = 1{\cdot}76$$

and interpolating in Table 3 we find that, approximately

$$F_2(z) = 0{\cdot}021.$$



F_3 gives the chance that the sum of the values of the variate in samples of three is greater than y_s.

FIG. 2.

Value (y_0) at which normal population should be truncated.

Therefore 2·1 per cent. of the mounted components have to be rejected. Since in this example only rough values are required, the result could be obtained also by interpolating in Fig. 1; we then find for what value of F_2 the curve passes through the point $y_s = 1{\cdot}4$, $y_0 = 1{\cdot}0$. It is necessary to use the Tables when accurate values are required.

If the cost of each metal part is c and we start with n such parts, after the initial rejection $0{\cdot}84n$ parts have to be mounted and $0{\cdot}84n \times 0{\cdot}979$ parts are finally used. The total cost of these $0{\cdot}82n$ parts is

$$nc + (0{\cdot}84n \times 9c).$$

Thus the cost per part finally used is 10·43c. In Fig. 3 the full curve shows the variation of this cost with y_0. The cost is given by the scale on the left. The scale on the right for the dotted curve gives the percentage of parts rejected as a result of the first acceptance test. It is seen that the minimum cost is about 10·3c, and it occurs for a value of $y_0 = 0{\cdot}75$ corresponding to an acceptance limit of 0·5105 inch. In this particular example it would, of course, be absurd to consider

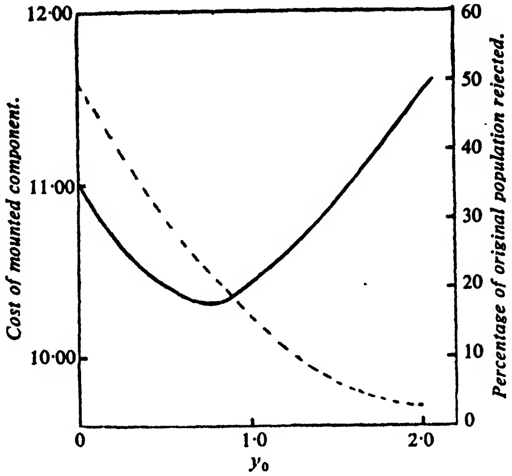


FIG. 3.

reducing the acceptance limit below $y_0 = 0{\cdot}7$, since the possibility of getting a final value for two components greater than 1·02 just becomes zero when everything greater than 0·510 is rejected on first test.

The calculations do show, however, that in the case considered it is necessary to set the initial acceptance limit at a value which gives practically no rejects on final test. In other cases of practical importance the minimum cost is obtained when there are rejects both on initial and final test.

Statistical Control Applied to High Duty Iron Production

By E. W. Harding

[Paper discussed before the London Group of the Industrial Applications Section of The Royal Statistical Society, January 26th, 1945.]

Introduction

The use of statistical control in foundry work is somewhat of a departure from the type of application which has been most frequently discussed at public meetings and in the technical press. For this reason it is felt the subject will provide some new interest and provoke discussion. The subject will be dealt with from the point of view of the technical man applying statistical methods to a branch of industry sorely in need of help in its control problems.

The application which will be described is that of controlling metal quality in foundries making Meehanite Metal, a high-duty cast iron. The problems present novel features of statistical control in industry. The control system developed by the author for these foundries will be described, together with the results obtained in practical operation.

The Control System

The development work in applying the control system was carried out within a group of some 15 foundries producing Meehanite Metal. Each foundry operated under widely different conditions of output, equipment, control facilities, types of metal, etc., and this added to the difficulties of establishing a standard system. On the other hand, a favourable factor was the centralized and uniform system of technical control prevailing for the whole group.

A brief description of the operating conditions will help in understanding the nature of the problems involved. The metal is melted in a vertical shaft furnace, called a cupola. Solid metal and fuel (coke) are charged continuously in at the top of the shaft. Combustion takes place at an intermediate zone by forced blast entering through air-ports lower down the cupola. The molten metal drops to the bottom of the shaft, from where it is tapped into ladles, either continuously or at intervals. There is a continuous supply of molten metal, and this metal, taken away in ladles, is subsequently poured into moulds to make Meehanite castings. Each ladle of metal is tested for its suitability for the particular type of casting to be produced, but in the time available this test can be qualitative only, providing an indication of metal structure obtainable in the casting. The complete testing for chemical and physical properties is carried out later.

The aim of technical control is to ensure standard chemical and physical properties in the metal produced. The complexity of the problem arises from the many sources of variation present; raw materials variation, charge weighing, charging practice, variable combustion conditions, etc., and the varying extent to which these operations are dependent on the human element.

In the past the problem has been approached entirely by judgment applied to the various operating controls; for example, control of raw material composition, accuracy of charge weights, charging procedure, blast volume measurements, etc. These controls are subject to special supervision, and every effort is made to keep variations from standard practice within reasonable limits. The method, however, failed to distinguish between variations in the final product due to causes inherent in the process and operating conditions and those due to assignable causes. It also failed to relate cause and effect—that is, the operating practice and the result in terms of metal test values.

In these circumstances there was always difficulty in deciding when corrective action should be taken to bring test values back to standard. This, again, was a matter of personal judgment, and often the correction only resulted in a more serious variation. Also, the nature of the corrective action to be applied was seldom clearly indicated.

A further limitation in the normal method of control was in regard to measuring the standard of control in operation over any given period or for any particular foundry. It was not possible to obtain a quantitative measure of control, by which the progress of a foundry could be accurately assessed or comparison made between one foundry and another. The nearest approach obtainable was an estimate, based on human judgment, of the standard of operating practice.

However, as is frequently the case in industry, there was available at each of these foundries a mass of test data which was not being sufficiently utilised. Each foundry engaged in producing Meehanite Metal normally takes from three to six samples of the metal melted per day for complete testing, depending on the number of types of metal produced and the quantity of metal melted. It was evident that these test results were eminently suited to statistical treatment, and the first step in applying statistical control was the collection of test data from all the foundries concerned for the previous six months. These consisted of chemical and physical test results for the various types of Meehanite Metal produced, and in each case values for Average and Standard Deviation were obtained for each property and each foundry.

From this work, and even at this early stage in the development of the control system, some very interesting and useful information was obtained. Differences between foundries in variation values were shown clearly, and it at once became evident that a sound basis for measuring the standard of control in operation at each foundry had been found. It was noted also that there was little or no relation between the magnitude of the test values (Average) and the variation value (Standard Deviation). This meant that a standard value for variation could be set, regardless of the type of metal produced. It was also observed that the standard of control was not dependent so much on the control facilities available at the foundry as upon the conscientious and consistent effort on the part of the control staff to operate to standard practice. Further it was found that in general the standard of control in operation was lower than had been assumed.

The next step was the setting of standards to be applied to the whole group of foundries. Standards were already in force for the magnitude of chemical and physical test values for each type of Meehanite Metal. Standards were now set for permissible deviation from these values, based on what the best controlled foundries could do and had already done. That is, Standard Deviation values were established in Meehanite practice for the principal chemical and physical properties. The values for the various properties are shown in Table I.

TABLE I

Standard Deviation Values for Meehanite Metal

Total carbon	0·08%
Silicon	0·12%
Manganese	0·06%
Tensile strength	1·00 tons/sq. in.

At this point interest was aroused in testing errors. It was felt desirable to establish to what extent these errors, rather than true metal variation, were responsible for the total variation. This applied particularly to tensile testing which was suspected of being unreliable. A preliminary series of tests was run at seven different foundries, and in each case six tensile bars were cast from the same ladle of molten metal, under controlled pouring, so that the conditions were the best obtainable for uniform results. Machining and testing of these bars was then carried out according to usual practice for the foundry concerned. The results obtained are set out in Table II.

These results indicated that testing errors were variable and generally large; in some cases higher than the total permissible metal variation. Steps were taken to reduce these errors, and subsequent control checks showed that for good average condition the normal Range for tensile testing errors for samples or groups of six was from 0·50 to 1·00 ton/sq. in. That is, of the total permissible variation due to metal and testing, represented by a Standard Deviation value of 1·00 ton/sq. in., the variation due to testing is equivalent to a Standard Deviation value of from 0·2 to 0·4 ton/sq. in.* This still appears a high proportion of the total, but is the best that can be done with present testing technique and equipment. Errors in chemical testing were less serious, but in some cases required controlling and reduction.

The establishment of the control system finally adopted followed on the above preliminary work. However, a number of questions had to be settled before control chart work could be started. These included:

1. *Measure of Variation to be Used.* Range was adopted chiefly on account of greater ease in calculation than for Standard Deviation—an important point in an application of this kind.

* In this class of work the testing error of necessity includes any variability in metal quality of bars cast from the same ladle.

TABLE II

Consistency Tests for Tensile Testing

Foundry No.	1	2	3	4	5	6	7
Type Meehanite	GD	GC	GA	GD	GC	GD	GB
Test bar dia.:							
As cast, inch	0·875	0·875	1·20	0·875	0·875	0·875	0·875
Machined, inch	0·505	0·564	0·798	0·505	0·564	0·564	0·564
Testing machine	Avery	Buckton	Avery	Buckton	Buckton (hand operated)	Buckton	Avery
Tensile obtained, tons/sq. in.:							
Bar No. 1	17·70	18·40	25·25	17·00	19·86	17·55	23·00
2	18·00	19·20	26·47	15·75	20·00	17·68	22·70
3	17·93	19·84	25·35	18·90	19·29	17·80	21·80
4	16·63	19·16	23·26	17·50	18·11	17·26	22·60
5	17·06	19·04	24·85	20·00	19·11	17·43	22·00
6	17·46	19·72	22·20	17·70	18·42	17·40	21·70
Average	17·46	19·23	24·56	17·81	19·13	17·52	22·30
Range	1·37	1·44	4·27	4·25	1·89	0·54	1·30

2. *Sample Size.* Considerations of sampling procedure, time of completion of a group of tests, made for the eventual choice of sample size of six.

3. *Control Chart Limits.* 99·8 per cent. limits were adopted, as being the simplest to operate and understand. No inner limits were used in the early stages of this work.

4. *Basis of Chart.* That is, whether the chart averages and limits should be based on the past performance of each individual foundry or on a single set of standards. The Standard chart was adopted, because the essential point in Meehanite practice is that, for each type of Meehanite Metal, all foundries shall produce the same material and operate to the same standard of control.

5. *Sampling Procedure.* A Standard sampling practice was found to be difficult owing to the varied conditions of operation, especially in regard to tonnage produced, tap weights and number of types of Meehanite melted. For example, a foundry operating with a large number of small tap weights would tend to show a lower ratio of weight of metal tested to total melt than a foundry using a small number of large tap weights. In extreme cases of the latter kind almost complete 100 per cent. sampling could be obtained, i.e., all taps were sampled. An example of this is seen in blast-furnace practice, where, in fact, there is 100 per cent. sampling. However, it was found possible to establish a maximum and minimum sampling ratio which would meet all cases; that is, from 1 in 5 to 1 in 10, with 1 in 7 as standard practice. For example, a foundry melting 15 tons of the same type of Meehanite Metal and tapping the molten metal in 1-ton lots (that is, 15 ladles of 1 ton each), would sample and test two ladles, thereby completing one group in three days for that type of Meehanite. It will be realized that each ladle of metal is assumed to be homogeneous and therefore the sample is an estimate of the properties, not of the material in the ladle, but of the heat or melt as a whole. That is to say, the population is the total metal melted, of which the ladles represent the individuals. The sampling ratio is thus reasonably high and, as a further assurance of a reliable estimate of product properties, a special kind of representative sampling was adopted. This consists of sampling in rotation, whereby, instead of sampling always from a fixed position, the position of the ladle sampled is progressively altered. Thus, Tap 1 on the first day, Tap 2 on the second day, and so on to Tap 7 on the seventh day, after which a return is made to Tap 1.

6. *Test Results to be Charted.* In order to make the system simple and easy to operate without undue clerical work, only the most important properties are charted. These are Total Carbon and Silicon, representing chemical composition and tensile strength for the physical properties. For convenience, the Average and Range of each of these three properties are put on the one sheet. This also assists chart interpretation, as will be explained later. With each foundry producing normally two to three main types of Meehanite Metal, each

foundry has usually two control sheets, and sometimes three or more, to operate. They are not encouraged to chart more than three types of metal, since it rarely happens that three mixes represent less than 75 per cent. of the total foundry output. There are good reasons for this, mainly in that the standard of control can accurately be assessed on 70–80 per cent. of the output; that, in general, satisfactory practice for this proportion of the melt ensures correct practice and provides a reasonable assurance of quality for the remainder; that small quantities of special mixes require too long a time for completion of groups, and that a multiplicity of charts tends to confuse the issue, and makes for more clerical work than is desirable, especially in the present stage of development. A minimum of two control sheets is preferred, since the chart results for a single mix, unsupported by those for other mixes, are less easily interpreted.

The control chart finally adopted takes the form of a sheet measuring 20 inches high by 16 inches wide, on which group values for Average and Range of the three properties are plotted. This chart covers from six months to one year's operation. Modifications have been made as experience is gained with the system, and further changes are expected, particularly in regard to reduction of standard control limits, as operating practice and control improves.

The installation of the system presented no difficulty. A standard Instruction Sheet was issued explaining the system in very simple language and co-operation in putting it into practice was readily obtained.

PRACTICAL RESULTS OBTAINED FROM SYSTEM

Early results, charted before the system was actually in operation, showed clearly many failures to conform to Standard and a serious lack of control. One example is shown in Fig. 1. Figs. 2

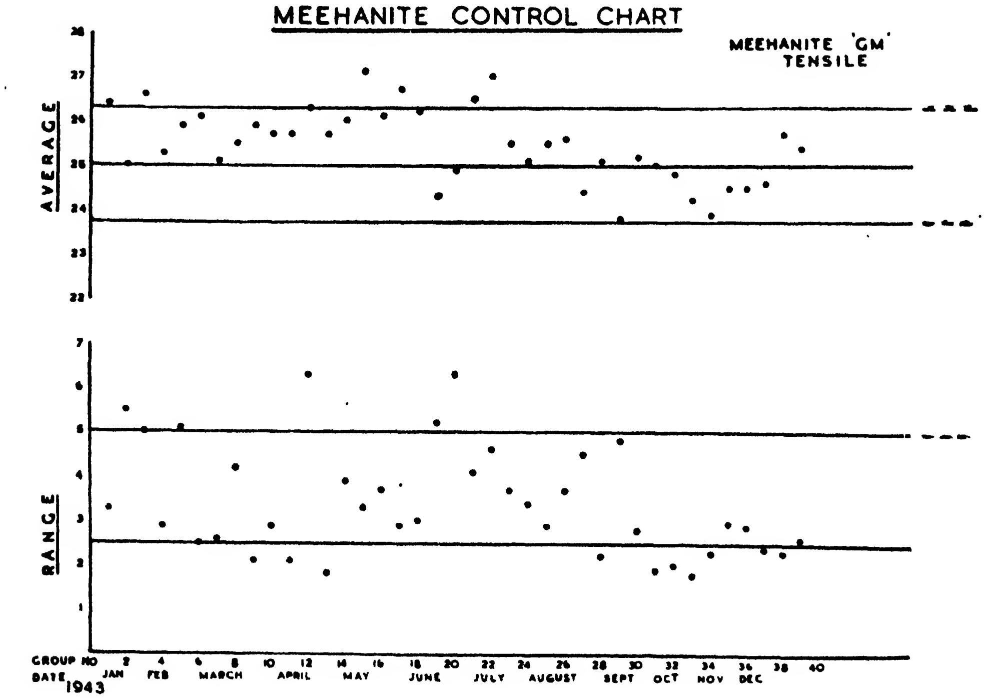


FIG. 1.

Illustrating type of result obtained before installation of control system.

and 3 show the very definite improvement obtained almost immediately after putting the system into operation. This improvement took the form, both of a closer adherence to Standard values

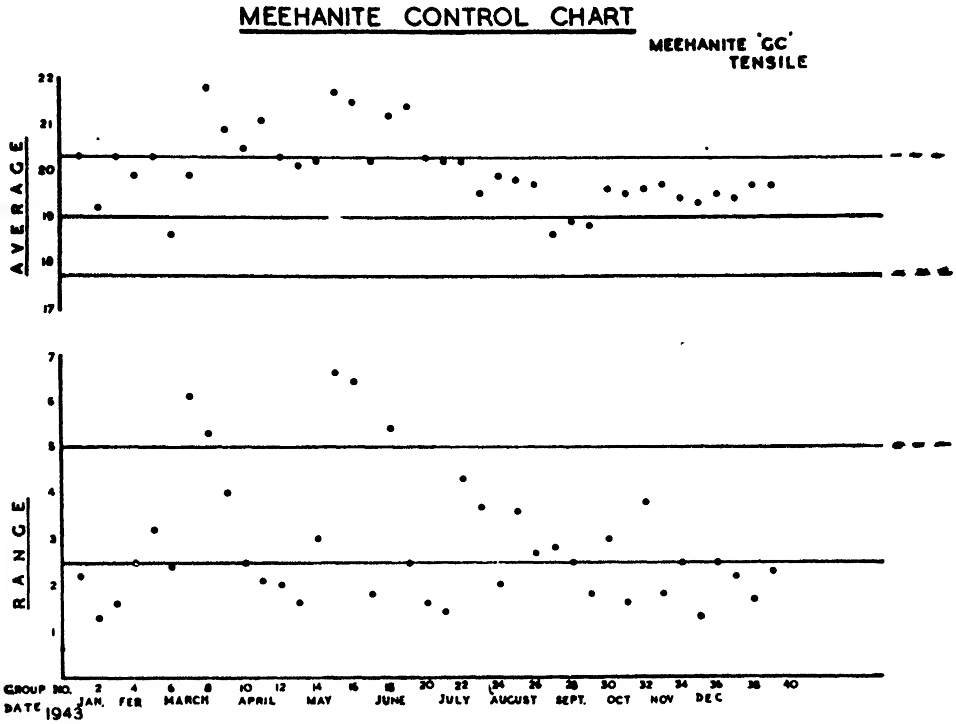


FIG. 2.

Showing improvement in standard of control from introduction of chart system (from July).

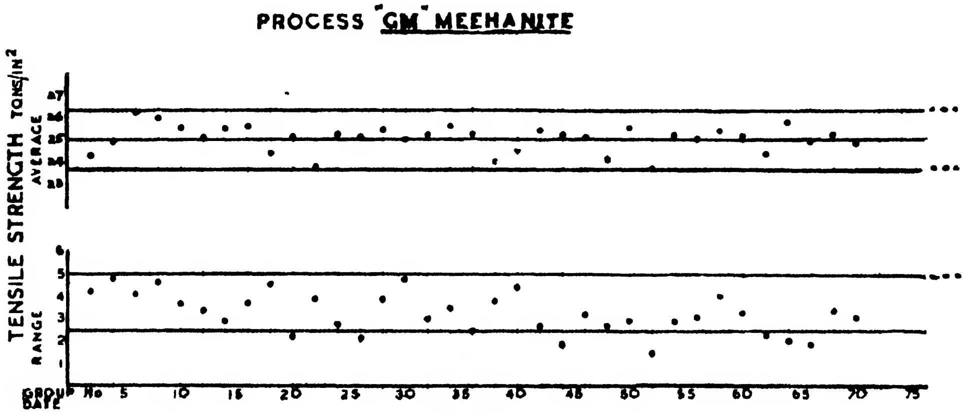


FIG. 3.

Showing present results with improved standard of control.

for the properties (Average charts), so that the groups fall completely within the limit lines, and also of a considerable reduction in product variation (Range charts), giving in some cases Range values far below the Standard values.

This remarkable improvement was due to two main factors. Firstly, the psychological effect of this form of chart, which by drawing attention to both Average and Range values produces a conscious effort to meet the chart limits. Secondly, the control charts give early warning of an

impending change in product properties and breakdown in control. They also provide information as to the probable cause of the change. Fig. 4 illustrates how persistent trends in product test values can occur without attracting attention until too late to prevent an undesirable change in quality. With the control charts, these trends become immediately apparent and a breakdown in control can thus be averted.

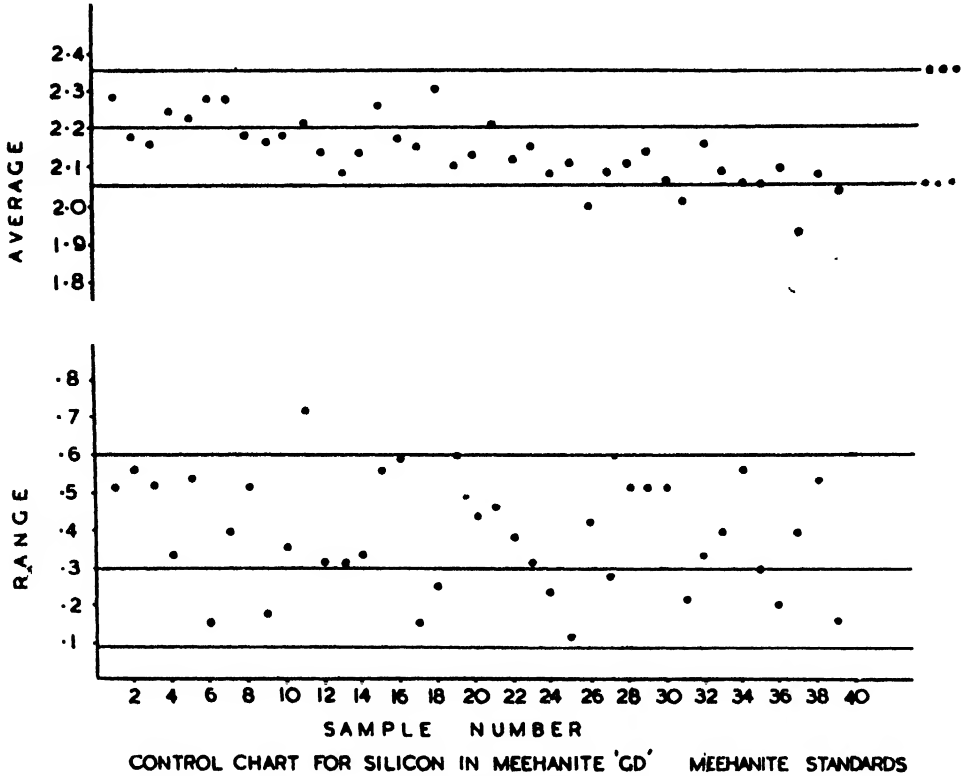


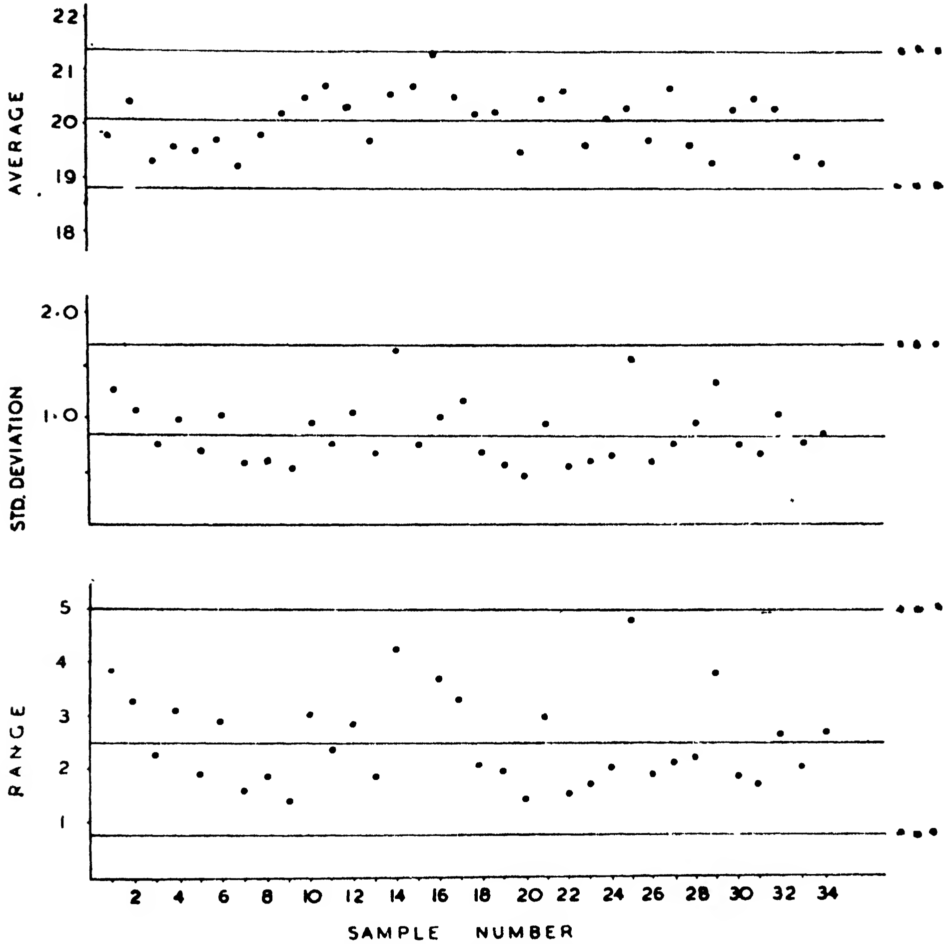
FIG. 4.
Showing type of trend experienced in test values difficult to detect without control chart.

The relation between a chart based on past performance of an individual foundry and the Standard chart is illustrated by Figs. 5 and 6 respectively. The examples show that a foundry may maintain its product in a state of control, based on its own past performance, and yet show a complete breakdown where the state of control is based on a standard performance, i.e., a process may be controlled at an unsatisfactory level. The examples show a lack of conformity in regard to Averages values. A similar lack of conformity may be found in Range values, or in both Range and Average.

The principal benefits from the system are associated with the early correction that can be applied to check off-standard tendencies. However, it has also proved useful in investigations as to causes of trouble. An example of this is seen in the case of a foundry troubled with high Range values in Total Carbon content. In this case the Averages were reasonably satisfactory and difficulty was experienced in tracing the source of the high day to day variations. By re-grouping the individual results, according to the period at which they were melted in the heat and plotting the groups so obtained on separate control charts (shown in Fig. 7) it became evident at once where the trouble lay. The average values for the latter portion of the heat were found to be consistently off-standard and the remedy was immediately apparent.

In general, the higher standard of control obtained since the introduction of the system is

practical proof of the value of this control system. Its possibilities have not yet been exhausted, and further benefits will undoubtedly result from greater experience in its operation. For example, accurate chart interpretation is as yet far from full development, and it is in this direction that progress in the immediate future will lie. Special attention is being given to interpretation technique, and some notes on the progress made will be given.



CONTROL CHART FOR TENSILE ON MEEHANITE 'GC'—BASED ON PAST PERFORMANCE.

FIG. 5.

Showing results on chart based on foundry past performance (compare with Fig. 6).

CHART INTERPRETATION

In all control chart work a point on which emphasis has been laid has been the importance of relating cause and effect. The charts themselves are only a means to the end—that of correcting faulty operating practice causing product variation. They can serve this purpose only if they can be interpreted correctly and the necessary corrective action applied in practice. Hence the technique of chart interpretation must be developed and mastered.

A glance at chart results gives a general indication of the state of control, and this is useful in assessing the control position in the foundry. But for the real work of maintaining a state of control, averting an impending change or correcting a change that has taken place, a close study of the

chart points is essential. Only in this way can the hidden clues as to operating causes be extracted. It is, in fact, surprising how much information of a technical nature can be gained merely by deductions drawn from control chart clues.

(a) *A Guide to Chart Interpretation.* In order to assist Meehanite foundries in this work, a Guide to Chart Interpretation has been drawn up. This is based on experience gained over some two years' working of the system. It is by no means complete, but it does tie-up cause and effect

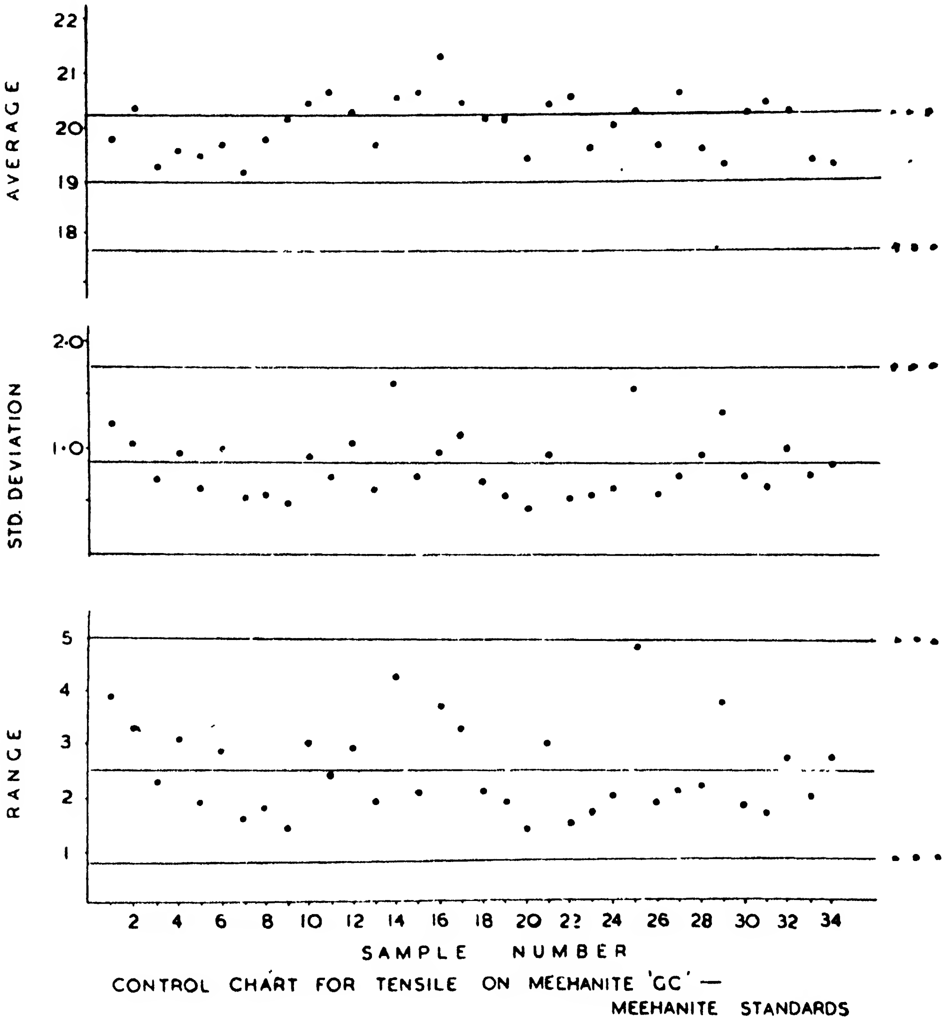


FIG. 6.

Same results as in Fig. 5 but on Meehanite Standards Chart (note also comparison between Standard Deviation and Range).

in such a way that it is now possible from the charts to establish the technical factors responsible for a change, in most cases without even going into the foundry. To appreciate this point it must be realized that a change in any one of the properties of the product may be caused by one or more of a large number of operating factors.

The procedure followed in tracking down the operating factors responsible for a change is mainly one of elimination. It resolves itself into putting the following five questions:—

1. Does the change occur on all mixes (that is, types of Meehanite) or on only one mix?
2. Does the change occur on all properties of the mix or mixes affected, or on only one property?
3. Are Average values or Range values, or both, affected?
4. Is the change a gradual one or is it an abrupt fluctuation?
5. What degree of correlation exists between chemical and physical properties?

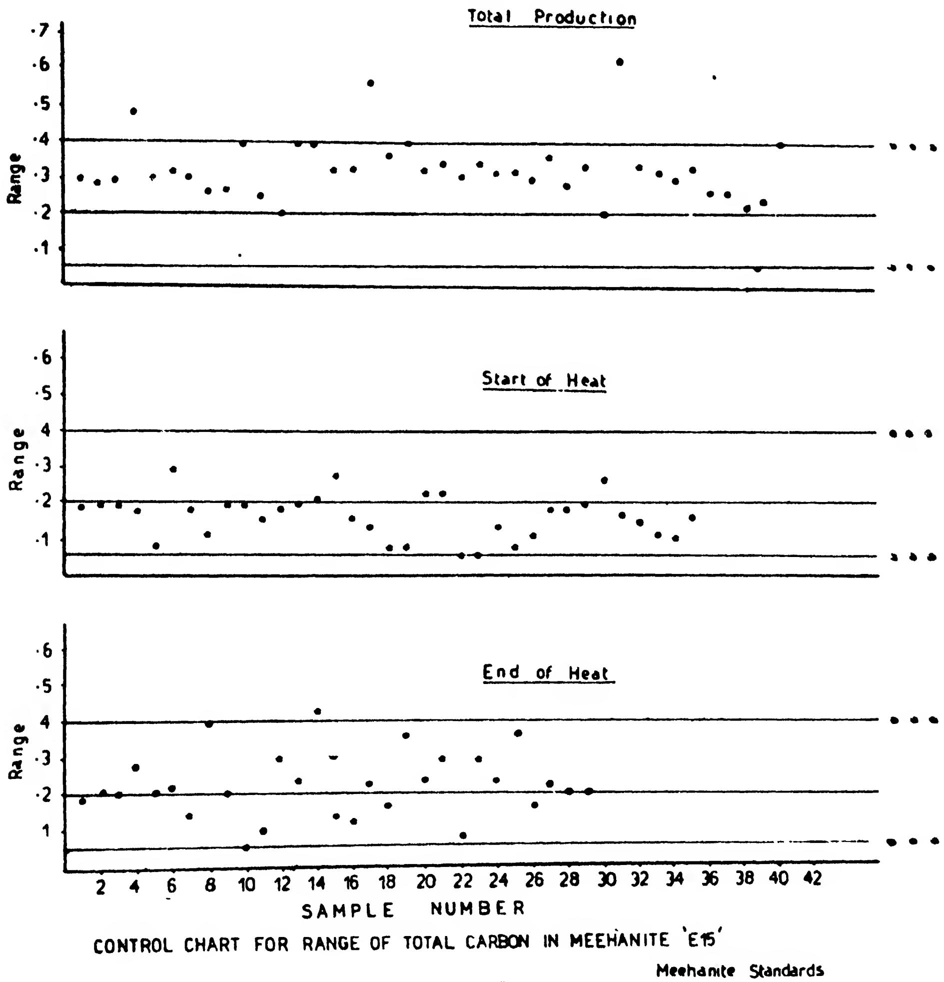


FIG. 7.

Showing Range values before and after regrouping results.

To understand the procedure, it must be realized that each foundry operates at least two control sheets, each of which gives results for Average and Range of three properties of a mix, or type of Meehanite. Consequently, the results for each property of one mix can be compared for the corresponding results on another mix, and advantage is taken of this fact to verify suspected causes of trouble. It will be possible here to give only a bare outline of the steps involved in locating the cause of trouble. The questions will be examined briefly in turn.

1. If the change is confined to one mix, then factors specific to that mix are examined; for example, mix composition, raw materials confined to that mix, position in the heat, etc

If the change is common to all mixes, then attention is concentrated on factors common to all mixes.

2. If the change occurs on all properties of a mix, then certain operations affecting all these properties are examined. This considerably restricts the number of possible factors. If the change is confined to one property, then the factors affecting that property are examined in the light of the answer to the first question. The procedure to be followed in the case of each property is detailed in the Guide. Usually at this stage sufficient information has been obtained to restrict the possible causes to one or two.

3. Three cases are involved in the third question, and each is examined in detail in the Guide, giving the causes indicated and the remedies to be applied for each contingency. For example, in the first case Average values are in control, but Range values fall outside the limits. Here the trouble is traced to variation in test values tending to balance one another, due to periodicity of operating variables. Factors of a periodic nature are listed for examination, such as, variations due to the use of different cupolas on alternate days, change in operators as in shift work, melting carried out at different times—*e.g.*, beginning and end of the heat, for the same mix, etc. Similarly, causes and corrective treatment are given in the Guide for the other two cases.

4. The fourth question—that of the nature of the change (gradual or abrupt)—is significant technically in that certain operating factors tend to produce a slow but cumulative effect, while others are immediate in their influence. For example, a change in coke quality tends to produce a gradually increasing (or decreasing) Total Carbon content in the metal extending over a long period. In such a case reference to the answers to questions 1 and 2 would confirm if this could be the factor responsible.

5. The question regarding correlation between chemical and physical properties is aimed at checking the possibility of errors of measurement. The term is used to include sampling procedure, accuracy of chemical and physical testing, and generally any factor which may result in incorrect test data and lead to false conclusions. For example, if Total Carbon values fall, with Silicon remaining constant, lack of a related rise in Tensile would lead to a check on testing practice. Similarly, chemical properties in control, with Tensile out of control would indicate definitely that an abnormal condition existed in testing procedure and led to an investigation on this point.

In a further section of the Guide a list is made of the main divisions of the melting process, called General Variables; that is, factors which are fundamental to the process and on which control and supervision are concentrated. Against each General Variable is listed all the operations which are involved in that particular basic factor. By this means, the actual operation responsible for trouble may be picked out of a large number of possible causes.

(b) *Operating Changes Chart.* This is an addition to the main control chart, on which all changes of an operating nature are recorded at the time they are made. It is placed below the main chart, with the group number and dates coinciding in a vertical line. For example, if a change is made in coke grading, it is recorded for reference at the date and group number in which the new coke is put into use. The effect of the change, if any, can then be traced on the main chart and, if necessary, correction made to hold the product to standard values. This has been found to reduce the work of searching for causes and avoids dependence on memory.

(c) *Rating of Standard of Metallurgical Control.* One of the incidental advantages of this system of control is that it permits of applying a numerical rated value to the standard of control in each foundry. This is extremely useful, not only in assessing the progress made from one period to another for the same foundry, but also in comparing the standard of work from one foundry to another.

The assessment is based on two factors:—

1. The closeness of approach of the metal test values to Meehanite Standard values.
2. The degree of consistency in these test values.

For the first factor, the deviation of each of the group Averages, for the period under consideration, from the standard Meehanite value for the property is calculated. The mean of these deviations is found and related to the standard permissible deviation for the property. A per-

centage rating is then taken direct from a Rating Chart, on which zero deviation is given 100 per cent. rating and deviation equal to the standard value given 75 per cent. rating. Other values are proportionately rated.

For the second factor, the mean of the group Ranges is taken for the period and, from a second Rating chart, a percentage rating is obtained. This is based on 100 per cent. rating for zero mean Range and 75 per cent. rating for Range value equal to the standard value set for the property. Other values are proportionately rated. The final rating is the mean of the Group Deviation and Range ratings for the type or types of Meehanite made.

FUTURE DEVELOPMENTS

The statistical system of control described has so far been applied completely only to metal control in the foundry. It is hoped eventually to extend it to the final product, which is the casting

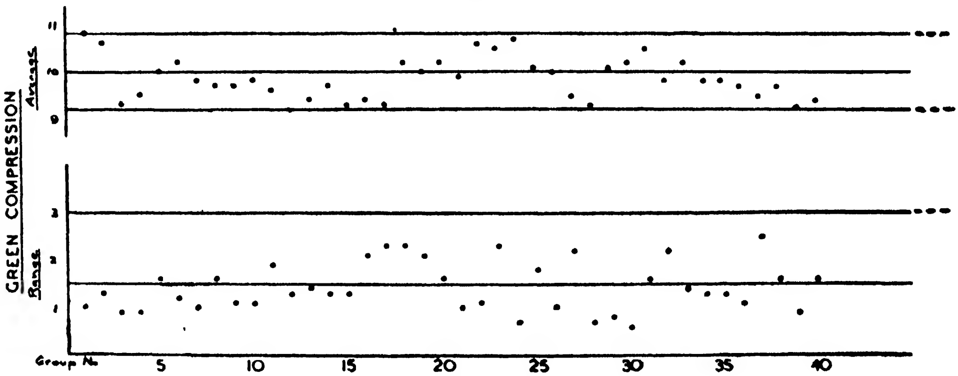


FIG. 8.

Showing results for compression strength in moulding sand.

itself; but owing to difficulties in measuring the properties of the casting this has not yet been accomplished. In the meantime, a start has been made by extending the application of the system to other operations involved in casting production. Of these, control of moulding sand properties is next in importance to metal control and the system has recently been put into operation for moulding sand control. The results obtained so far have shown excellent promise, particularly when applied to mechanical sand preparation. One of the great advantages of the system for moulding sand control is the early detection of trends away from standard, which has always been a source of trouble in sand control. Fig. 8 illustrates the type of chart used and some typical results obtained for sand compression strength.

It is hoped that this description of the application of Control Charts to cast iron production will encourage the use of this system of control for other metals and melting processes.

Ultimate Risks in Sampling Inspection

By Capt. A. H. R. Grimsey, R.A. (*Military College of Science, Blurton, Stoke-on-Trent*)

[Paper discussed before the London Group of the Industrial Applications Section of The Royal Statistical Society, November 24th, 1944.]

Whenever the suggestion is made that examination of a manufactured product should be by some scheme of inspecting only a proportion of the output, the first query which is made is "What risk am I undertaking?" This is the question which is put equally by producer, inspector and consumer, and unless it can be answered satisfactorily, then the proposed system of inspection will not be accepted, in spite of the economies which it may produce.

It is, of course, quite apparent that if some material is to be accepted or rejected unseen as a result of examination, then there are risks to be taken. The sample inspected may not be representative of the bulk; this is an important point, and one which is often overlooked in practice. No deduction concerning the bulk, based on the examination of such a sample, can have much validity. But even when the sample is chosen in such a way that it is thought to be as closely as possible equal to any other which might have been chosen (this defining a "random sample"), there are inevitably variations from one sample to another.

Thus from a batch of a certain quality, say 5 per cent. defective (*i.e.*, 1 article in 20, on the average, being a defect), then a random sample of 20 may contain 0, 1, 2, 3, . . . defects. We may expect a sample to contain just 1 defect—this is the mathematical expectation, and it can be shown that more random samples will contain 1 defect than any other number of defects. Nevertheless, we may select a sample containing no defects at all. Such samples will not be rare occurrences, their relative frequency being calculable. Other samples will contain 2, 3, . . . 20 defectives. The random sample containing 20 defects will certainly arise very rarely; the chance of it happening can be shown to be 1 in 20^{20}. On the other hand, if such a sample is not randomly selected from the bulk, then it might be quite a common occurrence.

The relative frequencies with which the various possibilities occur are, in fact, given by the following table:—

No. of defectives	Proportion of samples	Percentage of samples
0 defect	$19^{20} \div 20^{20}$	35·85
1 "	$20 \cdot 19^{19} \div 20^{20}$	37·73
2 or more defects	$20^{20} - 39 \cdot 19^{19} \div 20^{20}$	26·42
		100·00

Figures in the final column are the percentage probabilities which can be interpreted as either the relative frequency of occurrence over a long period or the chance (measure of confidence) of that particular result occurring in one sample.

This has been examined at length to show that a sample containing fewer defects than the average will occur quite often, and so will samples containing more. If the sampling scheme is based upon such an idea as "accept only batches which produce samples containing not more than 1 defect," then 26·42 per cent. of batches of quality 5 per cent. defective will be rejected on the average of a long run. Moreover, such rejection will be almost automatic over a long period, and there is *no difference between the quality of those accepted or rejected* according to this test, since all batches have been assumed to contain 5 per cent. defects.

Therefore, if 5 per cent. is a permissible proportion defective, the producer of material of this quality stands the risk of having over a quarter of his output unjustifiably returned to him. On the other hand, if 5 per cent. defects be not permissible, then such a scheme would lead to the wrongful acceptance of about three-quarters of the batches offered, and this is clearly a grave risk to the consumer.

The magnitude of such risks to both parties to any sampling scheme can be similarly evaluated for any assumed percentage of quality in the batches to be supplied. If a batch is condemned or approved as a result of one sample, this is known as a Single Sampling scheme. The following results apply to the scheme: " Take a sample of three and test; reject if one or more fails, accept if all pass," when applied to batches of 100 articles.

% defective in batch ...	0	10	20	30	40	50	60	70	80	90	100
Chance of acceptance ...	1·00	0·726	0·508	0·338	0·212	0·121	0·061	0·025	0·007	0·001	0·000

These results are illustrated in Fig. 1.

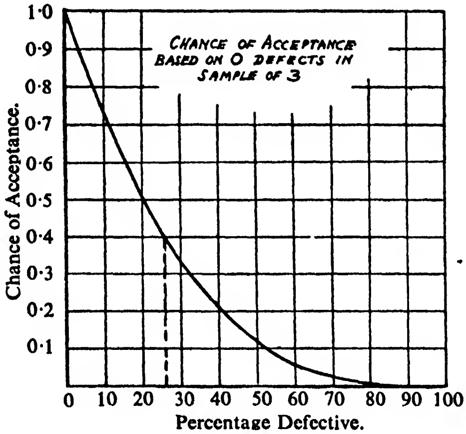


FIG. 1.

It may be desired to give the producer a greater margin by the modification of the scheme to read: " Accept if all are sound out of a sample of three, reject if two or more are defective; if one is defective, take a further sample of five and accept if no defects then occur."

This scheme, with its first not-proven category and subsequent re-test, is typical of many Double Sampling schemes. Multiple or Sequential schemes are obtained by extending this idea to further re-testing.

If the quality of the batch is known (*i.e.*, the " percentage defective "), then the chances of acceptance, rejection on first sample, and the necessity for re-test can all be calculated and plotted. (See Figs. 2*a* and 2*b*.)

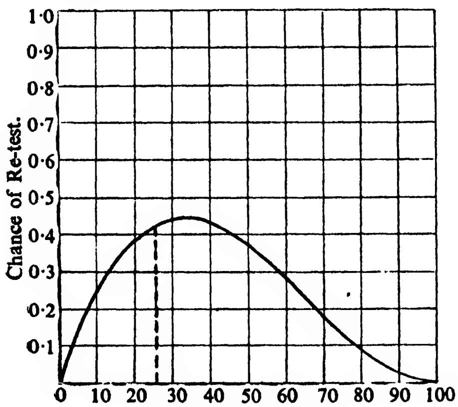


FIG. 2*a*.
Chance of Re-test based on 1 Defect in Sample of 3.

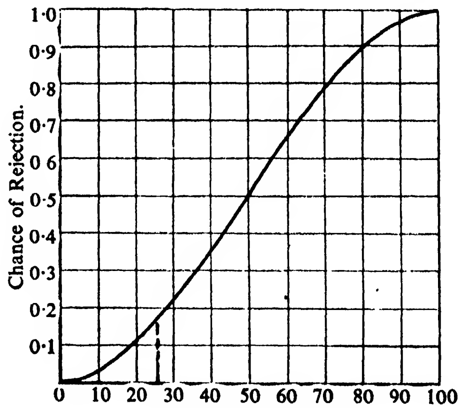


FIG. 2*b*.
Chance of Rejection based on 2 and 3 Defects in Sample of 3.

These results for the first sample are most conveniently shown together in one diagram (Fig. 3). In this Figure the ordinate at any percentage defective is unity, but is divided into three parts corresponding to the chance or relative frequency of accepting, re-testing, and rejecting repeated batches of each percentage defective. These are denoted:—

L—Accept on 1st test.
M—Re-test on 1st test.
N—Reject on 1st test.

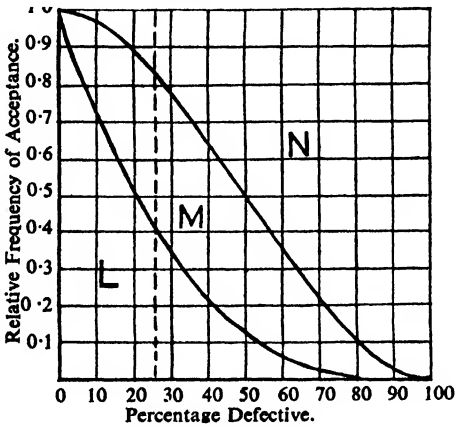
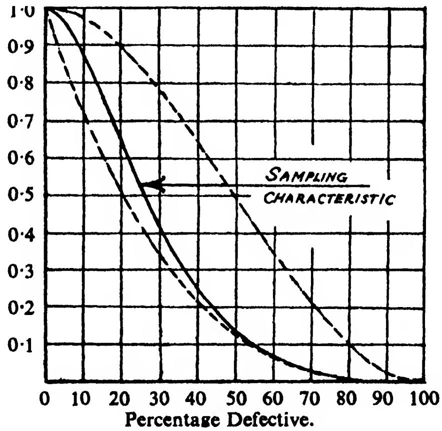


FIG. 3.
Illustrating Result of First Test in a Double Sampling Scheme.

Thus, considering the ordinate represented by the broken line in Fig. 3, the portion in the area marked "*L*" is equal to the ordinate in Fig. 1; that portion in area marked "*M*" is equal to the ordinate on Fig. 2*a*; and that portion in area "*N*" is equal to the ordinate of Fig. 2*b*.



Sampling Scheme.

First Sample 3; Accept if 0 defects.
Retest sample of 5 if 1 defect.
Reject if 2 or more defects.
Second Sample 5; Accept only if 0 defects.

FIG. 4.
Illustrating Result of a Double Sampling Scheme.

The examination of the second sample (of five) will result in the division of the re-test region into two: the whole figure then appears with one curve dividing the regions of acceptance and rejection (Fig. 4).

This curve has been called * the "Operating Characteristic of the Full Sampling Scheme"; the whole diagram details fully the results of the application of the scheme to a series of batches all at the same level of quality. To avoid confusion, this diagram will be called in this paper the "Sampling Diagram," and the dividing full-line curve the "Sampling Characteristic" of the scheme.

The Sampling Characteristic of any scheme can be calculated and drawn.

In practice it is usual for the consumer to lay down specification limits or tolerances and to institute a system of inspection to confirm that these are being kept. If the article be a really critical one, it may be important that no defective article be accepted. In such a case the consumer will probably call for 100 per cent. inspection; however, this may be impossible because the test is a destructive one—*e.g.*, functioning of fuses, ultimate tensile strength, life of lamp filaments, etc. Some scheme of rational sampling is then essential. In less critical cases a sound scheme of selective inspection may be quite sufficient, with advantages over the more costly complete examination.

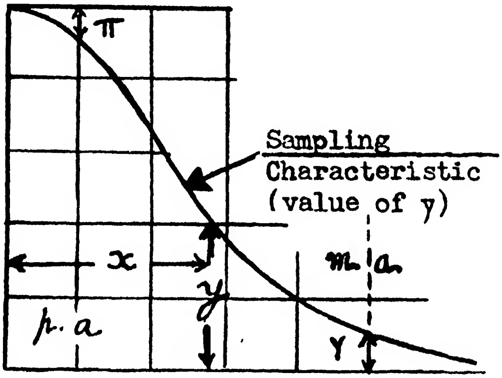


FIG. 5.

In examples where the highest level of quality is not essential, it is often agreed that it is preferable to accept a large quantity of fairly good material rather than a smaller amount of perfect material. This is a common state of affairs, and represents a compromise between the consumers' requirement for quantity and the manufacturers' difficulties with precision work. But such a compromise must incorporate some control of quality, and it is usual for the consumer to set some limit to the percentage defective. This has been called "lot tolerance percentage defective" by Dodge-Romig,† and more simply "maximum allowable" by Swan.‡ For example, the value of this maximum allowable (m.a.) percentage may be 1 per cent., and the consumer will then be satisfied if not more than 1 per cent. of a batch of articles is defective.

To attain this, it is clear that in order to avoid frequent rejection the producer must work to an average quality level—a "process average": † (p.a.) less than (m.a.). Later consideration will show that the ratio p.a./m.a. is of considerable importance in any sampling system, and should be as low as possible—a desirable but insufficient condition to the manufacturer.

The Producer's Risk is usually associated with the process average and the Consumer's Risk is associated with the "maximum allowable." This is true only under idealized conditions, which do not arise in practice. It is shown below that the former is a useful approximation, but that the latter may be misleading.

If the proportion defective in any batch of a manufactured article is exactly equal to the "process average," then the Sampling Characteristic shows the *chance of rejection* of such a batch. This chance or probability is the proportion of batches of this quality which would be rejected under the scheme. Further, this chance is a measure of, and can be called; the Producer's Risk, π, since all these batches, whether accepted or rejected, are of the same quality, and satisfactory to the consumer if fuller inspection were carried out. (Fig. 5.)

* "Sampling Inspection," by H. Rissik," *Aircraft Engineering*, May 1943.
† "Sampling Inspection Tables," *Bell Telephone Technical Journal*, January 1941.
‡ "Sampling Schemes for Qualitative Inspection," *Institution of Mechanical Engineers*, December 17th, 1943.

Similarly, if the proportion defective in any batch of a manufactured article is just greater than the "maximum allowable," then the Sampling Characteristic shows the *chance of accepting* such a batch. This chance or probability is the proportion of batches of this quality which would be accepted under the scheme, and clearly is a measure of, and can be called, the Consumer's Risk, γ.

It is important to note that the Producer's Risk is π only if *all* batches are of p.a. quality, and the Consumer's Risk is γ only if *all* batches submitted are just below m.a. quality. Now, these are unlikely conditions in practice. No process is likely to produce ten defects in every thousand. If the process average, determined over a long period, is indeed 1 per cent., then some batches of 1,000 will contain 10 defects, but others will contain 9, 11, 8, 12, 7, etc., defects.

In other words, from batch to batch there will always be an inevitable swing about the process-average value. This is inherent in any process, and requires greater investigation than it has hitherto received.

The nature of this variation in quality from batch to batch about an average quality has an important influence on the choice of a sampling scheme.

Most concerns will have sufficient data recorded to be able to investigate batch-to-batch quality variations. Thus, if batches consist of 1,000 articles, then past records will show the number of defective articles found in each batch. A histogram can then be constructed by compiling the results in the well-known "cricket-score" method. (See Fig. 6.)

Thus a run of 5, 6, 3, 6, 7, 4, 9, 8, 6, 6, 7, 3, 6, 5, 9, 7, 6, 10, 6, 5, 8, 7, 8, 5, 9, 6, 8, 7 defectives in successive batches gives the following:—

Defectives in batch	No. of batches	
3	//	= 2
4	/	= 1
5	////	= 4
6	~~////~~ ///	= 8
7	~~////~~	= 5
8	////	= 4
9	///	= 3
10	/	= 1
Totals :	Batches	= 28
	Defectives	182

∴ Estimated process average (p.a.) = 182/28,000 = 0·65%.

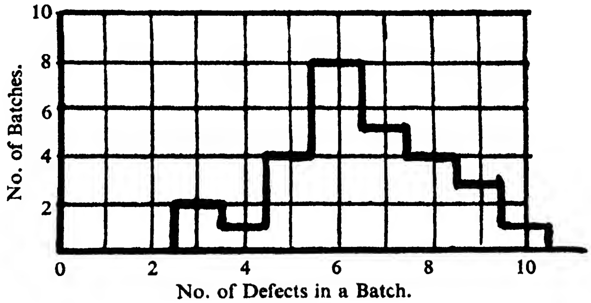


FIG. 6.
Process Characteristic.

The greater the number of results available, the better the estimate of the variability in quality from batch to batch, provided that all the results used are associated with similar processing and, as far as possible, are representative of *anticipated future production*. If many results are available it may be possible to fit a curve to this histogram: such a curve has been called the Operating Characteristic, but a preferable name is the Process Characteristic.

It should be remarked that the mathematics used for determining such a curve are not simple. The fitting of a curve, however, is not essential to the following argument. For the purpose of calculation, it is far simpler and better to treat results in the histogram or tabulated form. (See full stepped line in Fig. 7*a*.). The term Process Characteristic may be applied to the smooth curve, or the more fundamental histogram. Either is truly characteristic of the quality of production and gives the distribution of defectives in batches.

The chance of accepting or rejecting any batch submitted to the Sampling Scheme will depend on the proportion defective, and is given by the ordinate of the Sampling Characteristic appropriate to that proportion. To find the total chance of acceptance for the production represented by the Process Characteristic we must therefore add together, for all possible values of the percentage defective, the combined probability of:—

(*a*) a batch having a percentage defective, (x), (= z corresponding to x, see Fig. 7*a* and 7*b*) and

(*b*) of accepting such a batch whenever it is offered to the Sampling Scheme (= y corresponding to x, see Fig. 5).

This is, in effect, a weighting of the Sampling Characteristic by the Process Characteristic.

The probability associated with (*b*) is given by the ordinate y of the Sampling Characteristic at any value of x (Fig. 5), and the probability associated with (*a*) is given by the ordinate z of the Process Characteristic at the same value of x. (See Fig. 7.) The chance of accepting a batch of quality x is therefore the product yz, since z is the fraction of batches submitted and y the proportion of these accepted.

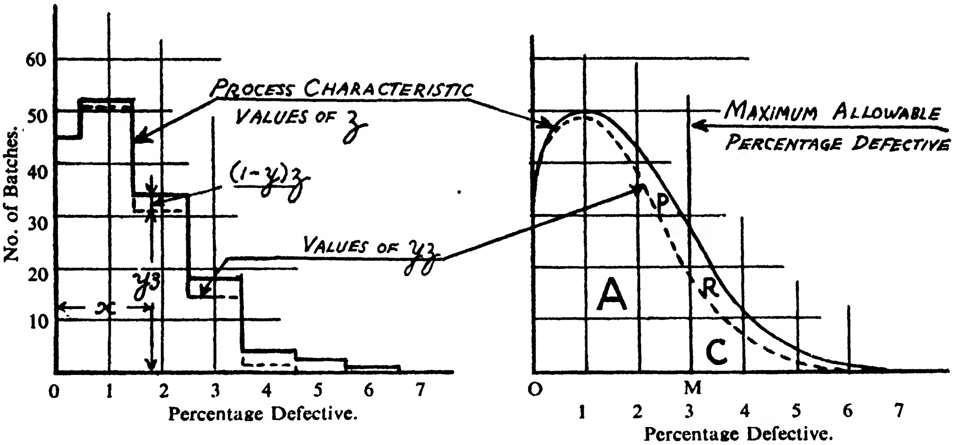


FIG. 7*a*. FIG. 7*b*.

Illustrating the Determination of Producer's and Consumer's Risks, for given Process and Sampling Characteristics.

The summation of yz for all values of x divided by the summation of z gives the total proportion of the production which will be accepted; the summation of $(1 - y)z$ similarly gives the proportion which will be rejected, since $(1 - y)$ is the chance of a batch of quality x being rejected.

The value of y is usually obtained from a smooth curve. If the value of z be given as a table or as a histogram, the values of yz will plot in a stepped form (Fig. 7*a*). On the other hand, if the value of z be plotted as a smooth curve, the products yz will plot as a smooth curve. (See Fig. 7*b*.)

In either case the area under the graph of yz represents the proportion of the production accepted, and the area between the graphs of yz and z represents the proportion rejected.

If on these graphs the position of maximum allowable (M) is drawn as an ordinate, the whole output (represented by the total area under the z curve) is divided into four categories corresponding to the areas A, C, R and P, shown in Fig. 7*b*, for the case of the smooth curves, but more simply calculated from the known true values.

That part of the Process Characteristic with x less than OM, namely, $A + P$, represents satisfactory product. Of this, A is correctly accepted by sampling and P wrongly rejected, because the broken curve is the boundary of acceptance and rejection. Thus, $P/(A + P + C + R)$ * could be called the Producer's Risk. It should be noted, however, that of the total production represented by the Process Characteristic, the area $P + R$ represents product rejected. Hence the ratio $(P + R)/(A + P + C + R)$ may be considered as the Ultimate Producer's Risk for the production represented by the Process Characteristic.

That part of the Process Characteristic with x greater than OM, namely, $R + C$, represents production which is unsatisfactory, and should be rejected. Nevertheless, C is accepted, and only R rejected; there is therefore a proportion C wrongly passing the Sampling Test. This is of vital interest to the consumer, and this proportion $C/(A + P + C + R)$ is logically termed the true or Ultimate Consumer's Risk (*UCR*) for the production represented by the Process Characteristic considered. [The consumer is accepting a risk of a different kind, viz., the rejection of some satisfactory material.]

In practice the Process Characteristic (z curve) may not extend as far as $x = OM$—*i.e.*, no

* If the diagrams are prepared so as to have a total area = unity, then the risks are more simply referred to in terms of the areas P, R, etc.

product is made worse than maximum allowable. Under this condition there can be no risk to the consumer and there should be no rejections. On the contrary, from an examination of the Sampling Characteristic alone there will be values of the *so-called* Producer's Risk (π) and Consumer's Risk (γ) (Fig. 5). The value π is somewhat in error, for it assumes no spread in quality from batch to batch about the process average, and the value γ does not arise, for the simple reason that it is known that no batches are offered containing as many defectives as the maximum allowable.

Investigation has shown that, in general, the effect of allowing for the inevitable variations in batch-to-batch quality is to change slightly the Producer's Risk and to reduce considerably the Consumer's Risk (which may in some cases be zero). This illustrates the importance of the value of the ratio p.a./m.a. referred to earlier in the paper.

It is urged that the simplicity by which Process Characteristics or Data can be found from existing records should be utilized to investigate the characteristics of various processes. It is likely that certain mechanical processes will have their own types of characteristic, and there is need for investigation of this.

The author has investigated the effect of Process Characteristics derived from the Poisson Distribution, which has some theoretical justification, but the main point emphasized here is that the principle is sound for any type of characteristic. The efforts of research workers could beneficially be directed to the determination of process characteristics in practical cases and to tracing the effects upon the whole question of economic Sampling Inspection schemes.

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TO THE SUPPLEMENT TO THE JOURNAL OF THE ROYAL STATISTICAL SOCIETY

VOL. VIII, 1946.

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SUPPLEMENT TO THE
Journal of the Royal Statistical Society
Vol. IX, No. 1, 1947

On the Interdependence of Blocks of Transactions*

By Richard Stone

[Read before the Research Section of the Royal Statistical Society, December 5, 1946, Dr. J. Wishart in the Chair]

1. *Introduction*

It is a common experience of investigators in applied economics that the amount of information available to test any particular theory is limited, and does not provide the variety of experience necessary for deciding a point at issue. For historical reasons data of the required degree of complexity and reliability are frequently available only for a comparatively short period, though much energy is nowadays devoted to extending the scope of such data. In a short period, say twenty years, an economic system may not perform crucial experiments of the type needed for testing particular hypotheses; the similarity of the movements exhibited by different parts of the economy may greatly restrict the conclusions which may validly be drawn from a given body of data. This state of affairs is manifested in the fact that the variation of a particular variable, say the quantity of a given commodity consumed, can often be explained with only a few of the predictors that would seem to be necessary on theoretical grounds. These predictors could, conceptually, have considerable independent fluctuations, but in fact within the available short experience they do not; to a close degree of approximation, unexpectedly simple relationships subsist between them. It will be useful to have a technique for uncovering these short-run relationships, whether they are the expression of general economic or statistical laws, or explainable short-run tendencies or of anything else.

This particular small sample difficulty is nowhere more important than in the field of macro-economics, that is to say that part of economics which seeks to explain the variations in total activity as opposed to variations in the behaviour of a single entity. In such investigations the variables in our systems of stochastic simultaneous equations consist of large blocks of transactions, such as the national income, consumers' expenditure, capital formation and the like, and other average or aggregate series, such as the level of prices, the rate of interest and the stock of capital equipment of all kinds. Some further subdivision may be adopted; for example, income may be subdivided by type of income payment or capital formation may be broken down into producers' durable equipment, building and the net change in inventories, but even so, the variables are aggregates or averages in which much of the individuality of the component series is lost.

Instead of simply noting the interdependence of this kind of variable we may select a set of such variables for analysis, and see how far it is possible to reconstruct their movement from a small number of common factors. These factors are not themselves observed, but are constructed with a view to explaining the observed variables. If we have n variables and m factors and assume linear relationships, this hypothesis is expressed in a system of n equations of the form—

$$x_j = \sum_s a_{js} F_s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where the x_j, $j = 1, \ldots n$, are the observed variables and the F_s, $s = 1, \ldots m$, are the hypothetical factors. If there is a high degree of intercorrelation in the movement of the variables, we may expect to be able to explain most of the variance of the x_j in terms of a number of factors which is small compared with the number of variables.

This position may be expressed differently as follows: Suppose $m = 1$; then all the x_j would move in exactly the same way and could be classified by one criterion of classification, the ampli-

* I should like to record my indebtedness to my friend, H. R. Fisher, who was kind enough to read this paper in draft and suggest a number of improvements. In particular he suggested the construction of a spherical correlation map, which seems to me to bring out so clearly the essential features of factor analysis.

tude of their movement. If the x_j required two (independent) factors for their explanation we could not order them by one criterion of classification; we should need two independent criteria. In this case the x_j might move very differently, since in any given x_j the weights of the two factors (the a_{js}) might be very different.

The statistical technique of factor analysis is designed to deal with precisely this problem. This method has been developed and applied largely by psychologists for analysing the structure of abilities reflected in batteries of mental tests, and a number of different procedures have been put forward and applied by different investigators. These differences are largely attributable to alternative views of the kind of structure of hypothetical mental abilities that different investigators consider to be reasonable on theoretical grounds.

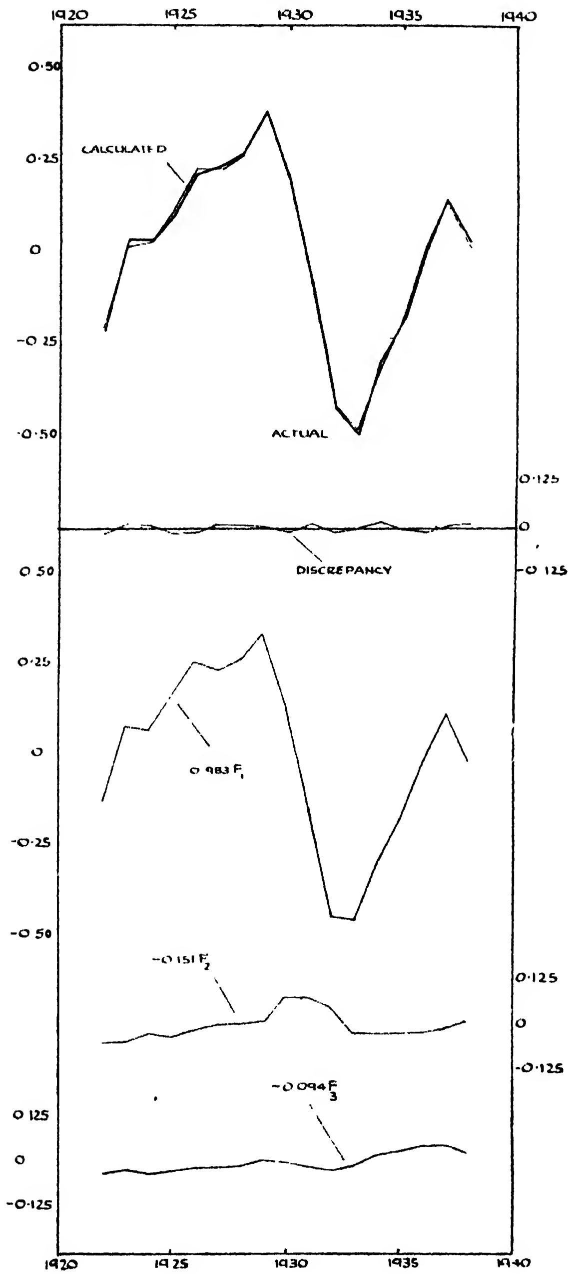
The method used here is due to Hotelling,* and has the advantages first that the hypothetical variables or factors in terms of which the observed variables are explained are orthogonal, and second that each factor accounts for as much of the remaining total variance of the observed variables as possible. In principle n variables observed on a large number of occasions can be explained completely (in the absence of exact linear interdependence between them) in terms of just n factors, but where the intercorrelations of the variables are high most of this combined variance can be explained by a number of factors which is small compared with n.

In geometrical terms the problem may be set out briefly as follows: our data consist of measurements in the form of variations from means of n transaction blocks for each of N years, which the reader accustomed to psychological applications may think of as tests and persons respectively. We set up orthogonal axes along which the transaction blocks are measured and in the resulting n-space each year may be represented by a point. Let us take the case where $n = 3$ and consider the configuration of the N points in the three-dimensional transaction space. If we express the deviations in standard form, then with independent transaction blocks each distributed according to the normal law, the points representing the N observations will tend as N increases to take the form of a spherically symmetrical swarm with density highest at the intersection of the axes, i.e. at the mean values of the transaction blocks and falling off evenly in all directions from the centre. If the transaction blocks are correlated the spherical distribution will be replaced by an ellipsoidal one. And, to take an extreme case, if the variables could be represented exactly by a single factor F_1, the swarm would be confined to a straight line, for the co-ordinates of a point of the swarm, being $a_{11}F_1$, $a_{21}F_1$, $a_{31}F_1$, would always be in the proportions $a_{11} : a_{21} : a_{31}$. Along the radius vector defined by these proportions the N points would be spaced according to the corresponding values of F_1. If now most, but not all of the variation of the observations is explainable by a single factor, the swarm is elongated but not purely linear; and the projections of the N points on to a line forming the core of the swarm constitute the first factor representations of the data. In Hotelling's method this core is chosen so as to minimize the sum of squares of residues, and the choice is found to fall on the first principal axis of a certain ellipsoid. Let now the variation due to this factor be removed. Points corresponding to the residues form the two-dimensional swarm got by collapsing the three-dimensional swarm in the direction of the first principal axis. Variability within the residues may be chiefly describable by distance along the core of this two-dimensional swarm, which, located by a similar least-squares method, is the second principal axis of the original ellipsoid. If the two-dimensional swarm is collapsed parallel to this axis there is left the remaining variability concentrated along the smallest principal axis of the ellipsoid; this corresponds to a third factor which exhausts the data. Corresponding to the orthogonality of the axes there are certain algebraical relations of orthogonality between the coefficients a_{js}; also the product moment of the swarm referred to any pair of the principal axes is zero (the principal factor axes being in fact the same as the principal axes of inertia of the swarm), and so the factor values, being proportional to the co-ordinates of the N points with respect to the principal axes, are uncorrelated, or orthogonal.

This geometrical representation is merely an extension of the familiar correlation scatter diagram. While convenient in introducing the subject, it has not the analytic power of the

* See "Analysis of a Complex of Statistical Variables into Principal Components," by H. Hotelling in *Journal of Educational Psychology*, 1933, pp. 417–41 and 498–520. Descriptions of this method can be found in *Factor Analysis* (1941) by Holzinger and Harman, especially in Chapter VII and Appendix D, and in *The Factorial Analysis of Human Ability* (1939) by G. H. Thomson, especially in Chapter V and Note 7 of the mathematical appendix.

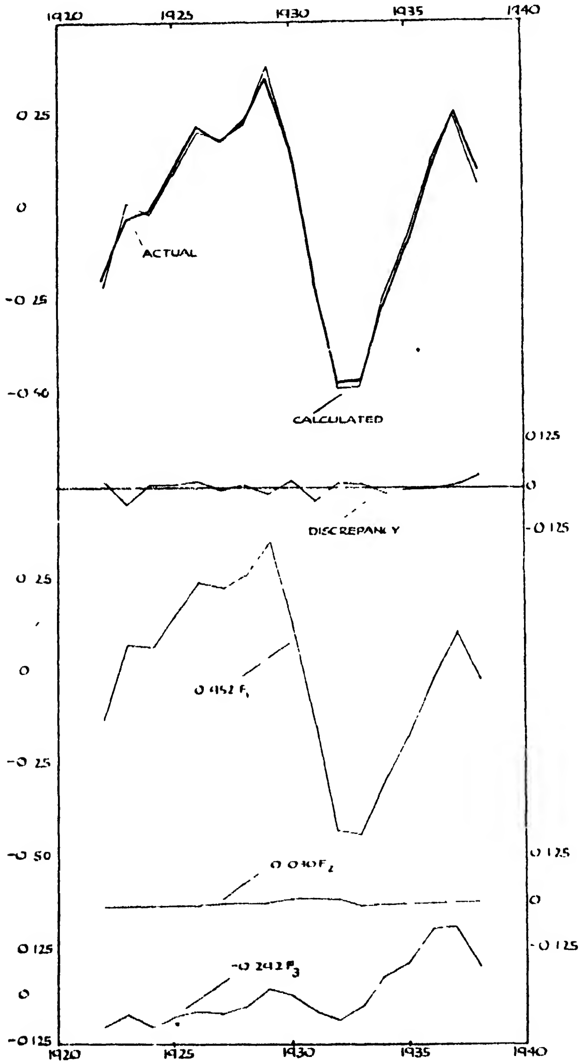
DIAGRAM 1
Employees' Compensation



obverse representation, more prominently used in works on factor analysis, in which there would be one point for each of the n transaction blocks subsisting in a space of N dimensions. If we consider only normalized variables, i.e. those with sum of squares equal to unity, it can be seen that

DIAGRAM 2

Consumers' Perishable Goods *plus* Producers' Durable Goods



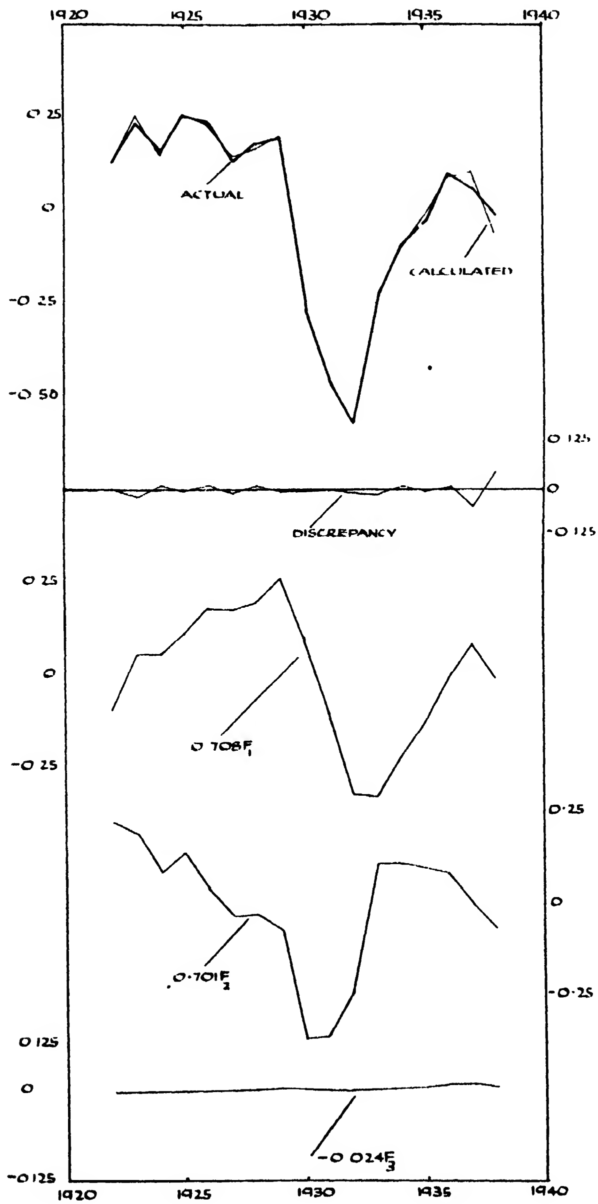
each variable will lie on the surface of an N-sphere, and that each point on this surface will correspond to a possible mode of variation over the period of N time units. If the observed variations of the n variables can be represented approximately in terms of m factors, it will be possible to find an m-sphere the surface of which approximately contains the n points. If, as in the present case, the set of variables is nearly describable by three factors, it is possible to represent them on the surface of a sphere. This representation (a model of which will be available for inspection

when this paper is read) shows conveniently the intercorrelations of the variables (or rather their three-factor representations), the affinities of these representations with the three principal or other factors and the possible good regression equations between them.

The sphere is imagined embedded in N-space, and to each of the n variables there corresponds a point on the surface of the sphere indicating the three-factor representation of the variable.

DIAGRAM 3

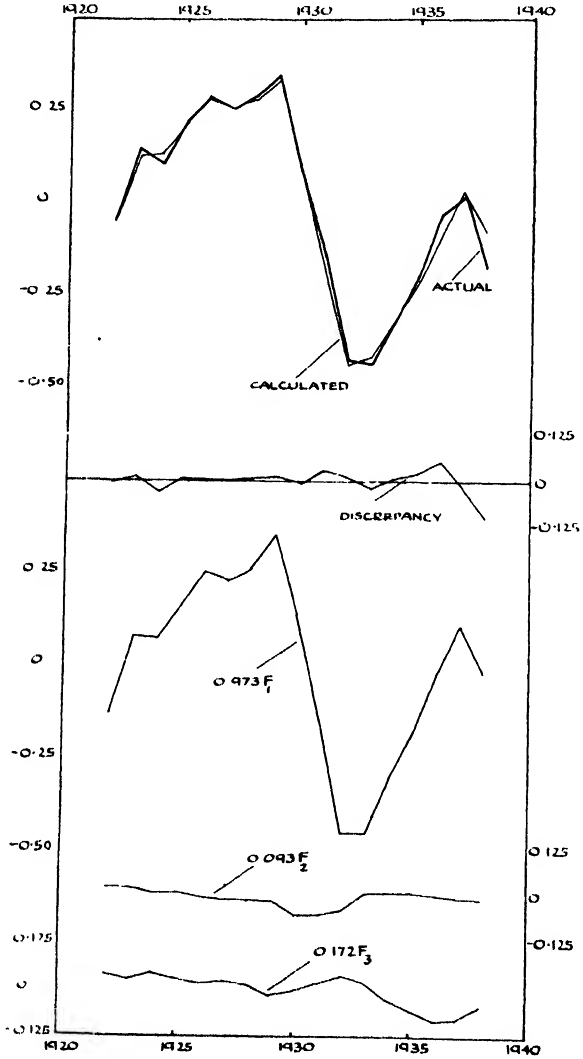
Unadjusted Net Savings of Enterprises *plus* Adjustment for Capital Revaluation



These points are the radial projections of the projections into 3-space of the true points in N-space. The distances between the projections of each variable on to the unit N-sphere and those on to the unit 3-sphere are proportional to $\sqrt{2(1 - R_{j\cdot st\ldots})}$, and are represented by circles with this radius drawn round the points representing the variables on the 3-sphere.

DIAGRAM 4

Consumers' Semi-durable Goods *plus* Consumers' Durable Goods

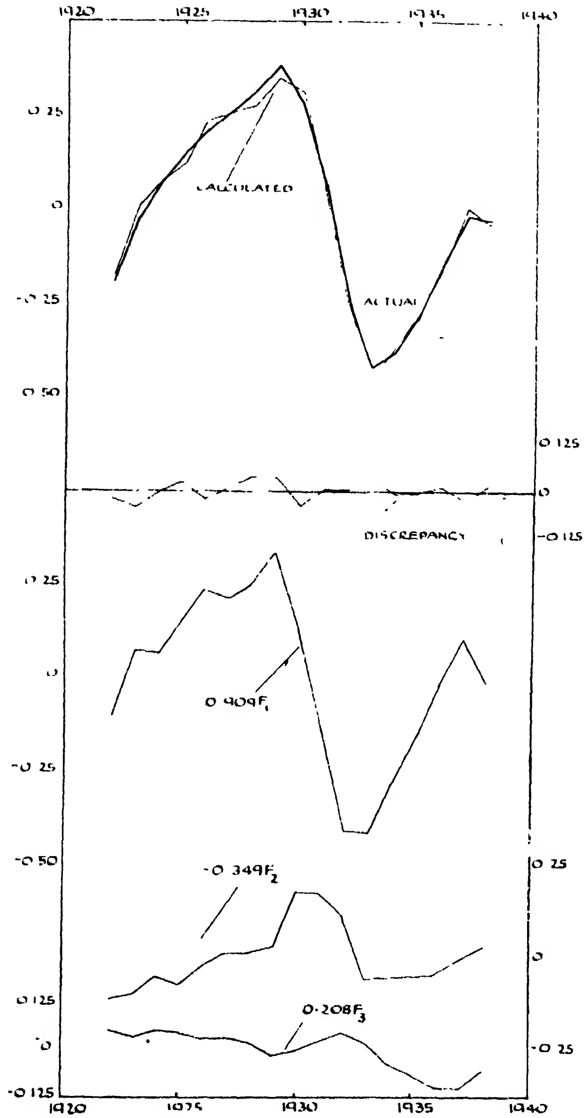


The surface of the 3-sphere may also contain three orthogonal points representing the first three factors or principal components of variation of the variables; indeed it is with respect to these three points that the position of the n points representing the variables is determined. It is also possible to put on to the surface of the sphere the three-factor representation of any other variables, for example, those which are thought to be useful in explaining the observed variation

of the n variables. It will be seen in this example that the point corresponding to the three-factor representation of total income over the period lies very close to the point representing the first factor F_1. This indicates that total income and F_1 were highly correlated over the period.

DIAGRAM 5

Consumers' Services



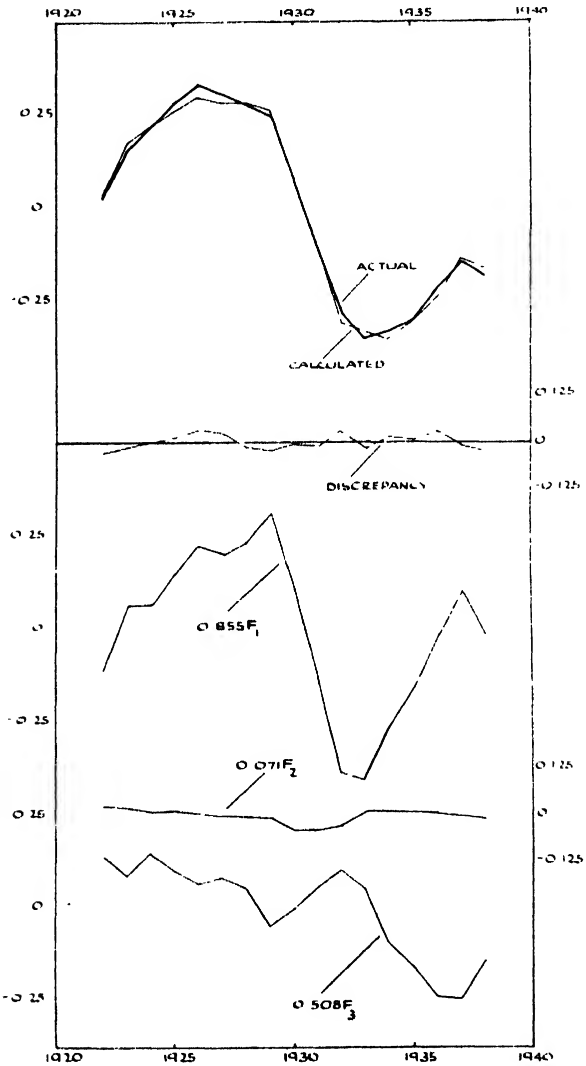
In many psychological applications of factor analysis the calculations are made with standardized variables, i.e. correlation coefficients rather than sums of squares and products form the basis of the analysis. The main reason for this is the difficulty of finding a common unit of measurement in which to express the scores in the different tests. This problem is not present in

the application here since all the transaction blocks are measured in a common unit, money. Accordingly no attempt is made here to standardize the observed variables.

The use of sums of squares and products does not complicate the analysis and indeed simplifies the arithmetic, but it does have the following effect: The variances of the seventeen observed

DIAGRAM 6.

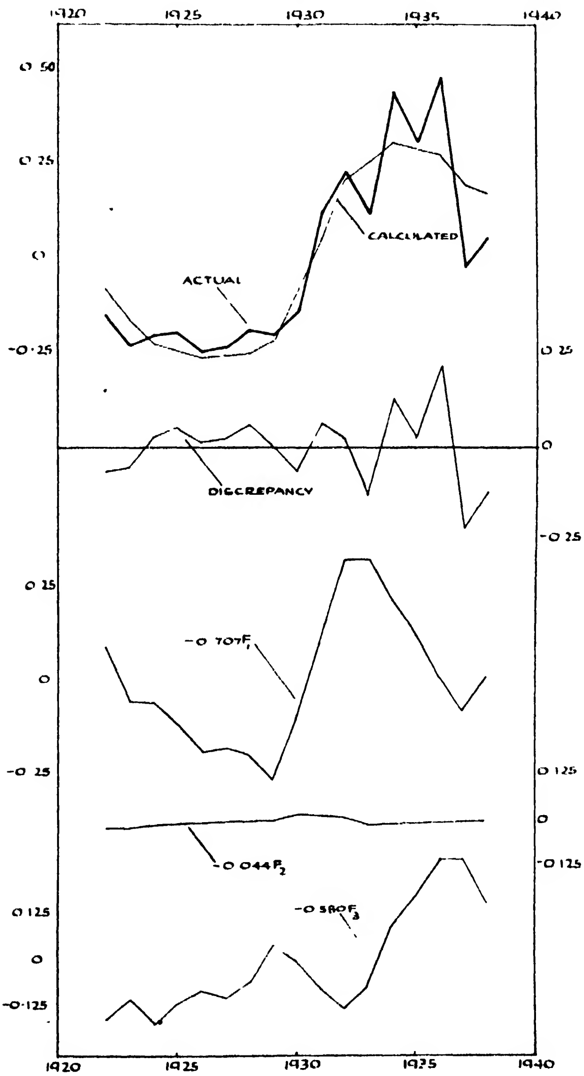
Construction.



variables differ widely, so that a large part of the combined variance may be explained by a factorial representation in which very little of the variance of the smaller variables is explained. In fact this does not occur to any important extent in the present case. On the other hand, if all the variables were standardized, the very small ones would play as important a part in determining the outcome of the analysis as would the large ones. This is especially undesirable where, as in the present case, the particular transaction blocks used depend on the way in which the available

series are presented rather than upon a grouping designed on theoretical grounds for this particular type of analysis. In some respects this application is rather like the earlier work in psychology in which such test material as came to hand was subjected to analysis. In more recent psychological studies the test material is carefully selected so that the varied activities of the

Diagram 7
Net Public Outlay

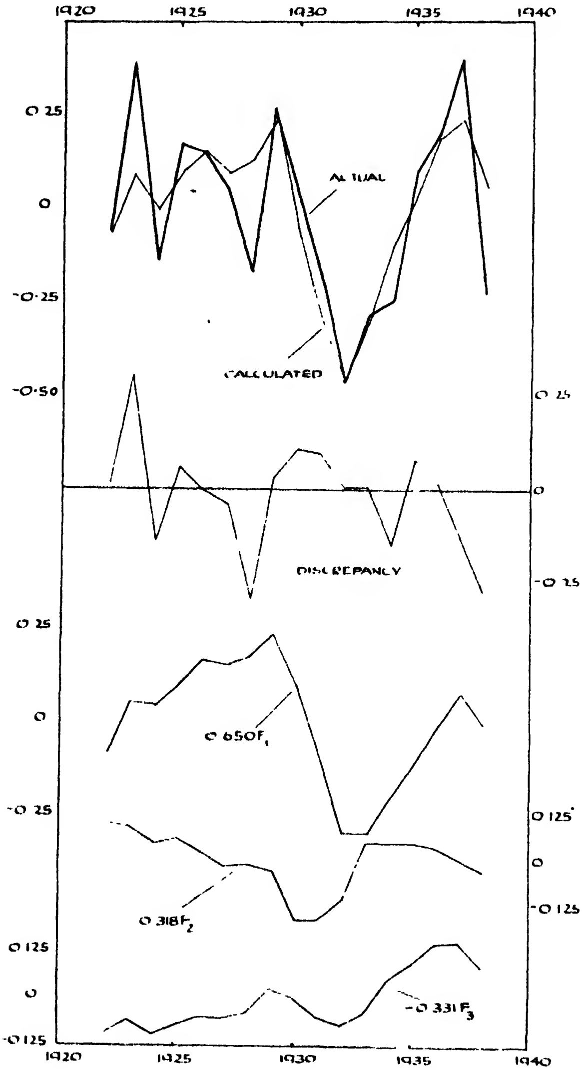


mind may be fairly represented. The corresponding refinement in the present case would consist in a careful *a priori* grouping of the transactions. In addition it would be appropriate to weight the data by expressing each transaction block in units of its estimated margin of error. For the approximate factorial representation is found by minimizing the sum of squares of residues, and if the analysis is taken far enough the residues will be largely drawn from the errors of estimation.

From examination of the manner in which the data have been compiled one may have legitimate prejudices on the relative sizes of the residues that may be ascribed to errors (or to accurately recorded but irrelevant fluctuations) in the different transaction-blocks. It is a distortion of

DIAGRAM 8

Net Increase in Inventories



purpose to "chase" an unreliable variable as strongly as one with a smaller absolute margin of error. On the other hand, if a few factors suffice to catch all the variables nearly enough, weighting the data can make little difference to the final result, though the order in which the variables get absorbed may be somewhat affected.

In this investigation an answer based on an examination of data for the United States over the years 1922–38 will be given to the following questions:

First, how far can the variance of a number of transaction-blocks be satisfactorily explained in terms of a small number of factors?

Second, if it is found that most of the total variance can be explained by a few factors, how far does this satisfactory result extend to the individual variances of the observable variables? Specifically, how far are the variables with small variances adequately explained?

Third, is it possible to identify the factors with series estimated from economic considerations and not from the factor-analysis itself?

Finally, something will be said on the broader possibilities of factor analysis in relation to econometric work.

2. *The Data*

This analysis is based on Messrs. Kuznets' and Barger's data* for the components of total income and outlay of the United States over the seventeen years 1922–38. For another purpose† these components had, subject to a small amount of prior aggregation, been correlated by pairs and amalgamated where the correlation coefficient exceeded 0·95. As a result the sums of squares and products of the series shown in Table I were available.

TABLE I.—*Components of Income and Outlay in the United States of America* 1922–1938

Variable.	Sign in Income or Outlay.	Component.	Standard Deviation (milliards of dollars).
1	i	Employees' compensation	6·16
2	o	Consumers' perishable goods *plus* producers' durable goods	4·40
3	i	Unadjusted net savings of enterprises *plus* adjustment for capital revaluation	3·95
4	o	Consumers' semi-durable goods *plus* consumers' durable goods	3·70
5	o	Consumers' services	2·86
6	o	Construction	2·80
7	o	Net public outlay	1·65
8	o	Net increase in inventories	1·64
9	i	Inventory revaluation adjustment	1·56
10	i	Net rent received by individuals	1·41
11	i	Entrepreneurial withdrawals	1·36
12	i	Dividends	1·07
13	– o	Adjustment for depreciation	0·90
14	i	Interest	0·51
15	i	Dividends, interest and non-commercial remittances from abroad *less* direct taxes paid by individuals *plus* veterans' bonus *plus* social security benefits *less* employees' social security contributions	0·51
16	– i	Adjustment for depreciation and depletion	0·35
17	o	Foreign balance including foreign tourist expenditure in the United States	0·33

* See *Outlay and Income in the United States 1921–38* (1942), by H. Barger, Table I, pp. 42–3, Table III, pp. 50–1, Table IV, pp. 58–9 and Table V, pp. 62–3. As can be seen from the footnote to Table IV, all the components of income apart from some small adjusting items are taken from Kuznets' work. See *National Income and its Composition* (1941).

† An application of H. R. Fisher's method of adjusting the components of the national accounts when systematic errors are believed to be present. This method and the numerical example referred to here will shortly appear in *The Review of Economic Studies.*

TABLE II.—*Sums of Squares and Products of Deviations from Means*

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.
1	644·6372	444·8425	245·4493	357·8750	277·1715	229·2911	−109·7995	105·6475	65·7008	94·2060	129·3948	101·7941	−88·7345	6·9204	−25·4968	−28·4633	6·0437
2	..	328·4562	195·7565	239·4492	174·2133	139·2525	−63·4836	82·9824	30·6797	54·2279	81·4011	70·2440	−57·2135	0·4274	−20·1256	−19·9194	1·1967
3	..	..	265·5798	185·3072	76·1139	121·3496	−56·3617	72·1874	−39·5188	60·1596	43·5974	24·0273	−23·5008	−20·9740	−11·2230	−18·8279	2·6127
4	..	..	..	232·4089	159·1952	163·8661	−79·2040	66·0683	23·5428	74·6707	78·0456	52·3214	−48·1411	−1·7816	−10·7240	−18·2502	4·9308
5	..	..	..	..	138·6140	116·7781	−59·1671	30·2178	43·6146	50·0104	64·7994	44·3500	−42·4528	8·1846	−8·5543	−11·1009	5·8334
6	..	..	..	..	..	133·7405	−69·7517	31·9157	20·9564	64·5282	59·0639	29·4146	−33·5950	−1·3663	−5·8774	−12·8199	7·4335
7	..	..	..	..	..	..	46·5015	−15·5738	−10·5122	−35·7807	−31·6571	−11·9940	17·7324	1·2314	6·6792	7·1340	−5·3486
8	..	..	..	..	..	..	..	45·5014	−2·8340	14·1815	15·5672	17·1872	−10·5447	−4·2766	−4·8709	−6·4016	−3·6058
9	..	..	..	..	..	..	..	..	41·3430	7·2641	18·0318	14·0425	−13·6389	8·0295	−0·3799	−0·7940	1·4649
10	..	..	..	..	..	..	..	..	..	33·7052	26·7798	10·4798	−13·9532	−2·3397	−2·5495	−6·3714	4·4656
11	..	..	..	..	..	..	..	..	..	..	31·3837	19·5859	−19·7011	2·5045	−4·6429	−5·8665	3·3714
12	..	..	..	..	..	..	..	..	..	..	..	19·4518	−14·2525	3·7519	−2·7763	−3·1599	−0·2726
13	..	..	..	..	..	..	..	..	..	..	..	..	13·6865	−2·7137	3·5279	3·5120	−1·7287
14	..	..	..	..	..	..	..	..	..	..	..	..	..	4·3806	0·6119	1·2992	0·0928
15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4·3639	1·6542	−0·6323
16	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2·0576	−0·6053
17	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·8912

TABLE III.—*Correlation Coefficients*

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.
1	1·0000	0·9667	0·5932	0·9246	0·9272	0·7809	−0·6342	0·6169	0·4025	0·6391	0·9097	0·9090	−0·9447	0·1302	−0·4807	−0·7815	0·1731
2	..	1·0000	0·6628	0·8667	0·8165	0·6644	−0·5137	0·6788	0·2633	0·5154	0·8018	0·8788	−0·8533	0·0113	−0·5316	−0·7662	0·0480
3	..	..	1·0000	0·7459	0·3967	0·6439	−0·5072	0·6567	−0·3771	0·6359	0·4775	0·3343	−0·3898	−0·6149	−0·3297	−0·8054	0·1166
4	..	..	..	1·0000	0·8870	0·9295	−0·7619	0·6425	0·2402	0·8437	0·9138	0·7782	−0·8536	−0·0558	−0·3367	−0·8346	0·2352
5	..	..	..	..	1·0000	0·8577	−0·7370	0·3805	0·5761	0·7317	0·9825	0·8541	−0·9747	0·3321	−0·3478	−0·6573	0·3603
6	..	..	..	..	..	1·0000	−0·8845	0·4091	0·2818	0·9611	0·9117	0·5767	−0·7852	−0·0564	−0·2433	−0·7728	0·4674
7	..	..	..	..	..	..	1·0000	−0·3386	−0·2398	−0·9038	−0·8287	−0·3988	0·7029	0·0863	0·4689	0·7293	−0·5704
8	..	..	..	..	..	..	..	1·0000	−0·0653	0·3621	0·4120	0·5777	−0·4225	−0·3029	−0·3457	−0·6616	−0·3887
9	..	..	..	..	..	..	..	..	1·0000	0·1946	0·5006	0·4952	−0·5734	0·5967	−0·0283	−0·0861	0·1657
10	..	..	..	..	..	..	..	..	..	1·0000	0·8234	0·4093	−0·6497	−0·1926	−0·2102	−0·7651	0·5593
11	..	..	..	..	..	..	..	..	..	..	1·0000	0·7927	−0·9506	0·2136	−0·3967	−0·7300	0·4376
12	..	..	..	..	..	..	..	..	..	..	..	1·0000	−0·8735	0·4064	−0·3013	−0·4995	−0·0449
13	..	..	..	..	..	..	..	..	..	..	..	..	1·0000	−0·3505	0·4565	0·6618	−0·3398
14	..	..	..	..	..	..	..	..	..	..	..	..	..	1·0000	0·1400	0·4327	0·0322
15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·0000	0·5520	−0·2201
16	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·0000	−0·3069
17	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1·0000

The components of income and outlay were independently estimated, and over the period total outlay was on the average about 3·4 per cent. greater than the income total. For the present purpose no attempt has been made to adjust the components so as to remove this discrepancy.

Denoting the sums of squares and products of any pair of series x_j and x_k by m_{jk} we have—

$$m_{jk} = \Sigma x_j x_k = \Sigma(X_j - \bar{X}_j)(X_k - \bar{X}_k)$$

and these values are shown in Table II, while the correlation coefficients, not used in the analysis but of interest in themselves, are shown in Table III.

The matrix given in Table II, which will be denoted by $\mathbf{m} = [m_{jk}]$, forms the basis of the subsequent analysis as indicated in the following section which, while not intended as a complete or rigorous exposition of the Principal Factor method, will introduce the necessary algebraic conceptions.

3. *Method and Results*

In the first-factor representation of the data it is desired that each x_j, as it ranges through time, shall be as nearly as possible reproduced by $a_{j1}F_1$, where the coefficients a_{j1} are constants, one for each variable, and F_1 is the factor varying through time. The a_{j1}'s and the range of values of F_1 are to be chosen so that the sum of squares of residues for all variables through time, viz.

$$\sum_j \sum_t (x_j - a_{j1}F_1)^2$$

is a minimum; but since the coefficients and the factor values that produce the minimum are indeterminate to the extent of a constant that may multiply the a_{j1}'s and divide the values of F_1, the condition is imposed that F_1^2 shall have unit sum, that is the factor is sought in normalized form. The sets of minimizing equations with respect first to each a_{j1} and then with respect to each value of F_1 in the time series take the forms—

$$a_{j1} = \sum_t x_j F_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\text{and } \lambda_1 F_1 = \sum_j a_{j1} x_j \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where λ_1 is some constant, being $\sum_j a_{j1}^2$ plus the indeterminate multiplier introduced into the minimizing equations by the condition normalizing F_1. Actually this multiplier is zero, for on multiplying (2) by a_{j1} and using (3) we find that—

$$\begin{aligned} \sum_j a_{j1}^2 &= \sum_t \sum_j a_{j1} x_j F_1 \\ &= \sum_t \lambda_1 F_1^2 = \lambda_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4) \end{aligned}$$

Equation (2) means that the a_{j1}'s are the regression coefficients of the x_j's on F_1, and that the residues $(x_j - a_{j1}F_1)$ are for each j orthogonal to F_1. Equation (3) means that F_1 is a linear function of the x_j's, the coefficients being proportional to the same quantities a_{j1}. The value of λ_1 appears when F_1 is eliminated between (2) and (3), the result being the set of n equations—

$$\begin{aligned} \lambda_1 a_{j1} &= \sum_t \sum_j x_j a_{k1} x_k \\ &= \sum_j a_{k1} m_{jk} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5) \end{aligned}$$

Elimination of the a's shows that λ_1 must be a latent root of the matrix $\mathbf{m}$. When λ_1 has its proper value, equations (5) are consistent and determine the ratios between the a_{j1}'s, the values of which are then fixed by (4), save for an ambiguity of sign, which may be settled arbitrarily.

The sum of squares of residues, $\sum_t \sum_j (x_j - a_{j1}F_1)^2$ is less than $\sum_t \sum_j x_j^2$ by $\sum_j a_{j1}^2$, that is by λ_1, and therefore λ_1 is chosen to be the largest of the latent roots, which are necessarily all real and non-negative.

A second factor F_2 may now be taken out in the same way from the first residues. F_2 will be a linear function of these residues, and therefore orthogonal to F_1. Consequently a_{j2}, which by analogy with (2) is $\sum_t (x_j - a_{j1}F_1)F_2$, reduces to $\sum_t x_jF_2$, and so—

$$\sum_j a_{j1}a_{j2} = \sum_j\sum_t a_{j1}x_jF_2 = \sum_t F_1F_2 = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The second residues are orthogonal to F_2 (as the first were to F_1) and to F_1, because they are linear functions of the first residues. Therefore when F_3 is found as a linear function of the second residues it too is orthogonal to both F_2 and F_1. In this way it appears that each principal factor is orthogonal to the rest. Also, by repetition of the argument leading to (6) all the sets of coefficients are mutually orthogonal in the sense that—

$$\begin{aligned}\sum_j a_{jr}a_{js} &= 0 \text{ when } r \neq s \\ &= \lambda_s \text{ when } r = s \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)\end{aligned}$$

In the above process λ_2 is the largest latent root of the first residual matrix, of which the typical element is—

$$\sum_t (x_j - a_{j1}F_1)(x_k - a_{k1}F_1) = m_{jk} - a_{j1}a_{k1}\ ; \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and λ_3 is the largest root of the second residual matrix, and so on. It may be shown that λ_2, λ_3, etc., are respectively the second, third, etc., largest latent roots of $\mathbf{m}$. This is hardly of practical interest since the computor finds λ_2, λ_3, etc., via the appropriate residual matrices,* but it shows how the principal factors exhaust the data. Each factor F_s can be used to take λ_s from the sum of squares, and the characteristic equation for λ is so formed that $\sum_s \lambda_s = \sum_j m_{jj} = \sum_t\sum_j x_j^2$. Thus each of the n equations of the type—

$$x_j = a_{j1}F_1 + a_{j2}F_2 + a_{j3}F_3 + \ldots \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

is exact if all $m = n$ factors are included, and is an approximation—a regression equation—if the earlier factors alone are used. Equations such as (3), giving a factor value in terms of the data, are exact.

The coefficients of the factor pattern, the a_{js}, for the first three factors are shown below in Table IV, together with the actual sums of squares, $\sum_t x^2_j$, the calculated values, $\sum_s a^2_{js}$, ($s = 1$ to 3), and the variance ratios, $\sum_{s=1}^{s=3} a^2_{js}/\sum_t x^2_j = R_j{}^2{\cdot}_{st} \ldots$. The final row of the table shows the proportion of the combined original variance explained by each of the factors, i.e. $\sum_j a^2_{js}/\sum_t\sum_j x^2_j = \lambda_s/\sum\lambda$.

In Table V the pattern coefficients a_{js} are divided through by the appropriate $\sqrt{m_{jj}}$ yielding the correlations, r_{js}, between the variables and the factors.

Regression equations of the form of (9), the coefficients of which are shown in the above table, are illustrated in Diagrams 1–17. In these are shown the actual and calculated values of each x_j, the discrepancy between the two values and the contribution of each of the three factors to the calculated value.

As already mentioned, equations of the form of (3) above provide exact estimates of the F_s in terms of the x_j. In Table VI are shown the normalized factors calculated in this way. In each case $\sum_t F_s^2 = 1$.

* For a practical method of computation see Holzinger and Harman. *op. cit.*, Appendix D, and Thomson, *op. cit.*, Chapter V.

TABLE IV.—*Pattern Coefficients, Actual and Calculated Sums of Squares and Variance Ratios*

Variable.	Pattern Coefficients multiplying F_1, F_2 and F_3.			Sums of Squares.		
j.	a_{j1}.	a_{j2}.	a_{j3}.	Original.	Calculated.	Ratio.
1	24·9622	−3·8396	−2·3958	644·6372	643·5937	0·9984
2	17·2581	−0·5389	−5·2854	328·4562	326·0687	0·9927
3	11·5443	11·4291	−0·3839	265·5798	264·0424	0·9942
4	14·8296	1·4191	2·6247	232·4089	228·8191	0·9846
5	10·7049	−4·1033	2·4457	138·6140	137·4138	0·9913
6	9·8883	0·8251	5·8749	133·7405	132·9730	0·9943
7	−4·8183	−0·2970	−3·9568	46·5015	38·9605	0·8378
8	4·3872	2·1437	−2·2301	45·5014	28·8161	0·6333
9	1·9175	−5·4593	1·0209	41·3430	34·5229	0·8350
10	4·2928	1·0580	3·6537	33·7052	32·8966	0·9760
11	5·1436	−1·3238	1·6324	31·3837	30·8737	0·9837
12	3·7408	−1·6395	−1·0711	19·4518	17·8286	0·9166
13	3·3644	−1·3508	0·3001	13·6865	13·2336	0·9669
14	0·0002	−1·8108	0·0167	4·3806	3·2794	0·7486
15	−0·9592	−0·0186	0·4839	4·3639	1·1546	0·2646
16	1·2068	0·4268	0·1230	2·0576	1·6536	0·8037
17	0·2907	−0·0981	0·8570	1·8912	0·8286	0·4381
λ_s	1605·3132	210·5318	121·1148	1987·7030	1936·9589	0·9745
$\lambda_s / \Sigma\lambda$	0·8076	0·1059	0·0609	1·0000	0·9745	0·9745

TABLE V.—*Correlation Coefficients Between the Variables and the Factors*

Variable.	Factor. 1.	2.	3.
1	0·983	−0·151	−0·094
2	0·952	−0·030	−0·292
3	0·708	0·701	−0·024
4	0·973	0·093	0·172
5	0·909	−0·349	0·208
6	0·855	0·071	0·508
7	−0·707	−0·044	−0·580
8	0·650	0·318	−0·331
9	0·298	−0·849	0·159
10	0·739	0·182	0·629
11	0·918	−0·236	0·291
12	0·848	−0·372	−0·243
13	0·909	−0·365	0·081
14	0·000	−0·865	0·008
15	−0·459	−0·009	0·232
16	0·841	0·298	0·086
17	0·211	−0·071	0·623

TABLE VI.—*Normalized Factors* 1922–38

Year.	F_1.	F_2.	F_3.
1922	−0·1316	0·3258	0·2737
1923	0·0800	0·2837	0·1707
1924	0·0752	0·1380	0·2877
1925	0·1680	0·2084	0·2009
1926	0·2581	0·0667	0·1338
1927	0·2405	−0·0404	0·1561
1928	0·2733	−0·0324	0·1007
1929	0·3689	0·0916	−0·0795
1930	0·1424	−0·5228	−0·0015
1931	−0·1549	−0·5038	0·1261
1932	−0·4537	−0·3383	0·2130
1933	−0·4607	0·1583	0·1118
1934	−0·3111	0·1583	−0·1753
1935	−0·1817	0·1432	−0·3019
1936	−0·0132	0·1245	0·4676
1937	0·1195	0·0077	−0·4748
1938	−0·0186	−0·0866	−0·2733

4. *A Discussion of the Results*

We may begin by attempting to answer the three questions set out at the end of Section 1. First, it can be seen from Table IV that 97 5 per cent. of the combined variances of the seventeen variables can be explained by three orthogonal factors. The first factor accounts for 80·8 per cent. of the combined variances, the second for 10·6 per cent. and the third for 6·1 per cent., leaving only 2·5 per cent. to be accounted for by the remaining thirteen* factors that could be extracted.

Second, Table VII below shows the extent to which the individual variables are well explained by the three factors. The squares of the multiple correlation coefficients, $R^2_{j \cdot st} \ldots$, are cross classified by size and by the standard deviation of x_j, so that the extent to which the variation of the smaller variables is explained can readily be seen.

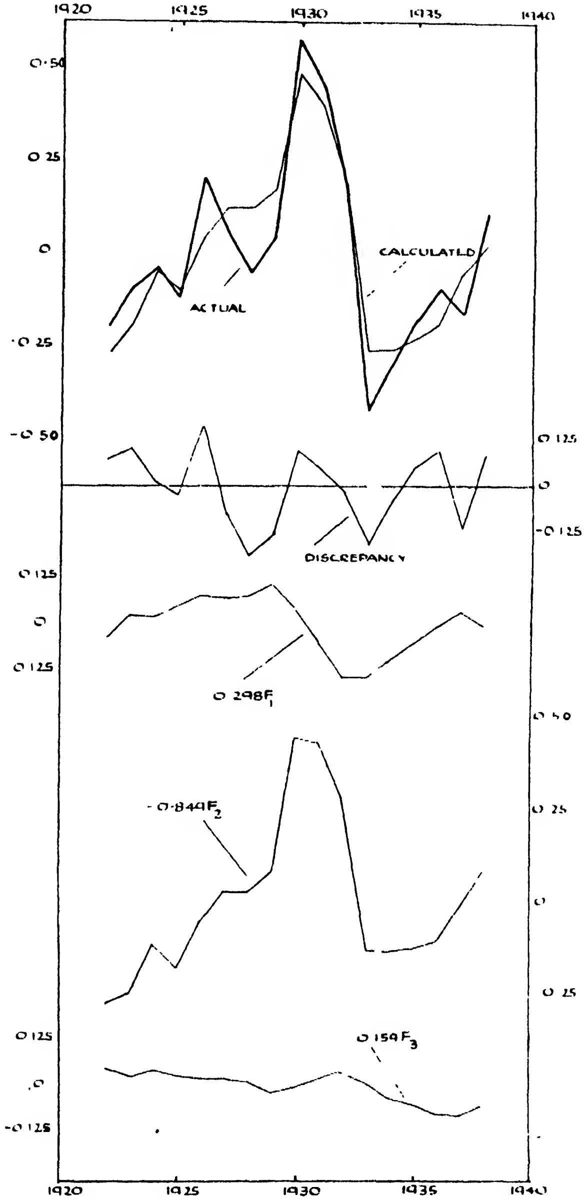
TABLE VII.—*Squares of Multiple Correlation Coefficients between Variables and Factors Classified by Size and by the Standard Deviations of the Variables*

	$\surd V(x_j)$.			
$R^2_{j.st\ldots}$	0–1·49.	1·50–2·99.	3·00–	Σ.
–0·499	2	—	—	2
0·50–0·749	1	1	—	2
0·75–0·949	2	2	—	4
0·95–	3	2	4	9
Σ	8	5	4	17

We may conclude from this table that although the variation of the smaller variables is not so well accounted for by the three factors as that of the larger variables, a fact which is not surprising, since sums of products and not correlations form the basis of the analysis and the individual variances show large differences, even this variation is accounted for to a high degree. In only two cases out of eight in the column containing the variables with the smallest variance is the

* The number of remaining factors is thirteen, making sixteen, not seventeen factors in all, because the number of years analysed is unfortunately small. Each variable is observed on 17 occasions only, and is brought into the analysis as a deviation from a mean. Of the 17 deviations of any one variable, only 16 are linearly independent. The matrix of data measured from means has rank 16 only, and so has **m**. One of the latent roots of **m** is zero.

DIAGRAM 9
Inventory Revaluation Adjustment



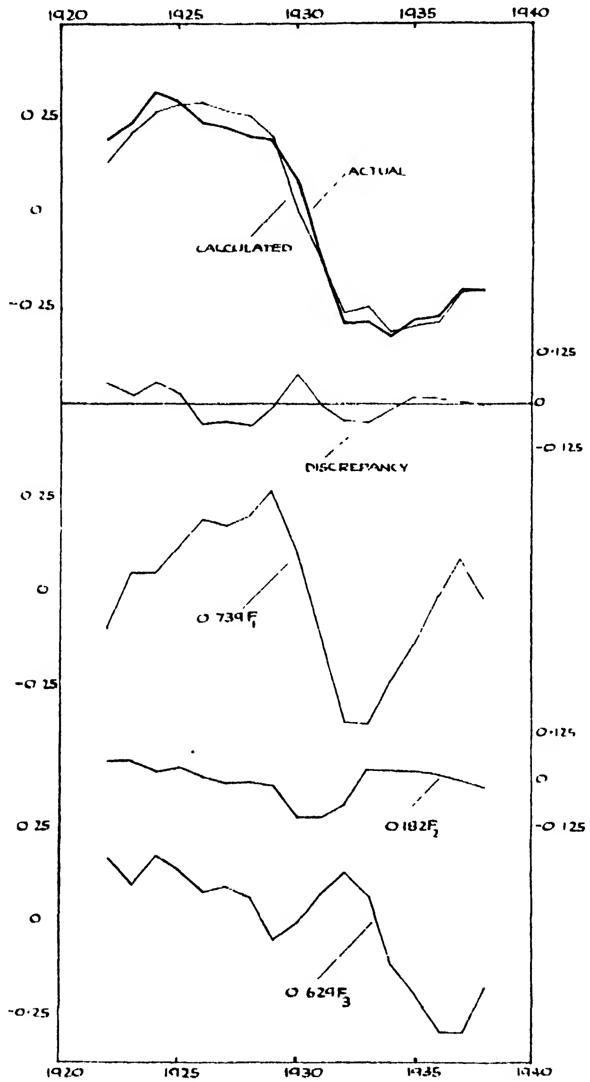
square of the multiple correlation coefficient, i.e. the proportion of the variance of the variable accounted for by the three factors, less than $\frac{1}{2}$. In just over half of all the seventeen cases R^2 exceeds 0·95.

A consideration of the composition of the series shows that the possibility of large systematic variation is not exhausted by taking out the variation due to three common factors. For example,

the variation of item 15, which is really a "rag-bag" composed of a miscellaneous assortment of small items, is largely explained by the three factors except for the year 1936, when a large veterans' bonus was paid out. Disturbances of this kind are well known to be an important element in economic variation, and it is not surprising, therefore, that all systematic variation is not exhausted,

DIAGRAM 10

Net Rent received by Individuals



by what can be accounted for by three factors. In this connection it is perhaps of interest to inspect the residuals of the $\frac{1}{2}n(n-1) = 136$ sums of products after the variability due to the three factors has been removed. A frequency distribution of these residuals which may be written $(m_{jk} - \sum_{s=1}^{s=3} a_{js}a_{ks})$ is shown in Table VIII together with the corresponding normal frequencies.

TABLE VIII.—*Frequency Distribution of Third Residual Sums of Products Compared with the Corresponding Normal Frequencies*

Range of Residuals.	Frequencies. Observed.	Frequencies. Based on normal curve with same mean and standard deviation.
−4·0 to −3·5	1	0·1
−3·5 „ −3·0	1	0·2
−3·0 „ −2·5	2	0·9
−2·5 „ −2·0	1	2·8
−2·0 „ −1·5	3	6·4
−1·5 „ −1·0	4	12·2
−1·0 „ −0·5	17	19·4
−0·5 „ −0·0	37	23·9
0·0 „ 0·5	33	24·3
0·5 „ 1·0	22	20·5
1·0 „ 1·5	8	13·3
1·5 „ 2·0	2	7·3
2·0 „ 2·5	1	3·2
2·5 „ 3·0	1	1·1
3·0 „ 3·5	1	0·3
3·5 „ 4·0	2	0·1
Σ	136	136·0

The mean of the residuals, 0·043, does not differ significantly from zero in spite of the fact that the sums of squares as opposed to products have not been included. It hardly requires an application of the χ^2-test to confirm the impression that the chances against the observed residuals being drawn from a normal population exceed 100 to 1. The distribution of the residuals has an undue proportion of relatively small and relatively large residuals, positive and negative, which may perhaps be attributed to minor systematic sources of variation not accounted for by the three factors. It must be recognized however that these figures are difficult to interpret, for they represent the sums of products of residues which in practice are almost certainly correlated. It is not clear, therefore, what distribution the residual sums of products would follow on the assumption that all systematic variation had been removed by the first m factors.

There remains the third question, namely, the interpretation of the factors. It was the expectation of the writer before the analysis was undertaken that most of the variation of the seventeen variables would be capable of being accounted for by the three components:

(*a*) Total income or some similar quantity.

(*b*) Rate of change of (*a*), and—

(*c*) A trend term representing the underlying tendency of the economy to expand or contract, or of its elements to do so relative to one another.

What is the relation of these concepts to the principal factors extracted from the data? It is important to remember that the individual principal factors are mere arithmetical abstractions, chosen for certain convenient algebraic properties. There is no general reason for identifying them singly with underlying causes. Any equal number of independent linear combinations of the factors could be used to give precisely the same factorial representation of the data. The important thing that has been demonstrated is that, save for small residues, nearly all the variables can be reproduced by a three-factor system, and there has been found that set of variate values of rank three which approximate to the data most nearly.* If the residues are neglected, the adequacy of (*a*), (*b*), (*c*) for the representation of the data may be judged by the extent to which they themselves fit into the same three-factor system.

* That the calculated three-factor system is the best three-factor approximation considered as a whole is not immediately obvious, since we have merely chosen each principal factor in succession to be that which, taken singly, reduces the sum of squares of residues as much as any one factor taken singly can reduce it. It may, however, be proved that no q-factor representation can reduce the sum of squares by more than the first q-principal factors reduce it.

Component (*a*) may be expected to fit in well, because it must itself be estimated by adding together several of the larger variables. There is a slight difficulty resulting from the imperfections of the data. The variables fall into two sets, of which the sums are total income, *i*, and total outlay, *o*, which would be identical if the estimates were perfect. However, the correlation between *i* and *o* is very high, $r_{io} = 0{\cdot}993$, so it will be convenient to refer to (*a*) as total income in what follows.

Table IX sets out the zero-order correlation coefficients between the six variables, F_1, F_2, F_3, *i*, Δi and *t*.

TABLE IX.—*Correlation Coefficients between the Factors and Certain Economic Variables*

	F_1.	F_2.	F_3.	i.	Δi.	t.
F_1	1	—	—	—	—	—
F_2	0	1	—	—	—	—
F_3	0	0	1	—	—	—
i	0·995	−0·041	0·057	1	—	—
Δi	−0·056	0·948	−0·124	−0·102	1	—
t	−0·369	−0·282	−0·836	−0·414	−0·112	1

The series for Δi used to obtain the correlations in this table is based on quarterly data which are not quite as complete as the data for *i* itself though they are compiled by the same author.* The figure for Δi at time *t* is obtained by subtracting the figure for *i* in the year ending at the end of the quarter $t - \frac{1}{2}$ from the corresponding figure for *i* in the year ending at the end of the quarter $t + \frac{1}{2}$. Since quarterly estimates of *i* are available for 1921 an estimate can be made of Δi for 1922. A corresponding estimate cannot, however, be made for 1938, so that the correlations involving Δi are based on 16 and not 17 observations.

The coefficients in the equations for the normalized values of *i*, Δi and *t* in terms of the factors are the correlation coefficients in the bottom left-hand quadrant of Table IX. The sum of the squares of these coefficients for each variable gives the proportion of the variance of that variable which is accounted for by the three factors. Thus, in the case of *i*, $R^2_{i\cdot 123} = (0{\cdot}995)^2 + (-0{\cdot}041)^2 + (0{\cdot}057)^2 = 0{\cdot}995$. Similarly $R^2_{\Delta i\cdot 123}$ and $R^2_{t\cdot 123}$ are equal to 0·919 and 0·915 respectively. Accordingly, the variables *i*, Δi and *t* fit reasonably well into the three-factor system and are represented on the spherical correlation map described above.

The figures in the bottom right-hand quadrant of Table IX are the zero-order correlation coefficients between *i*, Δi and *t*. The correlations between the three-factor representations of these variables can readily be derived from the coefficients in the bottom left-hand corner of the table, since, as already mentioned, these are the coefficients in the regression equations expressing the normalized form of the variables *i*, Δi and *t* in terms of the three factors. Since the factors are both normalized and orthogonal, so that $\sum_t F_i^2 = 1$ and $\sum_t F_iF_j = 0$, the correlation between the three-factor representation of any pair of variates is given by the sum of the products of the coefficients for like factors in the regression equations. Thus writing ρ for the zero-order correlation coefficient between the three-factor representations we have for example $\rho_{it} = (0{\cdot}995 \times -0{\cdot}369) + (-0{\cdot}041 \times -0{\cdot}282) + (0{\cdot}057 \times -0{\cdot}836) = -0{\cdot}403$ compared with a value of $-0{\cdot}414$ for r_{it}. Similarly $\rho_{i\Delta i} = r_{i\Delta i} = -0{\cdot}102$, while $\rho_{t\Delta i} = -0{\cdot}143$, compared with a value of $-0{\cdot}112$ for $r_{t\Delta i}$.

It would be possible to give a rather more complete treatment of these interrelationships through the calculation of ΔF_1, since this would make it possible to express the three-factor representations of the original data and the original data themselves in terms of F_1, ΔF_1 and *t*. It would also be of interest to attempt to find a factor Φ within the *F*-system, such that its rate of change Φ' is as nearly as possible within the *F*-system also. In the present application it was thought that these extensions were not worth while, since they would have involved very consider-

* Barger, *op. cit.* Table XVIII, pp. 179–83.

able additional calculation and the general conclusion that most of the variables, x_j, can be explained fairly accurately by an expression of the form—

$$x_j = ai + b\frac{di}{dt} + ct \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

seems to be established without their aid.

The equations expressing the variables in terms of the factors are additive and can be summed to give i and $o = i + \varepsilon$, where ε is the discrepancy between the two estimates, in terms of the three factors. In this way we obtain—

$$o = 49{\cdot}1762\,F_1 + 0{\cdot}7014\,F_2 + 0{\cdot}0297\,F_3 \quad . \quad . \quad . \quad . \quad (11)$$
$$i = 49{\cdot}4353\,F_1 - 2{\cdot}0313\,F_2 + 2{\cdot}8339\,F_3 \quad . \quad . \quad . \quad . \quad (12)$$

whence—

$$\varepsilon = -\,0{\cdot}2591\,F_1 + 2{\cdot}7327\,F_2 - 2{\cdot}8042\,F_3 \quad . \quad . \quad . \quad . \quad (13)$$

or, if ε is normalized to give ε^*—

$$\varepsilon^* = -\,0{\cdot}0445\,F_1 + 0{\cdot}4697\,F_2 - 0{\cdot}4820\,F_3 \quad . \quad . \quad . \quad . \quad (14)$$

This regression equation is illustrated in Diagram 18.

The variance ratio for calculated over observed ε, i.e. $R^2{}_{\varepsilon\,.\,123} = 0{\cdot}4549$, so that to some extent the variations in the discrepancy can be explained in terms of the three factors. Equation (14) shows that these variations are mainly to be accounted for in terms of the second and third factors rather than the first. In other words the discrepancy between outlay and income, apart from a constant term, tends to be large when income is changing and tends, rather unexpectedly, to increase with time. This confirms a conclusion previously reached on the basis of an analysis of variance.*

At this point it will be convenient to consider the coefficients of the seventeen equations set out in Table V. These figures may be considered along with their squares set out in Table X below, which show the proportion of the variance of each variable, which is accounted for by each of the three factors. This simple partition of the variance is possible because the factors are uncorrelated. The figures in the table are equal to $a^2{}_{js}/\Sigma x^2{}_j$. The residual proportions in the last column are the complements of the ratios in the last column of Table IV.

They will be considered in two groups: The income group, variables 1, 3, 9, 10, 11, 12, 14, 15, 16; and the outlay group, variables 2, 4, 5, 6, 7, 8, 13, 17.

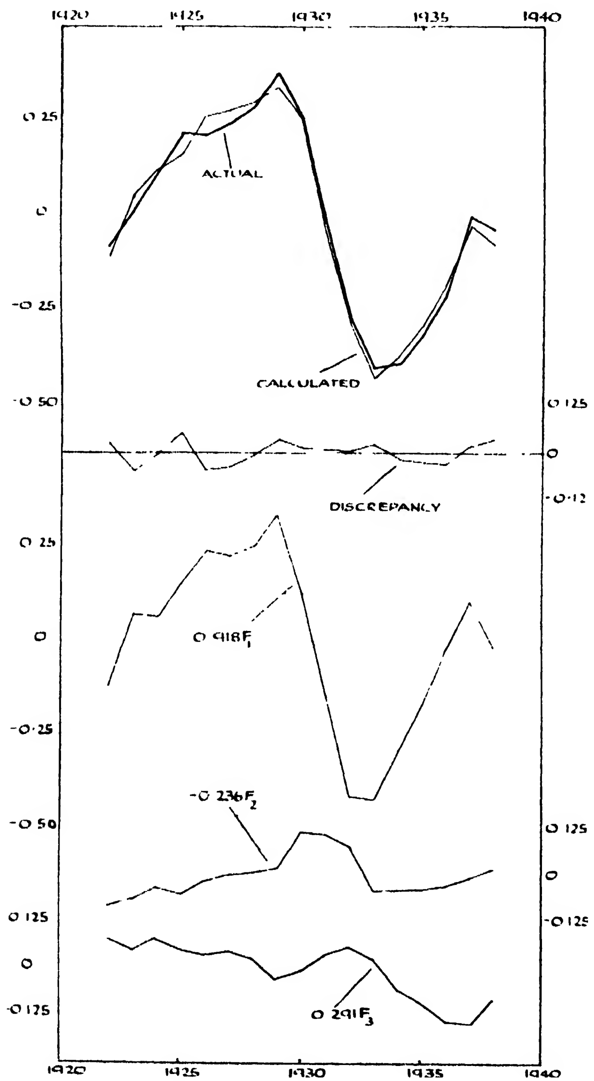
TABLE X.—*Proportion of the Variance of Each Variable Accounted for by Each of the Factors*

Variable.	Proportion accounted for by—			
	F_1.	F_2.	F_3.	Residual.
1	0·9666	0·0229	0·0089	0·0016
2	0·9068	0·0009	0·0851	0·0072
3	0·5018	0·4918	0·0006	0·0058
4	0·9462	0·0087	0·0296	0·0155
5	0·8267	0·1215	0·0431	0·0087
6	0·7311	0·0051	0·2581	0·0057
7	0·4992	0·0019	0·3367	0·1622
8	0·4230	0·1010	0·1093	0·3667
9	0·0889	0·7209	0·0252	0·1650
10	0·5467	0·0332	0·3961	0·0240
11	0·8430	0·0558	0·0849	0·0163
12	0·7194	0·1382	0·0590	0·0834
13	0·8270	0·1333	0·0066	0·0331
14	0·0000	0·7486	0·0001	0·2513
15	0·2108	0·0001	0·0537	0·7354
16	0·7078	0·0885	0·0074	0·1963
17	0·0447	0·0051	0·3883	0·5619
Σ	0·8076	0·1059	0·0609	0·0256

* See *The Economic Journal*, April, 1943, pp. 71–3.

The three factors account for almost the whole of the variance of x_1, employees' compensation. The movement of this variable is very similar to that of F_1, but it will be noticed that it is negatively associated with the second factor, i.e. as income falls there is an offset to the associated fall in employees' compensation. This characteristic is shared by all income payments with the exception

DIAGRAM 11
Entrepreneurial Withdrawals



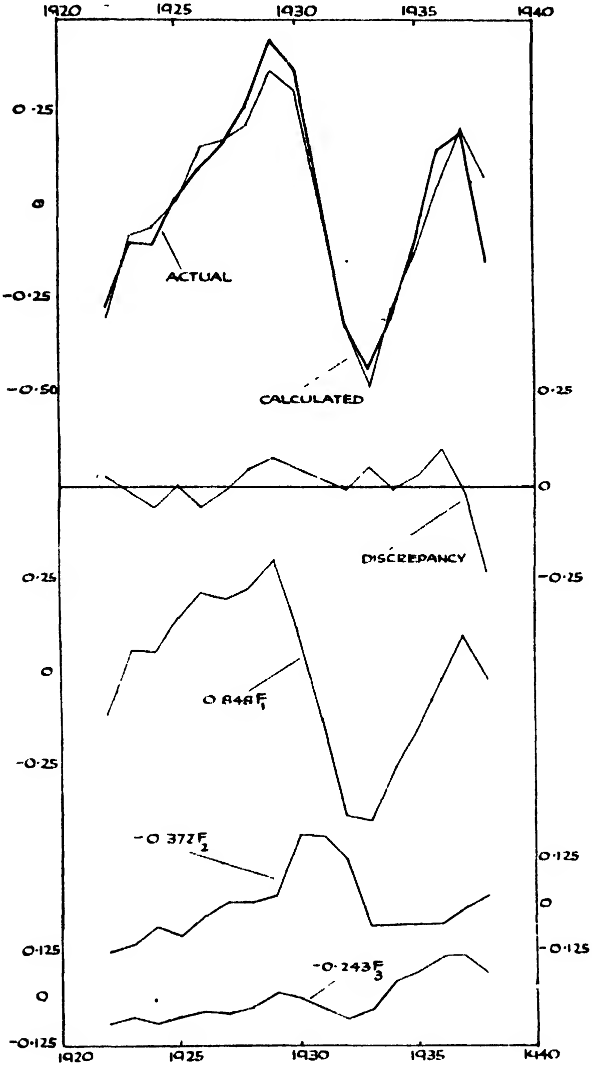
of net rents received by individuals. The opposite tendency can be seen in net saving of enterprises, x_3, even after the sums used to write down inventories, x_0, are written back.

Entrepreneurial withdrawals, x_{11}, and dividend payments, x_{12}, show a relationship to the factors somewhat similar to that of employees' compensation. The correlations with F_1 are lower and those

with F_2 are higher. The former shows a positive association with F_3 (a downward tendency), while the latter shows a negative association with this factor.

Interest payments, x_{14}, show what is almost certainly an untypical behaviour, being associated (negatively) only with the second factor. This is probably to be attributed to changing methods

DIAGRAM 12
Dividends



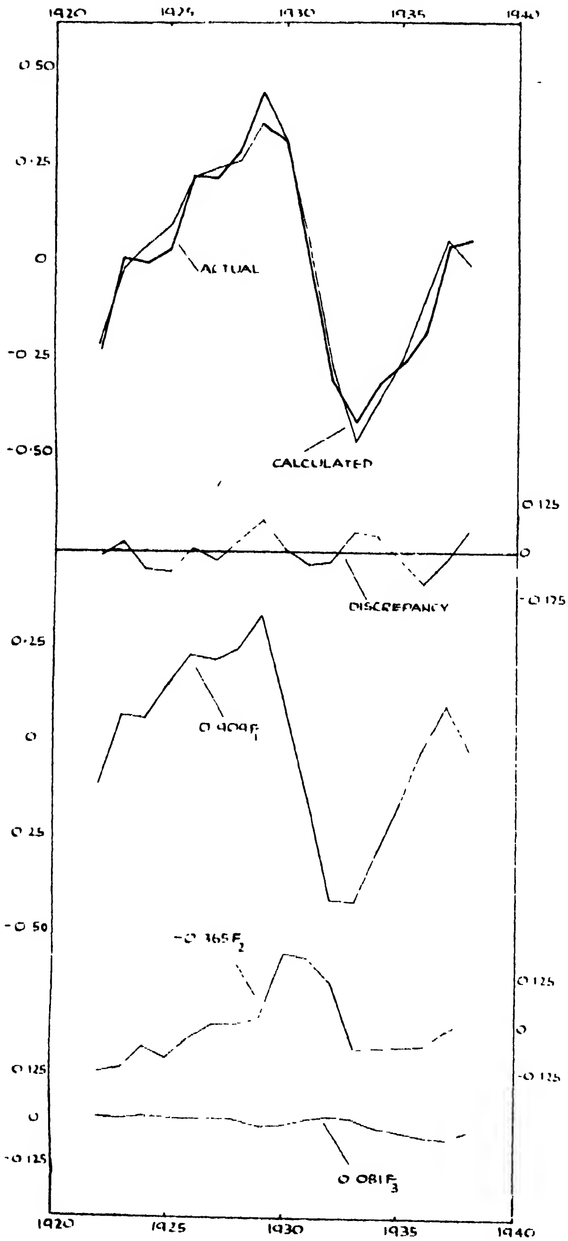
of company financing, and to conversions during the period of low interest rates following the depression of the early 1930's.

Net rents paid to individuals, x_{10}, show a small positive correlation with F_2 and a much larger positive correlation with F_3.

Net savings of enterprises adjusted for capital revaluations, x_3, is almost wholly explained in

DIAGRAM 13

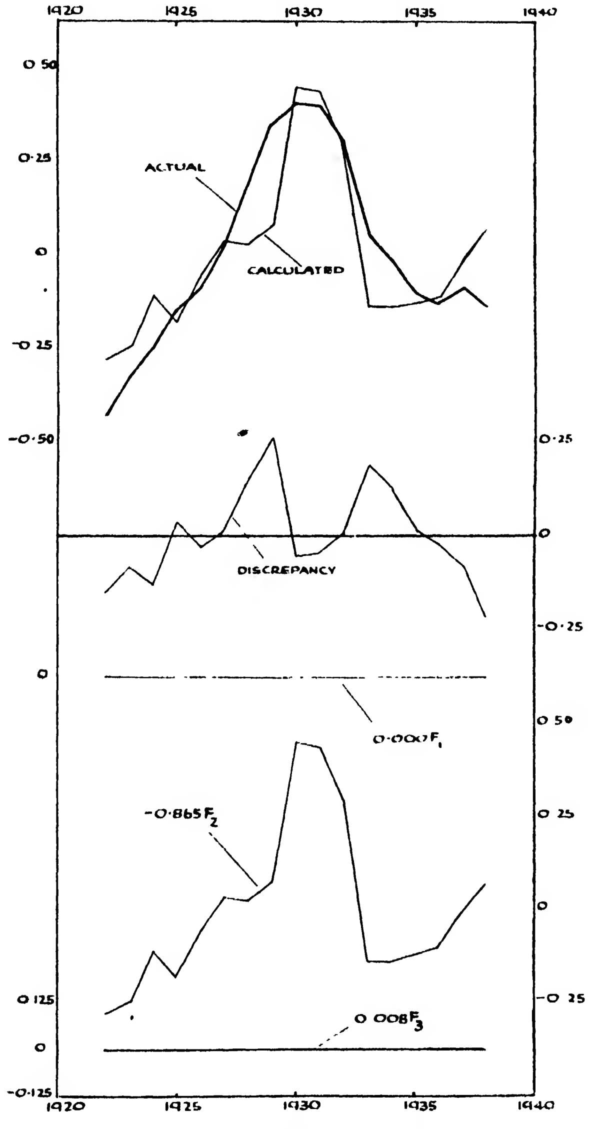
Adjustment for Depreciation



approximately equal proportions by the first two factors. This item reflects the movement of inventory revaluations, x_9, which are mainly associated with the change rather than the level of total income. Variable x_9 is an adjustment item needed to write back into total income the inventory losses which automatically appear as a deduction in x_3.

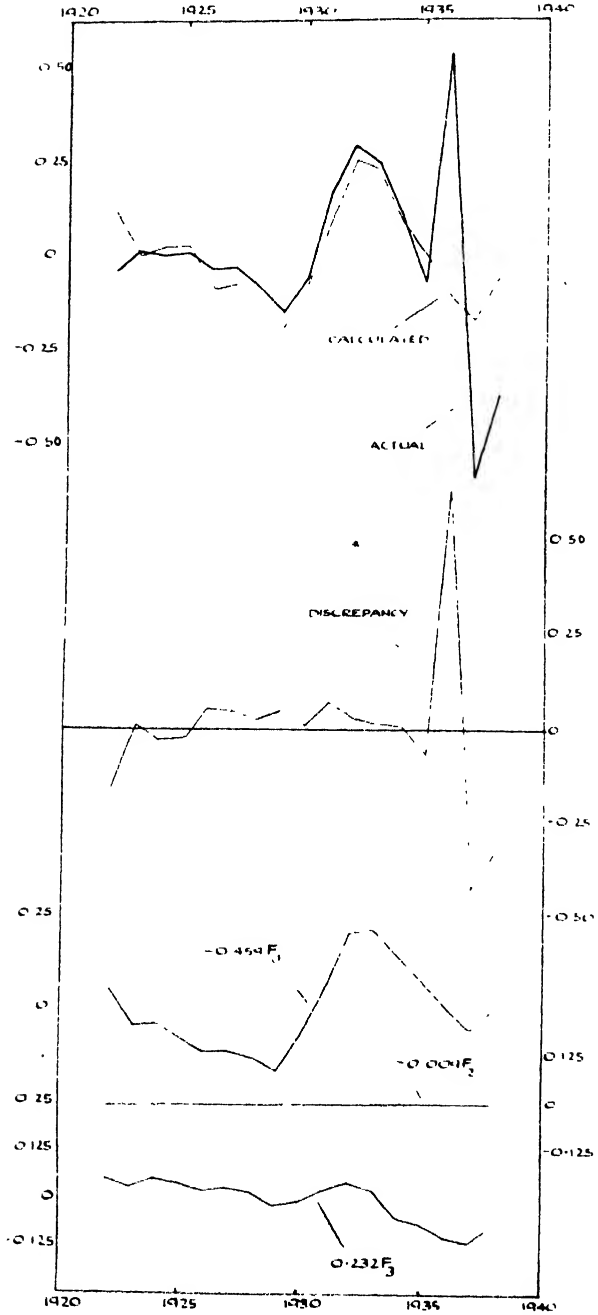
DIAGRAM 14

Interest



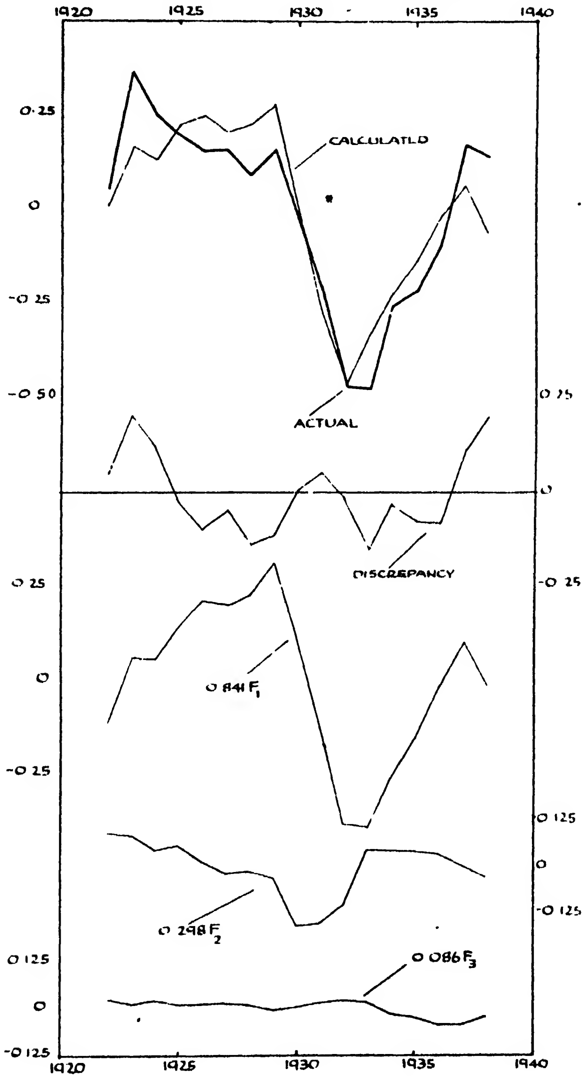
The remaining two income items are relatively unimportant. Variable x_{16} is another adjustment item needed to put the profit figures on the same basis in respect of depreciation and depletion as is adopted on the outlay side of the account. Variable x_{15} is a combination of small items not important enough to be treated separately. Its negative association with F_1 is largely

DIAGRAM 15
Numerous Small Items Combined



due to the fact that it contains direct taxes paid by individuals as a negative component. The inability of the three factors to explain more than some 26 per cent. of its variance arises mainly from the fact that, though a small item, it contains the veterans' bonus, which appeared as a substantial item in 1936.

DIAGRAM 16
Adjustment for Depreciation and Depletion



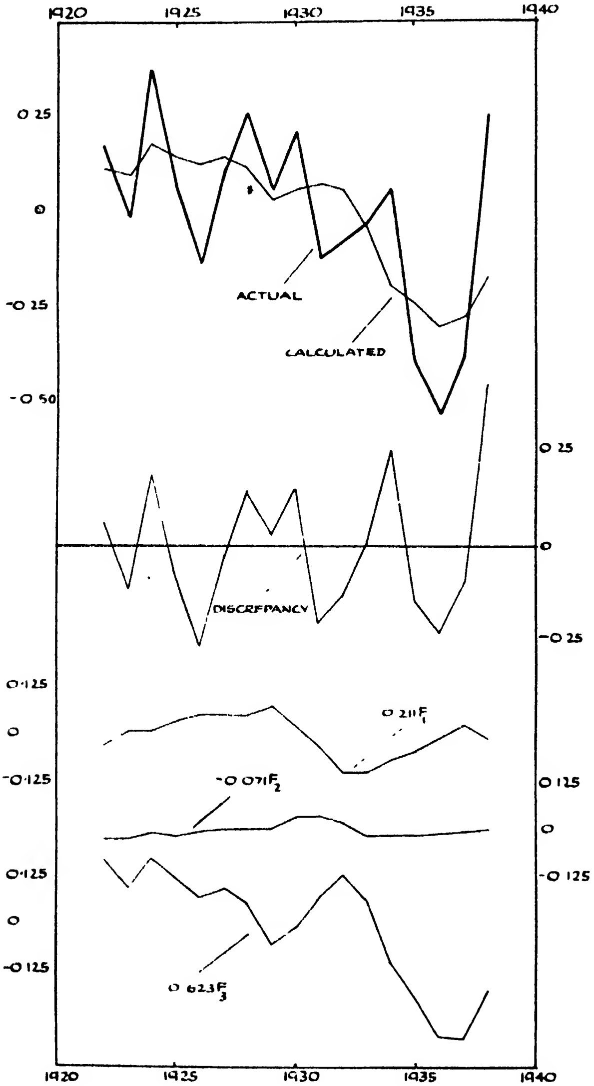
In the outlay group, consumers' expenditures on goods, x_2 and x_4, move closely with F_1. Consumers' expenditure on services moves somewhat less closely with F_1 and is negatively associated with F_2.

Gross fixed capital formation is represented by two items, producers' durable goods, which are included in x_2 owing to the high correlation ($r = 0{\cdot}954$) between these goods and consumers'

perishable goods over the period, and construction, x_6. The latter is fairly closely associated with F_1 and also with F_3, i.e. it shows a downward tendency over the period. Depreciation and depletion, x_{13}, an adjustment item needed to convert gross into net fixed capital formation, shows a moderately high correlation with F_1 and a negative association with F_2. The change in inven-

DIAGRAM 17

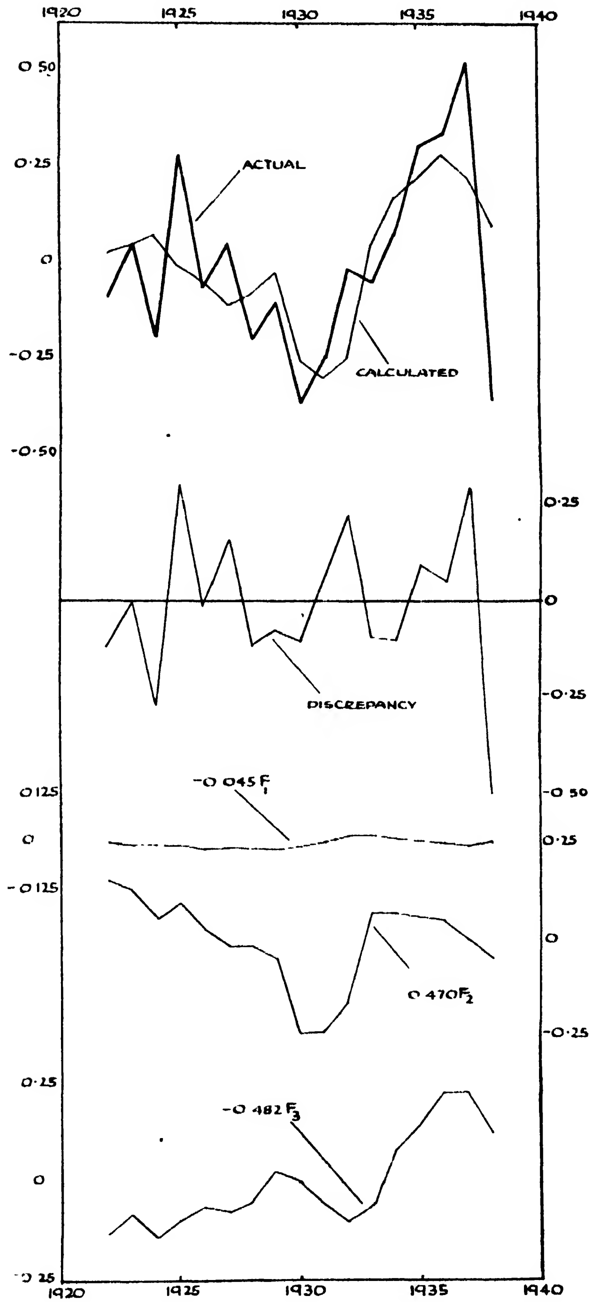
Foreign Balance including Foreign Tourist Expenditure in the United States



tories, x_8, the final component of domestic net capital formation, is associated most closely with F_1, but is also associated positively with F_2 and negatively with F_3, i.e. shows an upward trend.

Net public outlay, x_7, shows, as might be expected, an untypical behaviour. It is negatively associated with all three factors.

DIAGRAM 18
Discrepancy: Outlay *less* Income



The final component of outlay, the foreign balance including foreign tourists' expenditures in the United States, x_{17}, is a small variable only very partially explained by the three factors. Much the most important contribution is made by F_3, x_{17} showing a marked downward trend.

5. *Conclusions and Suggestions*

This section will begin with a summary of the conclusions which seem to emerge from this investigation, and will end with some brief remarks on the possible uses of factor analysis in econometric work.

(1) Starting with seventeen variables representing the components of total income and outlay in the United States, it has been found that the greater part, 97·5 per cent., of their combined variances can be explained in terms of three factors. Furthermore in most cases the individual variances are largely explained in terms of the three factors.

(2) It is possible by comparison with series estimated from economic considerations and not from the factor analysis to identify approximately the first factor with total income, and to show that the second factor is closely related to the rate of change of total income. It must be remembered that the individual factors are mere mathematical abstractions chosen for certain convenient algebraic properties, one of which in the present instance is mutual orthogonality. A more meaningful approach to the matter therefore is to ascertain if certain economically meaningful variables, such as i and Δi in this example, believed to be useful for explanatory purposes, belong approximately to the same reduced factorial system as the variables analysed, the x's in this example, rather than to attempt to identify individual economic explanatory variables with individual factors. It so happens in this case that F_1 and i are highly correlated, but this is not the important point. What really matters is that i, Δi and t belong approximately to the same three-factor system as the x's.

So much for specific conclusions. We shall now proceed to more general topics.

(3) It seems clear that factorial methods are potentially valuable in the analysis of economic data, particularly in dealing with problems of classification, or the formation of ideal types of variation by which sets of variables such as transactions or prices may be characterized. The method of principal components seems well adapted to economic investigations, and leads to measures which have many desirable algebraic properties. From this point of view the purpose of factor analysis lies in its usefulness in the reduction of data, enabling us to replace a large number of series by a small number which provide the principal components of variation of the original data. Subject to what is said in the next paragraph, we might restrict ourselves in the first place to an attempt to explain the variation of the first few principal factors of a set of variables rather than that of the much more numerous variables themselves.

(4) Since the purely mathematical character of the factors has been repeatedly stressed in this paper, it may seem foolish to suggest that economic theories should be directed to the explanation of the variation of such abstractions. This objection would, however, be false in so far as it was possible by rotation to reapportion the variation among a set of different (in general oblique) factors which could be identified with "real" economic variables. This is the same point as was made in paragraph (2) above, and shows the way in which economics (or whatever the subject may be to which factor methods are applied) comes into the whole proceedings. We may start by showing that a large number of variables belong approximately to a system with relatively few components of variation, say m in number, and we may then go on to show that certain "key" economic variables also m in number belong approximately to the same system, so that an explanation of their variation provides us with an explanation of the variation of all the variables with which we started. The selection of the "key" variables is a matter for the economist, though in his capacity of statistician he may ascertain that the first set that occurs to him will not do because it does not fit into a reduced system of the required number of dimensions.

(5) In the light of (3) and (4) it may be helpful to indicate some actual cases in which factor analysis would probably be useful in economics.

First, suppose we are interested in demand analysis. In trying to explain the amount of any commodity demanded we ought, in theory, to introduce as determining variables not only the commodity's own price, but also those of all the other commodities bought by the group of

APPENDIX

Kuznets' and Barger's Data for Income and Outlay in the United States of America

1922–38

(U.S. $ million)

j.	Component.	1922.	1923.	1924.	1925.	1926.	1927.	1928.	1929.	1930.	1931.	1932.	1933.	1934.	1935.	1936.	1937.	1938.
1	Employees' compensation	37,003	43,339	43,323	45,019	48,017	48,433	49,361	52,214	47,767	40,468	31,699	30,054	34,888	37,930	42,484	46,574	**43,232**
3	Unadjusted net savings of enterprises *plus* adjustment for capital revaluation	1,419	3,129	1,817	3,390	3,215	1,407	2,232	2,438	−5,134	−8,326	−10,054	−4,454	−2,192	−1,134	872	238	**−1,005**
9	Inventory revaluation adjustment	−877	−156	157	−350	1,695	784	60	702	4,115	3,230	1,471	−2,273	−1,487	−724	−164	−635	**1,104**
10	Net rent received by individuals	4,896	5,165	5,631	5,465	5,141	5,078	4,941	4,917	4,265	3,026	2,090	2,114	1,905	2,143	2,186	2,579	**2,575**
11	Entrepreneurial withdrawals	10,788	11,345	11,945	12,503	12,452	12,621	12,876	13,384	12,773	11,207	9,747	9,019	9,099	9,516	10,119	11,227	**11,056**
12	Dividends	2,962	3,745	3,683	4,270	4,615	4,918	5,344	6,117	5,768	4,345	2,752	2,251	2,905	3,729	4,861	5,022	**3,530**
14	Interest	3,979	4,206	4,374	4,579	4,698	4,935	5,272	5,604	5,720	5,712	5,515	4,999	4,848	4,648	4,608	4,688	**4,593**
15	Dividends, interest and non-commercial remittances from abroad *less* direct taxes paid by individuals *plus* veterans' bonus *plus* social security benefits *less* employees' social security contributions	−1,053	−947	−960	−955	−1,046	−1,037	−1,136	−1,272	−1,091	−608	−369	−446	−748	−1,103	146	−2,207	**−1,750**
16	*Less* adjustment for depreciation and depletion	−407	−854	−685	−600	−547	−553	−456	−553	−286	−9	353	364	44	−15	−185	−572	**−524**
	Total income	58,710	68,972	69,285	73,321	78,240	76,586	78,494	83,551	73,897	59,045	43,204	41,628	49,262	54,990	64,927	66,914	**62,811**
2	Consumers' perishable goods	21,410	22,967	23,750	25,404	27,107	26,672	27,348	28,550	26,395	21,481	18,147	18,133	20,756	23,095	25,363	26,706	**25,502**
4	Consumers' semi-durable goods	10,023	11,324	10,735	11,361	11,917	12,032	12,193	12,382	10,731	9,024	6,722	6,513	7,512	8,151	9,200	9,720	**8,992**
4	Consumers' durable goods	6,181	7,943	7,900	9,056	9,445	8,890	9,174	9,913	7,550	5,748	3,806	3,882	4,686	5,918	7,342	7,664	**5,410**
5	Consumers' services	20,318	22,286	23,510	24,345	25,181	25,804	26,382	27,164	25,890	23,377	19,704	17,671	18,186	19,283	20,954	22,416	**22,285**
6	Construction	5,318	6,882	7,586	8,250	8,810	8,580	8,180	7,883	5,941	3,712	1,767	1,091	1,232	1,605	2,551	3,326	**2,932**
2	Producers' durable goods	3,550	4,986	4,695	5,021	5,434	5,139	5,465	6,487	5,116	3,276	1,826	1,889	2,955	3,713	5,135	6,521	**4,846**
7	Net public outlay	387	−136	41	100	−293	−212	123	11	469	2,271	3,031	2,258	4,463	3,589	4,743	1,271	**1,850**
8	Net increase in inventories	32	3,126	−467	1,631	1,413	808	−658	2,291	712	−916	−2,711	−1,456	−1,223	1,151	1,816	3,138	**−1,084**
13	*Less* adjustment for depreciation	−7,508	−8,388	−8,345	−8,482	−9,176	−9,167	−9,413	−10,003	−9,532	−8,421	−7,256	−6,818	−7,178	−7,380	−7,676	−8,501	**−8,572**
17	Foreign balance, including foreign tourist expenditure in the United States	510	246	776	359	83	422	628	348	564	86	164	219	350	−270	−466	−256	**611**
	Total outlay	60,221	71,236	70,181	77,045	79,921	78,968	79,422	85,026	73,836	59,638	45,200	43,402	51,739	58,855	68,962	72,005	**62,772**

purchasers we are considering. This is clearly impossible, and what is usually done is to include a price index representing the movement of "all other prices." In doing this we are allowing approximately for the first component of variation of the price-complex, but for nothing else. By applying factor analysis to the complex of price variations we could ascertain a few more components of variation, which together with the first component would enable us to make a better allowance for price influences than is obtained by the usual method. It would still be necessary in certain cases to deal with the price movements of closely competing or completing goods, but the method would give a better representation of the variation of "other prices."

Again, if we are trying to analyse the equations of motion of the whole economic system we shall normally begin with a large number of variables and equations. With the object of reducing the labour involved we may try to reduce this number as much as possible. There is, however, another method of approach, namely, to start off with a system of any degree of complexity, extract the principal components of its variation, and concentrate on the explanation of these components or an equivalent number of identifiable components belonging to the same system. This, indeed, is a method of arriving at a set of "inner variables" which, if explained, will in turn explain all the other variables with which we started.

Discussion on Mr. Stone's Paper

Mr. Champernowne: It is a particularly agreeable task for me to propose a vote of thanks to Mr. Stone, who is a fellow of my own college at Cambridge, and with whom I have on so many occasions worked and discussed problems in economic statistics; but as my time is limited, and as I am now representing another university, I shall try to escape from these sentimental associations during my remarks, and confine my appreciation of the excellencies of his paper to a short space and try to develop one or two minor criticisms on small points.

The severely technical title of the paper first suggested that no more would be attempted than an account of the correlations existing between the various parts of the income and outlay of the United States during the period 1922 to 1938; but having applied the technique of factor analysis to these series, Mr. Stone had gone on to consider far wider problems concerning the explanation of the movements of the various series and other applications of factor analysis to such problems as demand analysis and elucidation of the equations of motion of the whole economic system, as discussed in the final paragraphs of the paper.

Constructive optimism is characteristic of the most influential economic statisticians, such as the late Lord Keynes and Mr. Colin Clark, so that they are impatient of the limitations of existing materials and methods, and their minds are always working to adapt existing material and statistical techniques to suggest the answers to the big central problems of economic theory and administrative practice. So it is with Mr. Stone, who during the war took hold of the idea of applying the principles of accountancy to national income statistics, and turned it into the impressive Government White Paper on National Income which the Central Statistical Office now provides for us every April. Again, starting from the idea of using Professor Frisch's confluence analysis to examine the demand for commodities, Mr. Stone gave to us nearly two years ago a comprehensive paper which was described in the discussion as the best in its sphere ever read before this Society. In that paper he derived estimates of the nature and importance of many economic influences which affect the quantities consumed of a large range of important commodities, and he derived all these estimates from a quantity of basic data so small that most academic statisticians would have lacked the courage to publish their conclusions in full detail and offer them for the criticism of this so learned Society. Mr. Stone's courage has been amply justified by the fact that his results have not after all these months been refuted.

The subject which Mr. Stone has attacked this evening is that of explaining the movements of various series of figures constituting the income and outlay of the United States during the years from 1922 to 1938. This problem is one which would exhaust the patience of almost any statistician. For when one studies the economic time series of this period for either the United States or this country one soon discovers that all the series behave in a very similar fashion; nearly all of them seem to respond to the boom of 1929 and the slump of 1933. Some of them lag behind and others lead. Some of them show a trend increase and others a trend decrease; but apart from these divergencies, most of them seem to behave alike. The academic statistician may well be tempted at this stage to throw in his hand in despair, and say that the series are so completely

mixed up with each other that there is no way of telling which is the cause of which. It would be always possible to account for any one series in terms of a linear trend and the movements of any two of the other series: provided that the fluctuations of the other series were not exactly in phase with one another.

Mr. Stone has not given up. He has taken the bull by the horns and said, "These series seem to be highly intercorrelated and therefore easier to explain because we can explain all of them in terms of two key series and a linear trend, and all that will remain to be done will be to explain these two key series."

Using the admirable device of a spherical map, Mr. Stone has been able quite adequately to represent the seventeen series of observations by seventeen points, with circles round them to indicate the margins of error. The distance between any two points represents the amount of correlation between their corresponding series, the precise mathematical relation being that the cosine of the distance between the two points represents, within limits of sampling error, the correlation coefficient between the two series concerned. The fact that this mapping can be done at all, within the margin of the sampling errors, is due to the high interdependence of the series, and also to the high margin of error due to sampling error for the series covered. A spherical map tells us all we can learn about the true intercorrelation from information so inadequate.

In passing, I think this point about the very large margins of error and the fact that spherical mapping only is possible because the data are inadequate may possibly bear on some of the points discussed in Mr. Stone's earlier paper about Demand Analysis. It may be suggested that the margins of error there are perhaps rather higher than one would otherwise suppose, but I think Dr. Barna is later going to develop that point, so I will leave it on one side.

The question may be put, about this mapping on the sphere, whether, in view of the roughness of the information available, even a spherical map is needed. Can we even distinguish three factors on the surface of a sphere, or ought we sometimes to be content with two? If we may focus our attention on the three influences of (1) trade cycle sensitivity, (2) time lag, or time lead, (3) trend increase or trend decrease, it is pertinent to ask whether, having measured the trade cycle sensitivity for any one series, we can still measure both its time lag and its trend increase. My own belief is that only a minority of the series here given provide sufficient information to enable this to be done with significant results. Most of the series will allow a significant measurement of trade cycle sensitivity and of time lag—or alternatively, of trade cycle sensitivity and trend; only a minority will provide significant measures of all three. I think Mr. Stone himself would allow that, since the circles drawn round the points on his sphere show the margin of error. If this were so he could confine himself to two great circles on this sphere cutting each other at right angles. He could even represent it on a blackboard, in that case by drawing two circles on the blackboard; but it also means that even less conclusions could be drawn about what is the cause of what.

This negative conclusion is based on an attempt to explain eight of Mr. Kendall's random auto-regressive series* in terms of Mr. Stone's three factors. These series oscillate in much the same way as economic series, but are generated by a random process. On the average, each factor explains about one-eighth of the total variance of any auto-regressive series. This equals one-fifth of the residual variance left unaccounted for by the three factors. But in some cases a factor explains as much as one-third of the residual variance, and this suggests that whenever in Mr. Stone's analysis a factor accounted for an amount of variance less than one-third of the residual variance this explanation was not really significant, because it might quite well have happened by chance as in the case of the auto-regressive series just considered.

If we turn to p. 21 and compare the figures in column F_2 and F_3 with the residuals, we find that on this test five figures in column F_2 are not significant and seven figures in F_3; so are three figures in column F_1. Only in the case of variable numbers 1, 4, 5, 6, 10, 11 and 12 are all three coefficients significant.

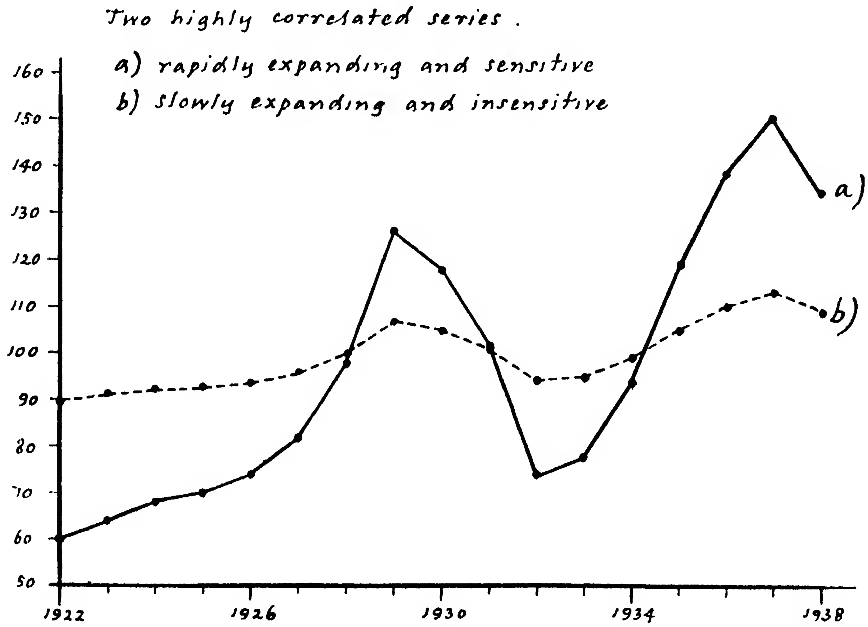
My time is practically up, and I do not wish further to labour the question of the unreliability of information extending over so short a period. Mr. Stone has laid considerable stress on the fact that each of his three factors has a high total correlation with the three variables, income, rate of change of income, and time. Mr. Stone had himself foreseen that this was likely, and it is no criticism of his paper to suggest that the explanation is fairly simple. I think I am right in saying that the total of his seventeen series almost exactly represents income; it is in fact almost exactly equal to income, and owing to the method by which Mr. Stone has chosen his series and to the fact that most of the factors are correlated with each other, I think it is inevitable that

* Kendall, M. G., *Contributions to the Study of Oscillatory Time Series.*

the first factor should be highly correlated with the total income.* The fact that the main difference between the other series is one of time lag automatically ensures that the other factors must be highly correlated with the rate of change of income, because if you add a small multiple of the rate of change of a factor on to the factor itself, it has roughly the effect of just advancing the series in time, making everything happen a little earlier.

I think that explains why the second factor is so highly correlated with the rate of change of income, or, at any rate, with some linear function of income itself and the rate of change of income. (The second alternative is in this particular example avoided, because the correlation between income and the rate of change of income happens to be very low.) The correlation with time itself is admittedly not quite obvious, and it is not so easy to show why that correlation should be expected; but it seems to be natural that you would have time represented in the third dimensional factor because income itself has no very steady trend in time, nor has the rate of change in income, yet some of the series represented change quite a lot over the whole of the period. In saying this I am not differing from Mr. Stone, who did himself say that it was a thing to be expected.

There are one or two general remarks to be made about applying factor analysis to economic time series. It is worth pointing out that there are certain dangers in using factor analysis based only on correlation coefficients for purposes of classification. For if we rely on the coefficients we shall classify together any two highly related series, even though they are unalike. Thus motor vehicle output may have a rapid trend increase and be sensitive to booms and slumps. Bread production may have a gentle trend increase and be fairly insensitive to booms and slumps. In this case the two series may well be highly correlated, but to classify them together would be misleading. The point is illustrated in the accompanying chart, which shows graphs of two such series which behave quite differently, although there is perfect correlation between them.



One would not want to classify these as identical. From the economic point of view they may represent quite different kinds of influence, but they are very highly correlated, as you can see from the diagram. They would automatically be classified together on the factor analysis diagram.

One other point upon this is that despite the difficulties I have stressed in applying this factor

* I agree, however, with Dr. Geary's point in the discussion, that the observed value of 0·995 is higher than one would expect: 0·97 is the value I should expect on the average, and the high value of 0·995 is a bit of a fluke, but not a very violent one.

analysis technique to economic time series, there is one small compensating advantage, namely, that it is possible to take advantage of the fact that the number of years is fairly small, and there is this continuous variation from year to year, in order to effect certain computing economies. I think it would be possible to compute the three main factors, or something very like them, without having to work out the correlation coefficient between each pair of series. Taking the approximation of the first factor, by applying weights plus 1, 0 or minus 1 to the various series, choosing these weights in the first place by inspection, one would then get the second approximation for the first factor by applying as weights to the series their various correlation or covariance with the first approximation to the first factor. One would then proceed in like manner to find approximations to the other two factors. But I will not spend time developing that point now: obviously the amount of work to be done would only increase in direct proportion to the number of series, whereas in the ordinary way the amount of work increases with the square of the number of series to be examined; so if the number of series is large, considerable computing economies may be effected.

In conclusion I would like to ask Mr. Stone to develop the remarks made in his final paragraph, where he suggests that factor analysis may be useful when we are trying to analyse the equations of motion of the whole economic system. Although from my own pessimistic point of view I find it difficult to see how factor analysis can be used to explain anything in economics at all except in the sense of describing the correlation coefficients observed, the idea of using factor analysis for showing inner variables sounds very fascinating.

I have great pleasure in proposing a vote of thanks to Mr. Stone for his lucid and entertaining paper.

Mr. Babington Smith: I have much pleasure in seconding the vote of thanks to Mr. Stone for his interesting and stimulating paper. I think there are good reasons why factor analysis should be tried outside psychology, the field in which it was born and grew up, and I am very glad this is being done. In the psychological field there has been a tendency to sectarianism, which I think is quite largely due to the present state of ignorance about the subject-matter and the consequent disagreement as to the interpretation of the abstract factors. The procedure of factor analysis is wholly mathematical, and in that sense should be applicable to any correlated sets of measures.

The question of the relative merits of the various mathematical processes has, I think, been fairly well thrashed out, and the main problem now is one of interpretation. While the use of factor analysis in fresh fields is all to the good, I should myself prefer to see the methods applied to cases where an answer is known rather than in economics—another field where, I believe, interpretations may still not find unanimous acceptance.

I am much interested in his argument at p. 8 in favour of the use of squares and products as against correlations. I think he correctly represents the trend of development in psychology to design tests *a priori*, but I am inclined to think the method is most useful in mapping an unknown field, and I would prefer to carry out the inverse process of assessing the relative importance of a few factors by another method—perhaps analysis of variance.

In the paper itself there are several points I would like to raise with Mr. Stone. I do not know the answers and I am seeking information. The first point is: By all the standards developed in psychological work, the size of the group—that is to say, the number of years considered—is very small; and it is also small when you consider the number of variables that have been used. This means that the correlation coefficients have standard errors of considerable size, and surely there are discouragingly few degrees of freedom in determining the new axes. I think there are methods for assessing the significance of factors and factor loadings; and certainly there are methods for seeing whether more than one factor is necessary. In view of this, perhaps the most surprising thing about the paper is the very close fit which is achieved by the regression equations, and I cannot help feeling—I think it was the point made by the last speaker, Mr. Champernowne—that much of this is due to the use of unstandardized squares and products and the preponderating effect of income itself.

The second point is that I notice the period under review covers years in which I have heard there was a great boom and a great slump, and I would like to know what effect this has had on the resulting factor analysis. If there were conceivable a halcyon period from which boom and slump were absent, would the same factor pattern appear?

The third point: What is the effect of constraints which would be incurred from the fact that certain constituents add up to income and others add up to outlay? What is the result in loss of degrees of freedom? Were the estimates of the constituents independent? Were they, for instance, independent, for successive years?

The fourth point is that I am very much puzzled to see how Δi, the change in income from year to year, can come out as a factor from a factor analysis of a set of variables which together represent income and outlay. Nothing in the correlation table, or in factor analysis as a method, relates to the order of the members in the sample. The nearest I can get to an explanation is that, if Mr. Stone had correlated years (which we may call Q analysis), not parts of income and outlay (which we may call R analysis), there might have been greater correlations between neighbouring years, and that this phenomenon would have made itself felt in the analysis as a factor representing neighbourliness or the resemblance between successive years. Then, on the principle that the situation underlying the Q or R analysis is the same, if a factor of resemblance between successive years comes out in the Q analysis it must have an analogue in the R analysis.

I am not quite satisfied by this explanation, and should be glad to have Mr. Stone's views on this and my other points.

The vote of thanks was then put to the meeting and carried unanimously.

Sir CYRIL BURT said he had listened to Mr. Stone's paper with a twofold interest, first because he had always been attracted by the possibility of applying the methods of factor-analysis to economic problems, and secondly because, when Mr. Stone and Miss Potter had first consulted him about factorial methods, he had himself ventured to try his hand at an elementary factorization of the figures they had sent.

As everyone knew, different psychologists had proposed many rival procedures to discover and estimate what they rather inappropriately had called "factors," and economists might feel a little doubtful as to which of these methods, if any, they were to adopt. Elsewhere he had sought to show that all of them could be reconciled, if they were regarded as convenient working procedures for approximating to what he himself considered to be the best and fundamental method of all—that of least squares as applied to the matrix of variances and covariances (or, more accurately, of square sums and product sums). It was, therefore, gratifying to find that, on quite independent grounds, Mr. Stone had eventually decided that this procedure was on the whole most fitted for his purpose. It might be noted that the use of principal axes as factors was originally suggested by Karl Pearson (*Biometrika*, I, 1901, p. 209), and was thus older than any other method of factorization.

Curiously enough, this was a procedure hardly ever adopted by psychologists; even Hotelling, in the reference Mr. Stone had given, had applied the technique to correlations, not to covariances; and Professor Spearman had always opposed the use of covariances. Sir Cyril Burt thought the real reason had been that the calculations required were so lengthy and elaborate. Since that might possibly deter economists who would like to experiment along factorial lines, he might perhaps mention that, at any rate for preliminary studies, there were much simpler methods available. Miss Potter, he thought, originally had tried using one or two of the short-cuts that he had used in factorizing physical measurements; and he himself, with the help of one of his assistants, had tried the effect of applying what was called "simple summation" (which Thurstone had re-named the "centroid" method) to the correlations derivable from Mr. Stone's covariances. So far as Sir Cyril could see, the factors that emerged were almost exactly the same, though the detailed figures were slightly different.

The main difference was due to the fact that, as Mr. Stone had pointed out, the observed variables differed so widely in their variance, as shown in his Table I. Sir Cyril Burt was inclined to suggest that, in applying the technique to a relatively new field, both methods ought to be tried. It always seemed possible that a variable which had a very small variance might, nevertheless, be one of the variables which furnished the best clue or guide to the variations of all the others. In such a case, factor-loadings based on covariances might lead to an under-estimation of its influence.

Whatever method were adopted, however, there could be little doubt that all the variances exhibited by the 17 variables in the initial table could be explained, with a very trifling remainder, in terms of three factors, and three factors only. Though the number of years over which the correlations were calculated seemed much smaller than was usual in factor-analysis, nevertheless that result seemed a most remarkable demonstration of the value of factorial analysis in economic research.

The nature of the three factors was, he believed, rather what might be expected on *a priori* grounds. In anthropological work the nearest parallel he could think of was the factorial analysis of assessments for physical growth and educational development, based on tests and re-tests over a period of years. There, too, much the same three factors emerged, though not always in the same order. The first factor was usually an average of all the assessments; it might be called the

child's general level. The second factor represented rate of change or rate of growth. The third factor, of course, was time, which, in the case of developing children, meant chronological age.

Mr. Stone's spherical map was itself highly suggestive. For explanatory purposes Sir Cyril and his students had always found it illuminating to plot the three main factors on a blackened globe, like those used by the geographer. But there was a further device which might be usefully employed, both for illustration and even for approximate calculation. As geographers had long ago pointed out, the positions of points upon a globe could, by certain devices, be represented on a plane surface. With what was called gnomonic projection, a map could be obtained on ordinary squared paper. Better still, for many purposes, was the diagram obtained if one borrowed the stereographic projection-nets which were used for similar purposes in crystallography.

Turning to the more general implications of the paper, Sir Cyril Burt said that all psychologists would listen with interest to the criticisms of factorial techniques that might be advanced by professional statisticians and economists. Many psychologists were, of course, still a little sceptical about the validity of those techniques. The earliest factorists to discuss or criticize those methods regarded them as in some way peculiar to psychology as such. They had supposed that we had here a new instrument peculiarly adapted for discovering the fundamental faculties of the mind. He himself, however, believed that, reduced to its simplest terms, factor-analysis was merely a convenient device for averaging. The first factor to be extracted was nothing else but an average—at its best an appropriately weighted average—of all the variables in the table. The second factor was a weighted average of the deviations about the first factor; and so on. That being so, the statistical technique ought to be applicable to almost any set of complex data where correlations with such averages were desirable—to physical measurements, medical data, anthropological data, sociological data, and the like. Indeed, the devices developed independently by the quantum physicists for what they called "spectral analysis" turned on precisely the same principles.

If that were true, then it seemed to him that the technique stood or fell on its own mathematical merits, quite regardless of its value in the narrower field of psychology. With that interpretation in mind, and with the help of his colleague, Dr. Rosenstein Rodan, some of his students had often tried their hand at factorizing economic data, and were eventually led to the conclusion that the method should be useful for numerous problems where the primary object was to find the best weighted average or to investigate independent factors—for example, in determining various economic indices, the cost of living index, and so forth. Indeed, one of his students, Mr. Hammond, had made an attempt at his suggestion to apply factorial analysis to variations in prices, etc., in different countries during a period which had been much the same as that covered by Mr. Stone. Mr. Hammond had briefly reported some of his efforts in the last number of the *British Journal of Educational Psychology*.

In the field of economics, of course, added Professor Burt, psychologists were amateurs, not to say trespassers. But he thought all who were concerned with the scientific analysis of complex data, such as form the foundations of all the human sciences, would welcome the intensive and exceptionally thorough investigation that Mr. Stone had carried out.

Dr. Barna said he was very glad to read Mr. Stone's paper; he had had only forty-eight hours or so in which to think about it, but he was sure that economic statisticians will benefit greatly by reading the paper. He had managed to clear up some of his doubts, which had troubled him for a long time, by reading the conclusions of Mr. Stone's paper.

He desired to call attention, not so much to the technical details of the paper, as to its economic implications, and to the purpose of the method of factorial analysis as against the purpose of other methods, such as the method used in Mr. Stone's paper about two years ago.

Mr. Stone had managed to explain short-period variation in a great number of factors in terms of three major factors, and he was very glad to see that a very simple and satisfactory economic interpretation could be found for that, and he was also glad to see that there was nothing surprising about this economic explanation.

Of course, the fact that fluctuations in total income were the chief explanation of short-period fluctuations of components of the national income was the basis of Lord Keynes's theory, and the fact that the rate of change of income was another factor to be introduced in the Keynesian system had been pointed out by Professor Haberler in his book, *Prosperity and Depression*.

But it was one matter to deal with the interpretation of explanations of short-periods of fluctuation, and quite another matter to make use of those explanations in forecasting. It was, of course, forecasting which was the primary purpose of making those explanations, quite apart from curiosity, and he was afraid he was very pessimistic on the subject of forecasting, and he desired

to point out what could be called the economic consequences of Mr. Stone's present paper, especially on the results of his earlier paper.

When reading Mr. Stone's earlier paper the speaker had not liked the results, and as an economist he had not been prepared to accept the actual figures contained in the results. He thought the price-elasticities were too high and the income-elasticities, on the whole, too low; and he also thought that the error in the two elasticities must have been correlated; thus, if one were too low, the other must be too high.

The present paper gave a clue to the previous problem and he was sorry to say it confirmed his fears, namely, that there was a very high correlation between the 17 series (and although the present data were not identical with those used previously, he thought very much the same result would be reached), and hence, as Mr. Champernowne had pointed out earlier, the margin of error in the income-elasticities and price-elasticities must be very high—probably so high that the method of using multiple correlation analysis of short-period time series in order to discover price and income elasticities would have to be given up altogether.

The fact that various economic series were highly correlated was a useful fact in certain contexts; for instance, Dr. Rhodes's index number of business activity could be very economically constructed because of that, or Mr. Stone's conclusions could be used to extrapolate some of the time series. He preferred to use the expression "extrapolate" rather than forecast. The fundamental fact was that all our series were derived from the movements of a free economic system in the United States in a short period, when everything moved up and down together, and that would not tell us anything, for example, about the demand for a single and important commodity the price of which was to be raised substantially by a duty; or what happened to various factors when the government kept the rate of interest stable and did not let it fluctuate in a way in which it had fluctuated in the past. In so far as there was no independent variation in the system in the past, the forecasting based on historical data would break down.

He was, however, more optimistic than Mr. Stone as regards the suggestions for further work in this type of research. Mr. Stone's main attitude had been that all the difficulties were due to the fact that our samples were small, and he had wanted to enlarge the sample by having time series for longer periods than we had had in the past.

Dr. Barna was not sure that that was the right way to enlarge the sample. A sample could be enlarged in more than one dimension, and he was not sure that this was the right dimension. The purpose of enlarging the sample was to catch the independent variation of factors in the economic system. In the short period we had no independent variation. We had a stability in some institutional factors and also high correlation between the various economic variables; by lengthening the series we might get some independent variation; but on the other hand, we might destroy the assumption of institutional stability; for instance, the introduction of a tariff, in dealing with imports, did very much more than introduce independent variation; it destroyed the whole institutional stability of the economic system and cut the time series into two. He was afraid that lengthening the time series too much might do more harm than good. He would like to hear Mr. Stone's opinion on that point. As an alternative, Dr. Barna suggested enlarging the sample in other directions.

He had two main suggestions to make: one was to include an analysis of family budgets and try to collate the results obtained by this method with the results obtained from time series. Actually, he considered the best example for that was a paper by Mr. Stone, in the *Review of Economic Studies* in 1938, where the latter had tried to estimate the Keynesian multiplier by three independent methods.

The other suggestion Dr. Barna desired to make was again tying up with what Mr. Champernowne had said earlier—that the forming of composite time series from single series should be placed on a more scientific basis than has been done in the past. The fact that two series correlated did not seem to be a sufficient reason for aggregating them; a high degree of economic substitution between two commodities might be a more valid reason for lumping them together. Perhaps Mr. Stone will consider the methods used by the late Erwin Rothbarth when he dealt with a similar problem in connection with index numbers in the *Review of Economic Studies.*

Mr. C. R. Rao said that Mr. Stone in his paper started with the expectation, before the analysis was undertaken, that most of the variation would be capable of being accounted for by the three components:

(*a*) Total income or some similar quantity,
(*b*) Rate of change of (*a*), and
(*c*) A trend term representing the underlying tendency of the economy to expand or contract, or of its elements to do so relative to one another,

and came to the conclusion that his expectation has been confirmed by the factor analysis, and that most of the variables x_j can be explained fairly accurately by an expression of the form

$$x_j = ai + b\frac{di}{dt} + ct.$$

The method was to identify this system of economic variables with three principal factors which account for 97·4 per cent. of the total variance.

This achievement was more the triumph of an economist who could discover the economic forces which had brought about the observed pattern in the data than that of the factor analysis used as a method to establish the theory. Having felt that a system of economic variables would determine the pattern of another set of variables, one could think of directly establishing a connection between them without going through the stage of determining the factor pattern of the latter set and searching for agreement with the former. This led to the following logical problems concerning two sets of variates $x_1, x_2 \ldots x_s$ represented by x and $y_1, y_2 \ldots y_r$ $(s \geqslant r)$ represented by y:—

Problem (A): Whether y forms an *exhaustive set* for explaining the variations in x. We might conventionally and conveniently define a set such as y as exhaustive for x if at least 95 per cent. of the variation of x is explained by y.

Problem (B): Whether the set y forms *a minimal exhaustive set* in the sense that it does not remain an exhaustive set if one or more variables in it are not considered.

Problem (C): Whether there are any factors in the set x which are entirely independent of y.

In answering problem (A) we had only to fit regression functions of individual x's on the y's and examine how much variation was explained in each case. A table giving the ratios of variation due to y's to the total variation, similar to Table IV of Mr. Stone's paper, where he gave the sum of squares explained by the factors for each of the x's, supplied all the information relevant for the purpose. If y did not come out to be an exhaustive set, we must enquire how many factors were necessary besides the set y to get an exhaustive set and how one could extract them in a hierarchical order. This could easily be done by finding the principal components of the residual dispersion matrix. If the residual variables were represented by $x'_1, \ldots x'_s$ and the set by x' then we had to extract the principal factors, using x' in the manner adopted for x in Mr. Stone's paper. Further observable variables might have to be searched for in addition to y to explain important factors, if any, arising out of the residuals.

This method was of particular importance in dealing with time series, where one could always determine how the time factor was affecting the individual series. It might not be linearly connected with the variables. In fact, if one were dealing with a long series, the regression of a variable on time would be in the nature of a polynomial trend superimposed on which are periodic fluctuations capable of being represented by Fourier components. The complex pattern introduced by the time factor might be removed and the factors affecting the residuals might then be studied. If this were not done, and the usual method of factoring were followed, one might get a large number of important factors, many of which might have to be identified with the complexities, which were difficult to determine at this stage, introduced by a *single factor*, as, for instance, the time factor.

Problem (B) was important from the point of view of an economist who was seeking for a set of directly observable variables to explain the variations in another set so that even if the whole data could be represented by a fewer number of abstract factors, a larger number of directly observable variables might be necessary to interpret the abstract factors. One had only to satisfy that the set y did not remain an exhaustive set if a variable from this was not considered. This could be done by examining the ratios of variances explained by the whole set y and a subset of y. It might be noted that the factor pattern of x obtained by the method of principal components can give no indication of the nature or number of observable variables in the set y. When once y's are determined from other considerations factor analysis for merely establishing a connection does not seem to be necessary.

To answer Problem (C) one might proceed as follows: Let a_{ij} be the covariance between x_i and x_j and b_{ij} between x_i and y_j. We want to find the most important factor expressible as a linear function of the x's, which cannot be predicted from the observed set of y's with any degree of confidence.

If $\Sigma l_i x_i$, such that $\Sigma l_i^2, = 1$, is a factor, then we have to maximize

$$\lambda = \Sigma\Sigma l_i l_j \, a_{ij}$$

subject to the conditions

$$\Sigma l_i^2 = 1$$
$$\Sigma l_i b_{ij} = 0 \qquad \text{for } j = 1, 2, \ldots r.$$

The maximum value of λ is a root of the determinental equation

$$\begin{vmatrix} a_{ij} - \lambda & b_{rs} \\ b_{sr} & 0 \end{vmatrix} = 0.$$

This gives rise to an equation of degree $s - r$, and the factor corresponding to any root can be determined in the usual way. Only those factors whose variances are appreciable need be considered. Time here did not permit a lengthy discussion of these problems, and Mr. Rao hoped to treat them elaborately in a subsequent communication, where the intrinsic connections of these problems together with the use of canonical correlations of Hotelling would be considered. He was not, however, sure how far these problems would be of use in economics.

In this connection he might mention here that in the Indian Statistical Institute the method of principal components had been employed in slightly modified forms in various problems merely for reduction of data, i.e. to replace a large number of observations on an individual by a relatively few functions of these variables, which preserve the configuration of observations relating to a collection of individuals.

In one case this method had been employed to distinguish the racial traits of a number of castes and tribes living in the same geographical area. In another case families were classified according to expenditure patterns by taking a single function of the proportionate expenditures on various items. An interesting result had been established that expenditure patterns were not entirely differentiated by the level of expenditure and family composition. These problems were discussed in *Sankhya*, the Indian Journal of Statistics (Vol. 7, p. 425 and Vol. 8, p. 201).

Mr. HERDAN said that Mr. Stone had given an admirable example of how to apply factor analysis to economic variations.

There was one technical point he desired to mention in connection with Table VIII, which Mr. Stone had said he was not quite satisfied with himself. To compare the frequency distribution of the third residual sums of products with a normal distribution—as was done in that table—would be quite legitimate in the case of the Spearman or Thurstone analysis, both of which worked with specific factors. Anything which was not accounted for by the so-called common factors was lumped together in the residual or uniqueness portion of the variance which contained the specificity and the unreliability portion of the variable. The Hotelling analysis, however, worked with common factors only, all principal factors being here common factors. There was, strictly speaking, no room in it for the uniqueness and, consequently, for a random or unreliability portion of the variance.

He would, however, agree with stopping factorization, as Mr. Stone had done, after three common factors had been extracted from the variables, since they included by far the greatest part of the total variance. The remaining part of the variance was not due to a random element, but to the fact that the approximation, which is implied in duplicating a number of variables by as many principal factors, had not been carried out to the limit. He agreed that nobody would do this in practice. There would be little use in factor analysis if, for the description of 17 variables, we needed 17 common factors, but from a theoretical point of view it was difficult to see what could be done in the case of the Hotelling analysis to satisfy oneself that the analysis had been carried sufficiently far.

This applied in the present case all the more because for the communalities unity was inserted in the diagonal of the correlation matrix, which implies that it is intended to factorize the total variance and leave no residue whatever.

He would like to draw attention to the fact that whilst the first and second economic variables were positively correlated with the first and second factors respectively, the third economic variable was negatively correlated with the third factor, which was difficult to understand. He suggested that the Thurstone process of "rotation" might be used for obtaining a "simple structure" with only positive loadings.

It was not often that one met with factor analysis applied to a matrix of correlations containing also negative correlation co-efficients, and he would like to ask whether, applying the Hotelling analysis, the row sums were formed algebraically ?

Mr. STONE intervened to explain that it was formed algebraically, taking the signs into account.

Mr. HERDAN asked if, for the purpose of obtaining the latent root of the matrix, the division was carried out by the absolute highest row sum.

Mr. STONE agreed that this was so.

Professor ALLEN said that the main economic points had been well put by Mr. Champernowne and Dr. Barna, and he would like to say a few words from the point of view of the ordinary economist, attempting to understand the subtle work of statisticians like Mr. Stone—the kind of economist who had no mathematical flair and little knowledge of statistics, but a great enthusiasm for empirical investigation.

He believed such an economist had appreciated the applicability of the analysis of variance to economic data while recognizing that not so much could be expected as under controlled experimental conditions. Henry Schultz and many others had taught the economist how to use multi-variate linear regression analysis. But the margins of error of the resulting regression coefficients were so wide that the depressing conclusion emerged that significant estimates of such economic concepts as demand elasticities are unlikely to be obtained from such regression analysis. The economist then looked hopefully to the "bunch map" technique of regression analysis as developed by Professor Frisch and recently applied by Mr. Stone in a paper read before the Society.

He pointed to another method which was very much used in the U.S. in the 'thirties—for example by the economic statisticians of the Department of Agriculture—and which was a simple version of Mr. Stone's present method. In this method, one variable—perhaps a price or an output—was selected to be "explained" by other variables. A simple regression was first taken on the variable regarded as the most important of the "explaining" factors. Residuals from this regression were then related by a second regression on the next most important of the "explaining" variables, and so on.

This process, he thought, is open to obvious and severe criticism and, in the end, it reduces to a question of whether the user of the method, or anybody else, can know what "explaining" variables to take and in what order. Mr. Stone's present method is a big improvement on this; he allows the system itself to say which are the relevant factors, and he takes them all together instead of one by one. The difficulty is that the factors which turn up are purely arithmetical constructions which still need to be interpreted in economic terms. The step forward taken by Mr. Stone is to push forward the point when judgment and interpretation are needed to the very end of the analysis, where it should be.

One minor point, right off the paper, he wished to make concerned the terminology of variance and co-variance. He thought that exposition would be much easier if these terms could be used for sums, and not for means, of squares and products. But he thought that it was probably too late in the day to suggest such a reform.

Dr. GEARY said that he would intervene in the discussion only to make a remark on Mr. Champernowne's observations on Table IX of the paper. When he first read the paper he had been much struck by the results given in Table IX, and he could not agree with Mr. Champernowne that the correlations shown were to be regarded largely as arithmetical phenomena. Mr. Stone had succeeded in expressing many economic variables as linear functions of three factors, which factors were found to be highly correlated respectively with national income, with the first difference of national income and with time. Dr. Geary agreed that, since the factors were determined by maximizing processes, positive correlations found were to be expected, even if one were dealing with random series. The point of Mr. Stone's results was that the correlations found were so very high. The factor F_1 was a linear function of the original variables, and it was true the correlation between F_1 and national income would be exactly unity if all the coefficients were unity, since, as Mr. Champernowne said, the sum of these variables was twice the national income. Reference to the second column of Table IV would show, however, that the coefficients of the x_j in the expression for F_1 were very different from unity, and this was what rendered so remarkable the correlation of ·995 which was found. Nor did the algebraic process from which the second factor was derived appear to afford much ground for supposing that so high a correlation as ·948 would be found between that factor and the first difference of national income. In his (Dr. Geary's) opinion the results found were probably economically significant.

Mr. COHEN hoped that the meeting would permit him to raise some difficulties that might be answered relatively easily. There did seem to be one point that had not been adequately dealt

with. It had seemed to him that analysis of this kind raised the question whether the three factors were in fact independent and also whether they were really fundamental factors.

For instance, in what sense could one refer to delta income as an independent factor from income and time; it only meant that the dependence on the factors income and time was not linear. It was some form of more complicated function to which we might find a closer approximation by bringing in the second differential of income. In other words, there was not simply a linear relation to income, but a more complicated mathematical function, and it might be possible to find a better identification of that mathematical function than the division into these rather arbitrary categories.

He also wondered in what way one could refer to income as an independent factor in respect to time, and whether income was not in itself a function of time, and therefore they could possibly be reduced to some single, more fundamental factor.

The real point was, were not these factors simply a conglomeration of other factors? Income itself was merely a summation of the original items, and the fluctuations in income resulted from fluctuations in the original items with time. He wondered whether there could not be some more scientific method of determining the factors upon which the rest depended, and it might be that if these factors were not shown in a linear form, but in the form of algebraic or perhaps experimental functions, it might become a much easier process; they might reduce to two or one fundamental factor.

There was one further point. It seemed unnecessary to use the surface of a sphere for what was after all a two-dimensional diagram. A two-dimensional diagram ought to be able to be drawn on a plain surface.

The following written contribution was received after the meeting.

Miss N. Carruthers: I should like to draw attention to a note by Dr. C. E. P. Brooks (1927)* which outlines a simple exhaustion method of determining an approximate regression equation when many variates are involved. It seems to me that this method is very similar to that used in "factor analysis," although the method of selection of factors (or variates) differs slightly. Both methods are very useful for interpolation, but they are rather dangerous as means of deducing the causes of fluctuations in the dependent variable(s). As Mr. Stone himself points out, the three or more factors (or variates) which account for the greater part of the variance are not necessarily, or even generally, the primary causes of fluctuation of the variable.

Mr. Stone said he would like to reply very briefly, and to say what a great pleasure he had had in listening to the comments on his paper, which had given him a great deal to think about. It was getting late, and he would like, if he might, to think about the problems which had been raised at his leisure and to submit his comments in writing. It only remained for him to thank the Society very much for the way in which the paper had been received.

Mr. Stone subsequently submitted the following comments:

I agree to a large extent with the technical parts of Mr. Champernowne's generous opening to the discussion of my paper. In particular in some cases the coefficients of the three factors are almost certainly not all significant. The problem of testing significance is, however, especially difficult in this field, and it must be remembered that in my example the total variance of the observed variates is analysed without any deduction for the error components of the individual series. If, but only if, the error component of each of the series is the same, will the pattern coefficients and the factors obtained by the method which I used be the same as those which would be obtained from an analysis allowing for these error components as deductions from the leading diagonal of the matrix of sums of squares and products. But while in this case the results would be identical, their significance would depend on the importance of the error components. Without some knowledge of these it is difficult to discuss questions of significance in any precise terms.

It is perhaps worth noticing that at any rate in the above case, in which no allowance is made for the error components in the series, there is not in fact a simple complementarity between the number of factors needed to describe a set of variables and the number of linear regression equations connecting them, despite the fact that the former are associated with the larger and the latter with the smaller latent roots of the fundamental determinantal equation. The reason seems to be that the larger factors do not pick out the systematic variation, leaving all the error

* Brooks, C. E. P. (1927), *Regression Equations with Many Variates*. London: Air Ministry Met. Off. Prof. Notes, No. 47.

to the end, and that some error is in general taken out by each factor, even the largest. I may mention in passing that the relationship between factor and regression analysis and the associated problems of significance testing are at present being investigated at Cambridge by Dr. Geary.

Again, while I find myself in substantial agreement with Mr. Champernowne in the explanation he gives of the relationship between the factors and the independently calculated variables, I think that he over-estimates the inevitability of the high observed correlation between i and F_1 due to the high intercorrelations of the primary series. In this connection it is perhaps of interest to set out the 136 intercorrelations shown in my Table II in the form of a frequency table.

Range of correlation.	Frequency.
1·0–0·8	30
0·8–0·6	30
0·6–0·4	29
0·4–0 2	30
0·2–0·0	17

In this table the coefficients have been taken without regard to sign. It can be seen that there is a remarkably even spread of coefficients over the whole range, the smallest ones only being somewhat deficient. This table is not quite fair to Mr. Champernowne, because it is probably true that the variables with the largest variances are more highly intercorrelated than the set of variables as a whole. Nevertheless I think there is *prima facie* evidence of considerable divergence between the series, and do not see why a correlation of 0·97 should be expected between i and F_1. It is, of course, clear that a fairly high correlation in some undefined sense is to be expected.

My concluding remarks about the use of factor analysis to detect a set of inner variables were intended in the following sense: If no more than m factors are needed to describe a set of variates in a particular sample of observations, then that sample does not provide enough information to test regression equations involving more than m independent variates. It may thus seem permissible to try to simplify the original set, since a simpler one might be sufficient to reproduce the modes of motion observed in the sample.

I agree with Mr. Babington Smith that there are unfortunately few degrees of freedom available in my example. This is a common difficulty in econometrics arising from the limited period for which adequate estimates are available, and the virtual impossibility of increasing the amount of information by experimental methods. Nevertheless I believe it is instructive to discover the results of analysing the limited amount of material that is available. The close fit of the regression equations seems to me to indicate that over this limited period of observation at any rate the original variables showed comparatively little independent variation. I do not think this result is due to the use of covariances instead of correlations, since if this were the reason it would be surprising to find the small variables relatively so well explained. Nor do I think that the correlation coefficients in my Table V support the conclusion that the close fit is due to the preponderating effect of F_1.

The answer to Mr. Babington Smith's second point is almost certainly no, because a period of stability even if it were not quite so stable as to remove all variability would alter people's responses, and so change the quantitative relationships between the variables. I do not know how far even the relationships themselves would be affected.

On the third point, the estimates of the constituents are independent in the sense that none was derived indirectly by inserting the others in an equation of definition. On the other hand, several of the series had common statistical sources, and the continuity of the methods of estimation used from year to year would ensure some correlation between the errors of successive observations on the same series. In other words, in all this work systematic errors enter as well as random errors; indeed, the former may well be the more important.

Finally, it is undoubtedly true that what Mr. Babington Smith called a Q analysis would reveal a factor of neighbourliness between successive years, but I am not clear how far my F_2 can be regarded as the analogue of this factor in R analysis. May it not be that an explanation on the lines given by Mr. Champernowne is really the more satisfactory? In other words, does not Δi come into the picture because of the time displacement of the original series relative to a more or less common mode of variation reflecting the state of business activity or the national income?

It is encouraging to have approval of the technique adopted expressed by so high an authority as Sir Cyril Burt, particularly since the precise method is apparently not used much by psychologists. The length of the computations involved may well be a reason for not using Hotelling's method, but it can hardly be a reason for preferring correlations to covariances, since the use of

the latter actually simplifies the calculations. The explanation would seem to lie in the lack of a common unit for psychological tests similar to the dollar unit in my example. Of course the common unit of money might not be available in economic applications, as would occur for example if prices, interest rates, etc., were included among the variables to be analysed. Furthermore, if we were considering theories involving the analogue of trigger mechanisms, we might lose sight of the most significant features of the system by adopting money as a common unit and concentrating on sums of squares and products.

Mr. Barna seems to me to be too sweeping in his condemnation of multiple regression methods as applied to economic time series. There are certainly great difficulties, but I do not think my present paper does anything to add to them since it is hardly concerned with sampling problems at all. Without going over the ground of my earlier paper on demand analysis, it may be useful to indicate the connection between regression analysis and factor analysis by means of an example.

I shall take as an illustration the $p = 4$ series in my earlier analysis of spirits consumption. Suppose these are factorized completely using correlations since there is no common unit and, as in the present paper, making no allowance for errors in the series. Now if we make no assumptions about the error components we can come to no conclusions about the number of significant relationships connecting the variates; the factorization is simply an arithmetical process. Since I have no estimates of the variances, V_i, of the error components of the four series, I shall assume for the purposes of this illustration that they are all equal. This assumption leaves the ratios of the latent roots, λ, unaffected and makes it possible, using some of Tintner's results, to discover the minimum common error variance, V, compatible with different assumptions about the number of relationships, R, assumed to connect the four variates. For, as Tintner has shown, if N is the number of sets of observations, then $(N-1)V^{-1}$ times the $(R+1) = r$ smallest λ's must be equal to or greater than the value of χ^2 at some accepted critical point with $(N-1-p+r)r$ degrees of freedom.

In the above example $N = 19$, $p = 4$ and r may range from 1 to 4. The following table brings together the relevant calculations. χ^2_c stands for the critical (5 per cent.) point of χ^2:

	$(N-1)\sum_{s=p}^{s=p-R}\lambda_s$	$(N-1-p+r)r$	χ^2_c	$V^{-1}_{(\min.)} = \chi^2_c/(N-1)\sum_{s=p}^{s=p-R}\lambda_s$	$V_{(\max.)}$
1	0·126	15	25	200	0·005
2	1·062	32	45·	42	0·024
3	8·892	51	72	8	0·125
4	72·000	72	97	1·35	0·741

In fact it is reasonable to expect that $R = 1$, i.e. that there is one and only one relationship, the demand relationship connecting the four variables. From the table we see that for $r = 2$ the maximum value of V is 0·024, i.e. that if our assumption is true, at most $2\frac{1}{2}$ per cent. of the variances of the four variables is ascribable to error. This seems a reasonable figure. The other values of V in the table on the other hand do not seem reasonable. With $R = 0$, V is absurdly small, i.e. we must have very high expectations of the accuracy of our series if we are not satisfied with the fit we get, while with $R > 1$, V is almost impossibly large, i.e. the series can hardly be so inaccurate that we could be satisfied with the fit of more than one equation connecting them or any subset of them. This result agrees with my original analysis based on bunch maps, though there are a great many assumptions involved.

The point of this example is to show that before we start talking about significance we must introduce the idea of error components. I have not done this in my present paper, and do not think therefore that my results provide any clue to the interpretation of other analyses in terms of statistical significance.

I agree entirely with Mr. Barna about the use of information other than that derived from time series. The case he mentions, budget data, is not as simple as it looks, however, because these data are rarely tabulated, so that the other determining variables, family size, age of head of household, etc., can be inserted in the equation used to explain consumption. In such a case it may easily happen that the simple regression between consumption and income is highly misleading, as has been shown by Haavelmo.

The dangers in excluding the size of the sample due to institutional changes can to some extent be met by a device often employed by Tinbergen of inserting a variable with the value 0 up to the date of the change and with the value 1 after that date which will catch any simple discontinuity of the type to which Mr. Barna referred. More complicated cases must in principle

involve setting up a system of equations and studying the effect on the system when one or more of these or their coefficients are changed.

I was interested in Mr. Rao's contribution to the discussion, but will confine myself to one point. In my example it happened that the series which I expected to be capable of describing the original data were in fact able to do so. Had they not been, I could have used the factor analysis to investigate other possibilities.

I agree with Mr. Herdan's main contention, which is, I think, closely connected with the views expressed above in commenting on the remarks of Mr. Champernowne and Mr. Barna. I do not know how to interpret the residuals from an analysis into Hotelling factors, and tried to make this clear in my paper.

Mr. Cohen's comment raises a fundamental question, but one which is a little beyond the limited scope of my paper. I think it is helpful to express certain variables in terms of others without necessarily having a complete theory of the interrelationships of the latter.

THE PRINCIPLES OF BIOLOGICAL ASSAY

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1. INTRODUCTION

"BIOLOGICAL assay, as carried out by the majority of workers in the world, still remains a subject for amusement or despair, rather than for satisfaction or self-respect. We have cat units, rabbit units, rat units, mouse units, dog units, and, latest addition of all, pigeon units. The field of tame laboratory animals having been nearly exhausted, it remains for the bolder spirits to discover methods in which a lion or elephant unit may be described." So wrote Professor J. H. Burn (1930), himself one of the leading British exponents of methods of biological standardization. He went on to explain that the fault lay not with the concept of biological assay, but with those users of it who "are still ignorant of certain principles which during the past few years have been shown to be capable of transforming this whole subject from the plane of an insidious means of self-deception to that of a well-ordered and progressive science." The pioneer work of Bliss, Fieller, Fisher, Gaddum, Irwin, Tattersfield, Trevan, to name only a few of those who have contributed to the development of the theory and practice of biological assay, has entirely changed the situation. Biological techniques are now widely accepted, not merely as unavoidable evils, but as sound methods for many types of estimation that cannot easily be made by direct chemicai or physical analysis. Indeed, when both chemical and biological methods are available for the same estimation, the generally greater precision of the former does not necessarily make it the one to be preferred; if the final result is required in terms of the biological potency of a complex material, the greater specificity of a biological assay may give it an advantage over a laborious chemical procedure (Bacharach, 1945).

When I was asked to prepare a paper on biological assay for the Research Section, I had to bear in mind that in recent years the Section has already received two papers on this subject. Dr. Irwin, in 1937, gave a comprehensive survey of the application of statistical methods to various problems of biological assay, and Mr. Fieller, in 1940, gave a paper which, though primarily concerned with the assay of insulin, described aspects of assay design and analysis useful for many other purposes. Since these papers were written, there have been further developments in the use of statistical methods for assays, but mostly in matters of detail rather than in fundamentally new ideas; an account of them might interest and help biologists, but to statisticians would perhaps seem a statement of the obvious. Instead, I have chosen this evening to review the statistico-mathematical principles underlying a good biological assay, and to show the essential unity of the different types of statistical analysis commonly used for assay data. Though biologists have enunciated the biological conditions requisite for a sound assay, so far as I can discover statisticians have never explicitly stated their conditions; a paper by Bliss (1940) goes some way towards this, but he does not there discuss statistical analysis in detail. The requirements of the biologist and of the statistician are in the main merely different aspects of the same truth, but more frequent attention to the statistical formulation might prevent some of the loose thinking that often occurs in discussions of assays. In order to avoid misunderstanding, I should point out that this paper has been written primarily for statisticians, and I have stressed the matters which seem to me of greatest statistical interest; had I been writing for an audience of biochemists or pharmacologists, or of those who must base commercial policy and actions on the findings of biologists and statisticians, though the mathematics would be unaltered I might have placed my emphasis rather differently.

What is a biological assay? In an excellent review of recent literature, Bliss and Cattell (1943) say: "Biological assays may be defined as determinations of potency or toxicity based

upon the reaction of living matter, including biological reactions not involving intact cells, such as serological tests *in vitro*." Thus a comparison of the relative potencies of two samples of insulin by means of the changes they produce in the blood sugar of rabbits is a biological assay; the emphasis must be placed on the estimation of potency, and a similar experiment in which the chief interest lay in the effect of insulin on the rabbit would not be an assay. Again, an experiment on the increase in potato yield caused by nitrogenous fertilizer would not generally be considered an assay; nevertheless, if the data were to be used for estimating the amount of a standard sulphate of ammonia equivalent to one unit of another fertilizer, the experiment would then come within the terms of the definition just given. The typical assay involves two components, a *stimulus* (e.g. a drug, a poison, a vitamin) and a *subject* (e.g. an animal, a plant, a bacterial culture). The potency of the stimulus is assessed by means of the effect it produces on a selected measurement of the subject (e.g. weight of subject or of a particular organ, percentage blood sugar, or a quantal "measurement" such as death); this measurement is the *response*, and, before a particular reaction of stimulus and subject can be made the basis of an assay, something must be known of the relationship between the *dose* of stimulus and the magnitude of the response. A stimulus can then be assayed either absolutely from a known dose-response curve, or, more commonly, by the comparison of doses of different stimuli which produce equal responses. Failure to distinguish between the needs of the preliminary investigation of the dose-response law and those of an assay to be used as a routine analytical method has sometimes confused the discussion of efficiency in assay design.

In the paper from which I have already quoted, Burn stated five principles of a good assay technique; he was writing as a biologist, but his conclusions are closely related to those I intend to put before you as the statistician's principles. Firstly he emphasized the much greater reliability of potency estimates obtained by comparison of the material to be assayed with a standard, as against attempts to measure absolute potency in "animal units," and the need for taking due account of variations between individual test subjects. In this he followed Trevan's (1927) demonstration of the misleading deductions that may be drawn from tests on small numbers of subjects unless sound statistical techniques are used. He then specified conditions which must be satisfied by the dose-response relationship if a valid assay is to be based upon it; these are biological versions of what, in the next section, I call Conditions I and II. I propose this evening to state these principles in statistical terms, then to show general methods for the estimation of relative potency and to summarize those at present in use for quantitative and quantal responses, and finally to discuss the planning of assays for obtaining potency estimates of maximum precision.

2. The Relationship of Dose and Response

The stimulus in any assay will be a dose of either a standard or a "test" preparation containing the substance to be assayed—for example, a dose containing either a known amount of nicotinic acid or a known amount of a food-stuff whose (unknown) content of the vitamin is to be assayed. A biologically valid assay must be such "that the response supposed to be produced by the known amounts of 'factor X' is actually due to the factor itself and not to some other substance associated with it, e.g. an impurity; and that the response produced by the material to be analysed (assayed) is also due solely to the presence in it of 'factor X,' without augmentation, diminution, or modification by any other substance also present. In other words, if we use the terms 'Standard Preparation' and 'Test Preparation' to denote respectively the solution of allegedly pure 'factor X' and the solution prepared from the material to be analysed, we assume that the Std. Prep. contains no substance, other than factor X itself, contributing to the response we measure, and that the Test Prep. behaves for the purposes of the analysis so similarly to the Std. Prep. that it may be regarded simply as a dilution of the Std. Prep. in a completely inert diluent" (Wood, 1946*a*). No statistical process can distinguish between Wood's "factor X" and a chemically different substance which affects the measured response in the same manner—a self-evident truth which nevertheless is sometimes forgotten. On the other hand, the statistician must examine the second part of Wood's statement, his "hypothesis of similarity," which is equivalent to Condition II below.

Suppose that a test subject is given a dose z of a stimulus and that, when the dose has had time to produce its effect, a response u is measured on the subject. For a given population of subjects, tested under constant experimental conditions, the expected response to any dose may be written—

$$E(u) = U, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where U is a regression function of the form—

$$U = F(z) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If this relationship is to be made the basis of an assay, U must be a single-valued strictly monotonic function of z, at least over the range of doses to be used (Condition I); the function need not be continuous, though in practice it will usually be so. Without loss of generality, U may be taken as an increasing function of z.*

If a standard and a test preparation both satisfy this condition, and have regression functions—

$$U_s = F_s(z), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$U_t = F_t(z), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

then, for any selected value of U within the range of validity, doses z_s, z_t can be uniquely determined such that the expected values of u for these amounts of the two preparations are both U. Hence, at this level of response, the second material may be said to have z_s/z_t times the potency of the first.

If the two preparations each contain the same effective constituent (or the same effective constituents, in constant proportions) and all other constituents are without effect on U, so that the less potent preparation behaves as though it were a dilution of the other in a completely inert diluent, this ratio must be independent of U, for it represents the relative amounts of the effective constituent in equal doses (Condition II). Equation (4) can then be written—

$$U_t = F_s(\rho z),$$

where ρ, a constant, is the potency of the second preparation relative to the first.

The fundamental assay problem may thus be stated as follows: A standard preparation, whose content of the effective constituent either is known or is defined in terms of arbitrary units, has a dose-response regression function,

$$U_s = F(z), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which is subject to Condition I above. A test preparation is known to have the same effective constituent, and the amount present in unit dose is to be estimated, making use of the fact that for this preparation—

$$U_t = F(\rho z). \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The required content of the effective constituent per unit dose is ρ times the content per unit of the standard preparation, where ρ is a parameter which has to be estimated from the measured responses of a sample of test subjects.

If the data cannot adequately be described by the same form of $F(\ )$ for both preparations, the basic assumption that only the same effective constituents were concerned in both must be false, unless the conditions of testing of the two preparations were not maintained sufficiently nearly constant. The assay is therefore invalid, either because of the inherent incommensurability of the substances or because of the non-comparability of the tests. Except for sampling variation, all

* It is conceivable that an assay technique might be based on a regression function not satisfying Condition I; even a function showing maxima and minima might be fitted to the data for the two preparations, and, providing that Condition II were satisfied, a relative potency could be determined as the magnification of the one curve relative to the other, though there would not necessarily be a unique dose of the one preparation of potency equivalent to a specified dose of the other. This situation seems scarcely likely to be of practical importance.

valid assay procedures for the same test preparation should give the same estimate of potency, irrespective of the test subject, the measurement chosen as the response, and the conditions of testing.

On the other hand, the fact that tests of two preparations give responses whose measurements can be satisfactorily fitted by equations such as (5) and (6) is in no way a demonstration that they have a common effective constituent. Various toxic compounds found in derris root (rotenone, elliptone, toxicarol, etc.) have shown constant relative potencies in insecticidal sprays used against *Aphis rumicis* (Martin, 1940, 1942), yet they are of different (though related) chemical structure. Even preparations of widely different chemical composition may, under particular conditions of test, act in conformity with equations (5) and (6); their relative potencies might still be useful as an expression of their behaviour under these conditions, but would not necessarily have any wider applicability. Wood (1944*b*) has drawn attention to the difference between these two types of assay, with special reference to the choice of experimental design.

In most assays, though the form of the function $F(\ )$ may be known, its specification will contain one or more parameters which have also to be estimated from the data. The experimenter would often like to determine a dose-response curve for his standard preparation and assay subsequent test preparations by reference to this curve. This procedure appears to have the considerable advantage that the standard curve could be determined with high precision, and in effect only the results for the test preparation would have an appreciable error. Unfortunately, the standard curve for many stimuli and test subjects does not remain constant in position, or even in slope and other characteristics of shape, but varies from one assay to another, probably because of uncontrollable differences in the experimental conditions and in the laboratory stock of test subjects. Bliss and Packard (1941) have described one example of a dose-response relationship which remained constant over a long period, namely the mortality of eggs of *Drosophila melanogaster* exposed to Roentgen rays; biological assays of this type of radiation might be based satisfactorily on a standard curve. But such stability of the standard curve is exceptional, and generally an assay procedure must involve simultaneous or consecutive tests of the standard and test preparations, in order that the results may be truly comparable. The optimal distribution of labour between the two will be considered in Section 7.

The theoretical basis of the statistical analysis when the parameters of $F(\ )$ are known need not be discussed separately from the more usual situation of unknown parameters, of which it is mathematically a degenerate case. The most important class of dose-response relationships comprises those which may be written in a modified form of (5)—

$$U_s = f(\alpha + \beta z^\lambda) \qquad (7)$$

where $f(\ )$ is again a function satisfying Condition I, and one whose parameters are all known, λ is a known parameter, and α, β require to be estimated from experimental data. As a limiting form of (7), corresponding to $\lambda = 0$,

$$U_s = f(\alpha + \beta \log z) \qquad (8)$$

may be included. A new scale of measurement of dose may be defined by transformation of the original z to—

$$x = z^\lambda, \qquad (9)$$

or, for equation (8),

$$x = \log z, \qquad (10)$$

so that

$$U_s = f(\alpha + \beta x); \qquad (11)$$

Finney (1947) has called such a transformed measure of dose the *dosage*, but perhaps the term *dose metameter* (Bacharach *et al.*, 1942) is less likely to prove confusing. If now a *response metameter* is defined by—

$$Y = f^{-1}(U), \qquad (12)$$

equation (11) may be re-written—

$$Y_s = \alpha + \beta x. \qquad (13)$$

When equation (6) is written in terms of the same metameters, it becomes—

$$Y_t = \alpha + \beta\rho^\lambda x \qquad (\lambda \neq 0), \quad . \quad . \quad . \quad . \quad . \quad (14.1)$$

$$\text{or } Y = \alpha + \beta \log \rho + \beta x \quad (\lambda = 0) \quad . \quad . \quad . \quad . \quad (14.2)$$

In this way the two dose-response relationships are represented as straight lines. If $\lambda \neq 0$, the two lines intersect at $x = 0$, and the relative potency is the λ-th root of the ratio of the slopes of the two lines. If $\lambda = 0$, the two lines are parallel, and the logarithm of the relative potency is the distance between them measured parallel to the x-axis. Whatever the value of λ, the most convenient method of estimating ρ is first to determine the two best-fitting straight lines in the (x, y) plane, subject to the constraint either of equality at $x = 0$, or of parallelism, and then to calculate R, the estimate of ρ, as the appropriate function of the ratio of slopes or of the distance between the lines. The acceptance of a standard dose-response curve determined with high precision from previous data, as in the example quoted from Bliss and Packard, imposes the additional restriction on equation (14) that α, β are already known and the only parameter left to be estimated is ρ; intermediate cases in which one of α and β is already known may be recognized, but are not of great practical importance.

A more general problem would, of course, be that which required λ also to be estimated from the data of each assay. The maximum likelihood equations then become much more complicated than those developed in Section 3; an example of the computations has been given by Finney (1947). Fortunately in many types of assay there is considerable evidence either that $\lambda = 1$ or that $\lambda = 0$ over a wide range of doses, and that departures from the usual value are indications of an unreliable assay rather than of true variations in λ. Preliminary investigations will often establish some simple value of λ as the normal for a particular assay technique, after which this value may be adopted for future routine assays unless the accumulation of further data indicates this assumption to be unjustifiable.

3. ESTIMATION OF RELATIVE POTENCY

When the dose-response relationship is of the type represented by equations (7) or (8), the relative potency of two preparations may be estimated from measurement of the response of test subjects at several dose levels of each preparation. If no assessment of the precision of the estimate is required, a graphical estimation procedure may be sufficiently exact. For the preliminary examination of results, and for some routine purposes when the consequences of occasional mis-statements of potency are not very serious, possibly nothing more than rapid graphical analysis will be needed; this is also often required as a first step towards a more complete analysis. In the graphical method each observed response, u, is transformed to a response metameter, y, according to equation (12); y is then plotted against the dose metameter, x. If x is a power of the dose, as in equation (9), the series of points for the two preparations should lie on two straight lines constrained to have the same value at $x = 0$; these lines may be drawn by eye and an estimate, R, of the relative potency obtained by equating the ratio of their slopes to R^λ. If x is the logarithm of the dose, as in equation (10), the two series of points should lie on two parallel lines; again the lines may be drawn by eye, and R is then given by equating the horizontal distance between the lines to $\log R$.

With experience, drawing these lines by eye can be made to give estimates of potency very little different from those obtained by computation, especially if some regard is paid to the major differences in the weights to be attached to the points (see below). For most types of assay, however, the precision of the potency estimate has to be assessed before important decisions relating to the material assayed can be taken. Though calculations of precision could be based upon measurements of individual deviations from lines fitted by eye, such a course has little, if any, advantage over a complete arithmetical analysis of the kind now to be described.

This section is written primarily in relation to responses which are quantitative measurements; the similar methods for quantal responses will form the subject of Section 5. Before a full analysis can be begun, some assumption must be made about the variation in response shown by test subjects receiving the same dose. Individual responses, u, may often be assumed normally distributed about their expected value, U; as is well known, small departures from normality

seldom have any serious effect, especially when (as here) the most important conclusions will be based upon means, the distributions of which will approach more closely to the normal form than those of the original observations. Furthermore, u may be assumed to be measured on a scale on which its variance is, practically speaking, independent of U; some simple preliminary transformation may be necessary to ensure this, and will not seriously interfere with the normality condition.* The variance per subject, σ^2, can then be assessed from the sum of squares of deviations of u "within dose-groups."

The method of maximum likelihood (Fisher, 1922, 1925), here equivalent to the method of least squares, may be applied to the estimation of the unknown parameters, α and β, in the dose-response relationship for a single preparation. The estimates are determined from the data so as to minimize the quantity—

$$L = \frac{1}{2\sigma^2} S(u-U)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

with respect to variations in α and β. Suppose that a_1, b_1 have been obtained, from a diagram or in any other way, as first approximations to the estimates. Then by the Taylor-Maclaurin expansion, to the first order of small quantities, improved values will be $a_2 = a_1 + \delta a_1$, $b_2 = b_1 + \delta b_1$, where the increments δa_1, δb_1 are the solutions of

$$\left.\begin{aligned} \frac{\partial L}{\partial \alpha_1} + \delta a_1 \frac{\partial^2 L}{\partial \alpha_1^2} + \delta b_1 \frac{\partial^2 L}{\partial \alpha_1 \partial \beta_1} = 0 \\ \frac{\partial L}{\partial \beta_1} + \delta a_1 \frac{\partial^2 L}{\partial \alpha_1 \partial \beta_1} + \delta b_1 \frac{\partial^2 L}{\partial \beta_1^2} = 0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

In these equations, the addition of the suffix to α, β is intended to indicate that after differentiation the first approximations are substituted. The three second-order differential coefficients may be replaced by their expected values, so that the equations become

$$\left.\begin{aligned} S\left(u-U_1\right)\frac{\partial U}{\partial \alpha_1} - \delta a_1\, S\left(\frac{\partial U}{\partial \alpha_1}\right)^2 - \delta b_1\, S\left(\frac{\partial U}{\partial \alpha_1}\cdot\frac{\partial U}{\partial \beta_1}\right) = 0 \\ S\left(u-U_1\right)\frac{\partial U}{\partial \beta_1} - \delta a_1\, S\left(\frac{\partial U}{\partial \alpha_1}\cdot\frac{\partial U}{\partial \beta_1}\right) - \delta b_1\, S\left(\frac{\partial U}{\partial \beta_1}\right)^2 = 0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (17)$$

where $U_1 = f(a_1 + b_1 x)$. By the use of equation (12), and introducing the *weighting coefficient*

$$w = \left\{f'(Y)\right\}^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

these equations may be rewritten

$$\left.\begin{aligned} \delta a_1\, Sw_1 + \delta b_1\, Sw_1 x = S\frac{w(u-U_1)}{f'(Y_1)} \\ \delta a_1\, Sw_1 x + \delta b_1\, Sw_1 x^2 = S\frac{wx(u-U_1)}{f'(Y_1)} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (19)$$

If now a *working response*, y, is defined by

$$y = Y + (u-U)/f'(Y), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

* Mr. E. C. Fieller has kindly shown me the typescript of an interesting paper (Fieller, 1947) in which he emphasizes the importance of a transformation which will equalize variances. Unfortunately I have seen his paper too late to be able to take full account of it in my own. Use of this transformation does not seem to be inconsistent with the method of analysis given here, for it amounts only to a definition of the scale in which the responses are considered to be measured. The further transformation to a response metameter, equation (12), is then adopted as an aid to computation, but, since compensatory changes will be made in the weights attached to observations, the estimates eventually obtained will be unaltered; the second transformation, in fact, is no more than a device which helps forward the systematic solution of the maximum likelihood equations, and which happens also to give a convenient linear representation of the data.

equations (19) show that the weighted linear regression of y_1, the working response calculated from the first approximation, on x is

$$Y_2 = a_2 + b_2 x, \qquad (21)$$

a new approximation to the maximum likelihood estimate of the dose-response relationship; the weight of each observation in the regression calculations is the appropriate w_1.

The procedure for the estimation of α and β from experimental data may thus be put into the form of a successive-approximation method ; the outline of this method was first given, for a special case, by Fisher (1935), and the theory was developed more fully by Garwood (1940). When the same response transformation has to be used often, the computations can be expedited by preparing a special set of tables. Firstly a table of Y as a function of U and secondly tables of the weighting coefficient w, the *minimum working response*, $\{Y - U/f'(Y)\}$, and the *range*, $1/f'(Y)$, in terms of the argument Y, are required; the names are given as analogues to the terminology of probit analysis, though the one is not a true minimum if negative responses can occur and the other is not a range in any obvious sense. The first step in the calculation is to read from the first table the values of y corresponding to each measured u, and to plot these against the dose metameter x. On this diagram a provisional estimate of the line (13) is then drawn by eye and the "expected" values of Y corresponding to each dose determined from the line. To the minimum working response corresponding with each Y is added the product of the range with the observed u; the resulting working response, y, is assigned the weight, w, appropriate to the value of Y, and the weighted linear regression of y on x is calculated in the ordinary manner. If this line differs appreciably from the provisional line, it may be used as a new "provisional" and the cycle of computations repeated.

The procedure requires very little adaptation for use in an assay. The empirical results for tests at various dose levels of the standard and test preparations are plotted in the (x, y) plane. Two provisional lines are then drawn by eye, subject to the condition that they shall intersect at $x = 0$ if the dose metameter is $x = z^{\lambda}$, or that they shall be parallel if the dose metameter is $x = \log z$. Working responses and weights are found just as for the single line, and improved approximations to the parameters obtained from the two weighted linear regressions of y on x; the regressions are now calculated subject to the condition either of intersection at $x = 0$ or of parallelism. As in the purely graphical process, the estimate of relative potency is eventually formed either from the ratio of slopes of two lines or from the horizontal distance between two parallel lines.

In the sense in which the term has so far been used, a biological assay is a method of estimating the composition of the test preparation; apart from sampling errors, the same result should be attained whatever experimental technique is used, independently of choice of test subject and measurement, provided only that the measurement made satisfies Conditions I and II. Bliss and Cattell's definition also includes assays of relative potency of, for example, two different methods of application of a poison in which the result need not be independent of the test subject or of the technique chosen. Thus Tattersfield and Potter (1943) found the relative potencies of two different ways of applying a rotenone spray to the chrysanthemum aphis to be different when a fixed spray deposit was applied at different rotenone concentrations from what it was when a fixed concentration was applied at different amounts of deposit. Even in these circumstances, experimental results may usefully be summarized in terms of relative potencies, and the statistical procedure will be the same as that needed for the analytical type of assay. The numerical example in Section 4 is an analytical assay, that in Section 5 is an assay only in the wider sense.

4. QUANTITATIVE RESPONSES

When the response is a quantitative measurement on individual subjects, experience has shown that for a wide variety of circumstances the response itself is linearly related either to the logarithm of the dose or to a power of the dose (usually the first power). If the response metameter is identical with the response,

$$f(Y) = Y \text{ and } f'(Y) = 1,$$

so that the weighting coefficient is unity and the working response is the same as the empirical response. There is then no need for any successive approximation process, and each observation is given unit weight in the calculation of regressions of y on x for each preparation.

Assays in which the response is linearly related to the logarithm of the dose are the commonest type in use to-day. Such a relationship can hold only over a limited range of responses, and must exclude zero dose. Irwin (1937) and Fieller (1940) have already shown this Society examples of their use, and they are now widely applied to the assay of vitamins, drugs, hormones, and other materials. By ordinary regression technique, two parallel straight lines are fitted to the data:

$$\left.\begin{aligned} Y_s &= a_s + bx_s, \\ Y_t &= a_t + bx_t. \end{aligned}\right\} \qquad (22)$$

Comparison with the "expected equations" (13), (14.1) then gives R as the estimate of ρ, where

$$\log R = M - (a_t - a_s)/b. \qquad (23)$$

Even when the main analysis is to be arithmetical rather than graphical, a rough diagram of the dose-response relationship for both preparations is often valuable, as it may reveal irregularities or trends which would escape notice in an uncritical application of routine arithmetic.

The calculations are often most conveniently carried out with the aid of an analysis of variance; in order that the assay may be valid there must be no indication of departure from linearity for either preparation (as evidenced by comparing the residual variation of the mean responses for each dose with the variation amongst individual responses for a fixed dose) and no indication of a departure from parallelism. If s^2 is the "within doses" variance obtained in this analysis, the variance of M may for some purposes be taken as

$$s_M^2 = V(M) = \frac{s^2}{b^2}\left[\frac{1}{N_s} + \frac{1}{N_t} + \frac{(M - \bar{x}_s + \bar{x}_t)^2}{S_{xx}}\right], \qquad (24)$$

when N_s, N_t are the total numbers of observations for the standard and test preparations, $\bar{x}_s$ and $\bar{x}_t$ the weighted means of x, and S_{xx} is the sum of squares of deviations for x used in the calculation of the common regression coefficient, b. Strictly speaking, the variance of a ratio of two normally distributed quantities is infinite, and equation (24) is an approximation of limited usefulness; it can be used for assigning fiducial limits to M only if b is large compared with *its* standard error at the level of probability chosen. The variance of b is

$$V(b) = s^2/S_{xx}; \qquad (25)$$

if t is the deviate for the chosen probability level, with the number of degrees of freedom on which s^2 is based, and

$$g = s^2t^2/b^2S_{xx}, \qquad (26)$$

the exact fiducial limits may be found as

$$M + \frac{g}{1-g}(M-\bar{x}_s+\bar{x}_t) \pm \frac{st}{b(1-g)}\sqrt{(1-g)\left(\frac{1}{N_s} + \frac{1}{N_t}\right) + \frac{(M-\bar{x}_s+\bar{x}_t)^2}{S_{xx}}} \qquad (27)$$

which tend to $M \pm s_M t$ when g is small. This result is analogous to one developed by Bliss (1935*b*) for the fiducial limits of the median lethal dose in the analysis of quantal response data; the extension and general application to the assessment of precision in all types of assay seems first to have been appreciated by Fieller (1940). Conclusions based on equation (24) may be very misleading unless the condition that g shall be small is satisfied, and users of the assay technique should always assess their limits of error by means of (27) except when they are certain that the simpler calculations will lead to essentially the same results. No examples of the familiar pattern of computations for this type of quantitative response assay need be given here.

In recent years microbiological techniques have been developed for the assay of vitamins and other substances. The test subject is then no longer a single animal, but is a tube containing a basal medium plus a dose of the standard or test preparation. The tube is sterilized and, after

the addition of a fixed amount of a standard bacterial culture, is incubated under standard conditions of time and temperature. The bacterial growth in the tube is then assessed by a measurement of acidity, turbidity, or other quantity closely correlated with growth; this measurement constitutes the response.

Experience has shown that, with a carefully controlled biological technique and a suitable basal medium, some types of micro-biological assay usually show a linear relationship between the response and dose from zero dose up to a moderately large quantity, though the addition to the basal medium of a very small amount of the factor under assay may be necessary to ensure linearity (Wood, 1946*a*). For other assay techniques more complex relationships occur. These have not yet been studied at all completely; some appear to fall within the class defined by equation (11), and linear relationships between the logarithm of the response and the logarithm of the dose have recently been reported (Wood, 1946*b*). Once the form of the response and dose transformations have been settled, the general theory of Section 3 may be applied. For practical purposes, in this and other types of assay, the correctly weighted full analysis may not always be needed. If the data agree well with a linear relationship between dose and response metameters, the fitting of unweighted linear regressions will scarcely affect the estimate of potency; estimates of variance, fiducial limits, and tests of assay validity, however, may be more seriously affected. The merits and demerits of this simplification in the arithmetic will need to be examined more closely when a greater amount of evidence on the form of transformations to response metameters has accumulated.

Only the simple form of analysis, in which no transformation of dose or response is made, will be discussed in detail here. The responses obtained with a series of doses of the two preparations have then to be fitted by the equations:

$$\left.\begin{aligned} Y_s &= a + b_s x_s, \\ Y_t &= a + b_t x_t. \end{aligned}\right\} \qquad (28)$$

"Blank" or zero-dose tubes usually will be included in the test, and these give a direct estimate of α. Comparison with equations (13), (14.2) shows the estimate of ρ to be

$$R = b_t / b_s. \qquad (29)$$

The statistical technique for dealing with data from this type of assay has been described by Finney (1945); more detailed formulae have been given by Bliss (1946) and by Wood and Finney (1946). The appropriate procedure may be considered as the fitting of a bivariate regression equation

$$Y = a + b_s x_s + b_t x_t \qquad (30)$$

to the whole of the data, every observation having either $x_s = 0$ or $x_t = 0$. Comparison of the residual variation "between doses," after elimination of the regression component, with the variance "within doses," again in an analysis of variance, gives a comprehensive significance test of the deviations from linearity and hence of the validity of the assay. The computations are illustrated in the following example.

Example: Kent-Jones and Meiklejohn (1944) have given the detailed results of an assay of nicotinic acid in a meat extract. Five concentrations of a standard nicotinic acid preparation and three of a solution prepared from the meat extract were inoculated (in duplicate tubes) with a standard culture of *Lactobacillus arabinosus;* two blank tubes were also inoculated. After incubation at 37° C. for 72 hours, each tube was titrated with N/14 sodium hydroxide to give a measure of acidity. The results of the titrations are shown in Table 1, and a diagram (Fig. 1) shows the two series of points to lie nearly on two straight lines.

The normal equations for b_s, b_t are found in the usual manner as

$$\begin{aligned} 0{\cdot}15b_s - 0{\cdot}75b_t &= 2{\cdot}34, \\ -0{\cdot}75b_s + 10{\cdot}00b_t &= 7{\cdot}65; \end{aligned}$$

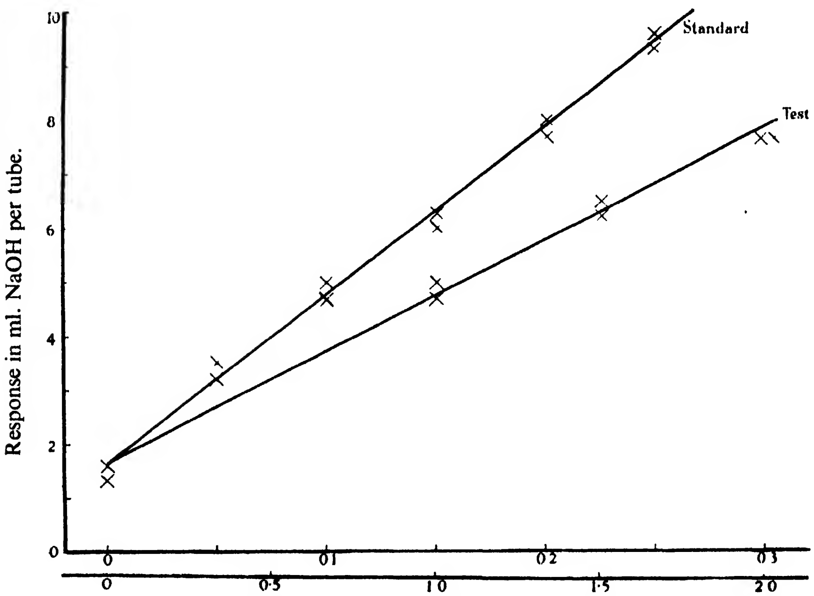
TABLE 1

Assay of Nicotinic Acid in Meat Extract

(Data of Kent-Jones and Meiklejohn)

μg. Nicotinic acid per tube (x_s).	ml. NaOH (y).		ml. solution per tube (x_t).	ml NaOH (y).	
·05	3·5,	3·2	1·0	4·9,	4·8
·10	5·0,	4·7			
·15	6·2,	6·1	1·5	6·3,	6·5
·20	8·0,	7·7			
·25	9·4,	9·5	2·0	7·7,	7·7

Blank tubes ($x_s = x_t = 0$) : 1·5, 1·4 ml. NaOH.



Upper scale: Dose of nicotinic acid in μgm. per tube.
Lower scale: Dose of meat extract solution in ml. per tube.

FIG. 1.

Response diagram for assay of nicotinic acid in a meat extract.

the numerical values here are exact. The inverse matrix of the coefficients, to be used also in obtaining the variance of the estimated potency, is

$$V = \begin{pmatrix} 10{\cdot}6 & 0{\cdot}8 \\ 0{\cdot}8 & 0{\cdot}16 \end{pmatrix},$$

whence

$$b_s = 31{\cdot}08,$$
$$b_t = 3{\cdot}096,$$

and therefore

$$R = 0{\cdot}0996.$$

This value of R is expressed as µg. nicotinic acid per ml. of test preparation, and, as 1 gram of the original meat extract was contained in 5000 ml. of the solution, the potency of the extract is estimated to be 498 µg. per g.

TABLE 2

Analysis of Variance for Nicotinic Acid Assay

	d.f.	Sum of squares.	Mean square.
Linear regression	2	96·412	
Deviations from linearity	6	0·278	0·0463
Between doses	8	96·690	
Within doses	9	0·175	0·0194
Total	17	96·865	

Table 2 shows the analysis of variance for these data: the mean square for deviations from linearity, though larger than the error mean square, does not indicate any significant non-linearity. The error mean square is

$$s^2 = 0{\cdot}0194,$$

and the product of this with the elements of V gives the variances and covariance of b_s, b_t. Providing that b_s is large in comparison with its standard error, a standard error for R may be derived from

$$s_R^2 = V(R) = \frac{1}{b_s^2}\left\{V(b_t) - 2R\,\mathrm{Cov}\,(b_s, b_t) + R^2 V(b_s)\right\} \quad . \quad . \quad . \quad . \quad (31)$$

$$= 0{\cdot}0194 \times (0{\cdot}1600 - 0{\cdot}1594 + 0{\cdot}1058)/966$$

$$= 0{\cdot}00000214$$

$$= (0{\cdot}00146)^2.$$

The estimated potency is therefore 498 $\pm$ 7·3 µg. per g.

If the elements of the matrix V are written as v_s, v_t, c, the general expression for the fiducial limits of R, analogous to (27), may be derived from Fieller's (1944) formula and written

$$R + \frac{g}{1-g}\left(R - \frac{c}{v_s}\right) \pm \frac{st}{b_s(1-g)}\sqrt{v_t - 2Rc + R^2 v_s - g\left(v_t - \frac{c^2}{v_s}\right)}, \quad . \quad . \quad . \quad (32)$$

where now

$$g = s^2 v_s t^2 / b_s^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

In a good microbiological assay there should be relatively much less variation between replicate tubes than is found between animals in a macro-biological assay. Consequently g is usually negligible, so that (32) reduces to $R \pm s_R\, t$. In the example just considered, the 5 per cent. level of t is 2·26, so that $g = 0{\cdot}0011$, a negligible quantity, and the fiducial limits to the estimated potency of the extract are 482 and 514 µg. per g.

If the dose response relationship cannot be put into the form of equation (11), the assay problem is much more difficult. Little use has been made of polynomial relationships such as

$$U = \alpha + \beta x + \gamma x^2 + \ldots\ldots,$$

or of regression equations of the form

$$U = \alpha(1 - e^{-\beta x}),$$

since for them there is no simple arithmetical procedure. For the quadratic, indeed,

$$U = \alpha + \beta x + \gamma x^2,$$

where $x = \log z$, an unbiased estimate of $\log \rho$ can be obtained from a symmetrical four-point assay (two doses of each preparation, at equal intervals of x, and equal numbers of subjects in all four groups) by ignoring the quadratic term; the estimate is the same as that derived from the fitting of two parallel lines (Gridgeman, 1943; Wood, 1944*a*). But for a greater number of dose levels, the fitting of pairs of equations of the form

$$\left.\begin{aligned} U_s &= a + bx + cx^2, \\ U_t &= a + b(x + M) + c\,(x + M)^2, \end{aligned}\right\} \qquad (34)$$

or, if $x = z^\lambda$,

$$\left.\begin{aligned} U_s &= a + bx + cx^2, \\ U_t &= a + bR^\lambda\, x + cR^{2\lambda}\, x^2, \end{aligned}\right\} \qquad (35)$$

would involve a very troublesome set of computations.

Emmens (1940) has proposed the use of the logistic function for assays based on quantitative responses, when the range of responses is too great for a linear relationship between U and x to be a satisfactory representation of the data. He has found this function to agree well with observation in a number of assays. His equation is equivalent to

$$U = \tfrac{1}{2}H\left\{1 + \tanh\,(\alpha + \beta x)\right\}, \qquad (36)$$

where x is any suitable dose metameter; this represents a mean response which approaches asymptotically the maximum value H. If H is known, or can be reliably estimated from previous work, the transformation

$$Y = \tanh^{-1}\left(\frac{2U-H}{H}\right) \qquad (37)$$

can be used to reduce the relationship to the form of equation (13). The general procedure can then be used for the estimation of the parameters α and β, in a form the same as that given for "logits" in Section 5 except for a difference in the weighting coefficient. Any other sigmoid curve might be considered instead of equation (36), such as, on the analogy of probits,

$$U = H \int_{-\infty}^{\alpha + \beta x} \frac{1}{\sqrt{2\pi}}\, e^{-\frac{1}{2}t^2}\, dt, \qquad (38)$$

but for many quantitative biological data perhaps the logistic is the most reasonable to try. When H is not known in advance and has itself to be estimated from the data, the computations become more troublesome. Finney (1947) has shown how the standard form of calculations for the probit transformation can be generalized for the fitting of equation (38), and his method can be modified so as to suit equation (36) or any other of the transformations considered in Section 5. The process is scarcely likely to prove popular for routine assays if by a suitable choice of experimental conditions the necessity for it can be avoided.

In a study of dose-time-mortality relationships, Ipsen (1941) has suggested simplifying certain response curves by using an "equivalent dose" instead of time. His idea might be adapted for use with assays in which no obvious choice of dose and response metameters reduces the response curve to the form of equation (11). So far as is known to the writer, this approach has never yet been used, and a number of theoretical objections to it could be raised; nevertheless it might prove helpful in the assay of a group of materials showing an otherwise intractable form of response curve. From tests at a number of different doses, the relationship between U and the logarithm of the dose, x, might be determined empirically for the standard preparation, the curve being fitted either by eye or as a polynomial regression on x:

$$U = f(x). \qquad (39)$$

Providing that Condition I is satisfied by this function, in subsequent tests for assay purposes a response metameter may be defined by the inverse function

$$Y = f^{-1}(U),$$

which is read either directly from the empirical curve or from a tabulation of its values. If the potency of the standard preparation were exactly as in the preliminary tests, the regression equations for the two preparations would be

$$\left.\begin{aligned} Y_s &= x, \\ Y_t &= \log \rho + x; \end{aligned}\right\} \quad . \quad (40)$$

even if conditions have changed to some extent since the preliminary tests, as long as Condition II holds equations of the form of (13), (14.2) are likely to fit the data tolerably well. Hence ρ may be estimated graphically as before or, if the empirical function (39) seems sufficiently reliable to justify the trouble, $f'(Y)$ may be found from it and the maximum likelihood process continued in the standard manner. Alternatively, if the response curve seemed simpler with z^λ as a dose metameter, a similar course could be followed with an empirical form of equation (39) fitted to this metameter. If the potency of the standard remained unaltered in subsequent assays, the regression equations would then be

$$\left.\begin{aligned} Y_s &= x, \\ Y_t &= \rho x, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

and in any case equations (13), (14.1) could probably be fitted to the data satisfactorily provided that Condition II held. This method, however, is likely to be of limited applicability and will not be considered further here.

5. QUANTAL RESPONSES

Many important assays are dependent upon a quantal or "all-or-nothing" response. The typical response of this kind is death of the test subject, but there are others, such as spore germination in fungicidal assays, or cure of deficiency symptoms in vitamin assays, for which no graduation of the response is possible and all that can be done is to describe a test subject as responding or not responding to a dose z. The role of u in Section 2 is now played by p, the proportion of test subjects responding in a sample receiving a dose z. The expectation of p will be a function

$$P = F(z) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

which will usually satisfy Condition I, and for which, over the range of doses to be used,

$$0 < F(z) < 1.$$

For convenience of nomenclature, the response will be considered to be death of the subject.

Unless there is a natural mortality amongst undosed subjects (Finney, 1944), $F(0)$ must be zero, and unless some of the subjects are immune to the material under test $F(z)$ must become unity at a sufficiently high value of z. These possible complications will be considered later. If the reasonable assumption that any subject killed by a dose z would, under the same circumstances, have been killed by any dose greater than z (and consequently that any subject surviving a dose z would have survived any dose less than z) be accepted, then with each individual in the population from which the test subject was drawn can be associated a threshold value of z, the *tolerance* or maximum dose which would just fail to kill the individual under the circumstances of the test. This concept is entirely different from that of the "minimal lethal dose" at one time popular with toxicologists. The idea of measuring potency in terms of the least dose potentially fatal to all test subjects has been abandoned, as a result of the pioneer thinking of Trevan (1927) and Gaddum (1933), who showed it to be inconsistent with the known occurrence of wide variation in susceptibility from subject to subject; instead a frequency distribution of individual tolerances must be envisaged.

If n subjects are each given a dose z, to which they react independently, the probability that r will die and $(n-r)$ survive is

$${}_nC_r\, P^r Q^{n-r}$$

where $Q = 1 - P$. Hence the probability of obtaining $r_1, r_2, r_3 \ldots$ deaths in batches of $n_1, n_2, n_3 \ldots$ receiving doses $z_1, z_2, z_3 \ldots$ is proportional to e^L, where

$$L = Sr \log P + S(n-r) \log Q. \quad . \quad . \quad . \quad . \quad (43)$$

Application of the principle of maximum likelihood shows that if θ is a typical unknown parameter of the function $F(\ )$, estimates of θ and other parameters will be obtained from the simultaneous solution of equations of the form

$$\frac{\partial L}{\partial \theta} = S\frac{n(p-P)}{PQ}\frac{\partial P}{\partial \theta} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

where $p = r/n$ is the estimate of P for a dose z. Now equation (44) is very similar to the maximum likelihood equation derived from equation (15), except that the binomial variance, PQ/n, has replaced σ^2. The argument of Section 3 may be repeated to show that, when P can be expressed in the form

$$P = f(\alpha + \beta x), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

the estimation of α and β may be carried out with the aid of weighted linear regression computations: if a_1, b_1 are first approximations to the maximum likelihood estimates, improved estimates are obtained from the weighted regression of a working response on x. As in equation (20), working responses are derived from the first approximations and are

$$\left.\begin{aligned} y &= Y + (p-P)/f'(Y), \\ \text{or } y &= Y - (q-Q)/f'(Y); \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46)$$

the weight to be attached to a working response based on n subjects is nw where

$$w = \{f'(Y)\}^2/PQ \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (47)$$

Again the first approximation may be obtained by plotting the metameters of the empirical values of p against x and fitting a straight line to these by eye. If the same response transformation has to be used often, a table of Y as a function of P, and tables of the weighting coefficient, the minimum working response, and the range as functions of Y will be helpful. The minimum working response is now a true minimum, being the value of y to be taken when the observed kill is zero ($p = 0$); the range is the difference between this and the *maximum working response* for the same Y, the working response when all the subjects are killed ($q = 0$). Quantal responses may be used for assay purposes just as easily as quantitative responses. The necessary extension of the above procedure is exactly that described in the last paragraph of Section 3; an example is given below.

The frequency distribution of individual tolerances in terms of the dose, z, is easily derived from equation (42) as

$$dP = F'(z)dz, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

or, in terms of the dose metameter for data in agreement with equation (45),

$$dP = \beta f'(\alpha + \beta x)dx. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (49)$$

The most useful distribution of individual tolerances to study is that which gives a normal distribution of the corresponding dose metameters. If equation (49) takes the form

$$dP = \frac{1}{\sigma\sqrt{2\pi}}\, e^{-\frac{(x-\xi)^2}{2\sigma^2}}\, dx, \quad . \quad . \quad . \quad . \quad . \quad (50)$$

the appropriate response metameter is defined by

$$P = \int^{Y} \frac{1}{\sqrt{2\pi}}\, e^{-\frac{1}{2}t^2}\, dt \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

and equation (13) becomes

$$Y = (x-\xi)/\sigma. \qquad (52)$$

Gaddum (1933) termed Y in this transformation the *normal equivalent deviate* of P; Bliss (1934*a*, *b*) introduced the name *probit* for the same quantity increased by 5 in order to avoid negative values. The probit transformation has proved satisfactory for simplifying the expression of the dose-response relationship for many types of quantal response (Bliss and Cattell, 1943); Bliss (1935*a*, *b*), Irwin (1937), and others have described the use of probits in biological assay. No example of the probit method will be given, as its use has been so frequently illustrated elsewhere, and the procedure is exactly parallel to that shortly to be described for "logits."

There is no limit to the number of alternative functions that could theoretically be taken for $f(\ )$, though few of them are likely to be of much practical importance. Urban (1909, 1910), who followed Fechner (1860) in using the essentials of the probit transformation for psychometric investigations many years before the development of its biological applications, suggested what in the present notation may be written as

$$P = \tfrac{1}{2} + \frac{1}{\pi}\tan^{-1} Y \qquad (-\infty < Y < \infty).$$

Wilson and Worcester (1943*a*, *b*, *c*) established equations relating to the use of quantal responses in biological assay, though not in quite so general a form as the equations given here, and discussed several alternative functions. In addition to the probit transformation they considered

$$P = \tfrac{1}{2}(1 + \tanh Y) \qquad (-\infty < Y < \infty)$$

$$P = \tfrac{1}{2}(1 + Y) \qquad (-1 \leqslant Y \leqslant 1)$$

$$P = \tfrac{1}{2}(1 + \sin Y) \qquad \left(-\frac{\pi}{2} \leqslant Y \leqslant \frac{\pi}{2}\right)$$

$$P = \tfrac{1}{2}\left(1 + \frac{Y}{\sqrt{1+Y^2}}\right) \qquad (-\infty < Y < \infty)$$

The first of these uses a logistic curve instead of the sigmoid obtained as the cumulative normal curve. The second is merely a linear function of P, which might have been chosen, less symmetrically but more simply, as

$$P = Y \qquad (0 \leqslant Y \leqslant 1),$$

and the third is effectively the same as

$$P = \sin^2 Y, \qquad (0 \leqslant Y \leqslant 1),$$

a transformation introduced by Bliss for use in the analysis of variance of percentage data.

From equations (46) and (47), the various formulae required for use with these different response metameters can be obtained, and are as follows:—

	P	Minimum working response	Range	Maximum working response	Weighting coefficient w
1.	$\int_{-\infty}^{Y} \frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}t^2}\,dt$	$Y - \frac{P}{Z}$	$\frac{1}{Z}$	$Y + \frac{Q}{Z}$	$\frac{Z^2}{PQ}$
2.	$\frac{1}{2} + \frac{1}{\pi}\tan^{-1} Y$	$Y - \pi P(1+Y^2)$	$\pi(1+Y^2)$	$Y + \pi Q(1+Y^2)$	$\frac{1}{\pi^2 PQ(1+Y^2)^2}$
3.	$\frac{1}{2}(1+\tanh Y)$	$Y - \frac{1}{2Q}$	$\frac{1}{2PQ}$	$Y + \frac{1}{2P}$	$4PQ$
4.	Y	0	1	1	$\frac{1}{PQ}$
5.	$\sin^2 Y$	$Y - \frac{1}{2}\tan Y$	$\operatorname{cosec} 2Y$	$Y + \frac{1}{2}\cot Y$	4
6.	$\frac{1}{2}\left(1 + \frac{Y}{\sqrt{1+Y^2}}\right)$	$-Y^3-(1+Y^2)^{\frac{3}{2}}$	$2(1+Y^2)^{\frac{3}{2}}$	$-Y^3+(1+Y^2)^{\frac{3}{2}}$	$16P^2Q^2$

In addition to their tables for the first of these, the probit transformation, Fisher and Yates (1947) have given tables for transformation No. 5, commonly known as the angular transformation, which can be used in the same manner, and have indicated how other tables in their collection can be used for transformation No. 3. "Transformation" No. 4 presents no difficulties; the results above show that if P itself is linearly related to the dose metameter, the empirical values p are to be used as working responses and weighted inversely as the binomial variance estimated from the provisional line (of course such a relationship could scarcely occur except as an approximation, excluding extreme values of P). For the six forms of the function $f(\ )$ at present under examination, equation (49) gives the frequency distributions of individual tolerances as

$$dP_1 = \frac{\beta}{\sqrt{2\pi}} e^{-\frac{1}{2}(\alpha+\beta x)^2} dx,$$

$$dP_2 = \frac{\beta}{\pi}\left\{ 1 + (\alpha + \beta x)^2 \right\}^{-1} dx,$$

$$dP_3 = \tfrac{1}{2}\beta \operatorname{sech}^2 (\alpha + \beta x)\, dx,$$

$$dP_4 = \beta\, dx,$$

$$dP_5 = \beta \sin 2(\alpha + \beta x)\, dx,$$

$$dP_6 = \tfrac{1}{2}\beta\left\{ 1 + (\alpha + \beta x)^2 \right\}^{-\frac{3}{2}} dx.$$

Berkson (1944) advocated using the logistic curve instead of the normal sigmoid in the analysis of data from quantal assays; he argued, "However, the logistic function is very close to the integrated normal curve, it applies to a wide range of physico-chemical phenomena and therefore may have a better theoretic basis than the integrated normal curve." This statement seems to be based on a misconception, since the reason for introducing the transformation specified by equation (51) is not directly the shape of the normal sigmoid, but the derivation of the transformation from a normal distribution of tolerance as measured on the x-scale. Though absolute normality of distribution may seldom be attained, there seems little cause in general to prefer a distribution curve whose ordinate is proportional to $\operatorname{sech}^2 (\alpha + \beta x)$.

Extensive tables now available (Bliss, 1935*a*, 1939; Fisher and Yates, 1947; Finney, 1947) make use of the probit transformation a simple computational routine. De Beer (1945) has made ingenious suggestions for improving the graphical method with the aid of specially graduated rules; his scheme does not take full account of the finer points of the maximum likelihood solution, since it only employs working probits for zero and 100 per cent. kills, but it should be good enough for most practical purposes and might prove much quicker than the full computations.

Wilson and Worcester (1943*a*) have given tables to simplify the computations for the logistic transformation, providing that there are only three doses, with equal numbers of subjects at each. Berkson (1944) has developed a computational procedure for the logistic transformation, which fails to take account of the asymmetry of binomial frequency distributions by the introduction of "working responses" or otherwise, though he draws attention to this possibility, and thus does not lead to the maximum likelihood solution. He says, "I believe that the work of fitting the logistic as given here is considerably simpler than that of fitting the normal curve by probits and maximum likelihood as advocated by Bliss and Fisher." This greater simplicity is entirely a result of the omission of the calculation of working responses; with the aid of Tables 3 and 4 below, these may be introduced into the logistic calculations and the two transformations are then exactly equal in respect of the amount of computing they require.

In practice, the alternative transformations given above, as well as others such as the *rankit* (Ipsen and Jerne, 1944), would often give very similar results, and many series of data would fail to distinguish one of them as peculiarly appropriate; even the rectangular tolerance distribution, corresponding with

$$P = \alpha + \beta x,$$

though objectionable on theoretical grounds, may not be too bad a fit unless the data either involve extreme values of P or are very precise. Unless the data clearly indicate the need for something different, the probit seems the obvious choice for use in assays. Nevertheless, that based on the

logistic curve is not without interest, for it implies a linear regression of log (P/Q) on log z and may therefore be of value in connection with the Langmuir adsorption law (Bliss, 1935*a*). Adapting Berkson's terminology, but, as with probits, taking steps to avoid the occurrence of negative values, the *logit* of P may be defined to be Y in the equation

$$P = \left\{1 + e^{-2(Y-5)}\right\}^{-1},$$
$$\text{or } Y = 5 + \tanh^{-1}(2P-1) \qquad . \quad . \quad . \quad . \quad . \quad (53)$$

In Table 3, Y is shown as a function of P. The expressions for minimum and maximum working probit, range, and weighting coefficient have been given above (transformation No. 3), and numerical values at intervals of 0·1 in Y are given in Table 4.

As both the probit and the logit transformations require that $P = 0$ should correspond to an infinite negative value of x, the only simple dose metameter of interest is the logarithm of the dose. The arithmetical procedure for assaying a test material against a standard is then very similar to that described in the early part of Section 4. The fitted regression lines are constrained to be parallel in each cycle of the computations (one cycle is often sufficient) and the lines are found in the form of equations (22). The estimate of relative potency is then obtained from equation (23) and its variance from equation (24), with the difference that N_s, N_t, instead of being the total numbers of subjects for the two preparations, are now the totals of the weights, Snw, where n is the number in a batch for which the weighting coefficient is w; fiducial limits are obtained from the expression (27). The weights, being derived from binomial frequency distributions, are equal to (instead of merely proportional to) the reciprocals of the variances of the response metameters, and therefore the residual sum of squares may be referred to the χ^2 distribution as a test of agreement between the results and hypothesis. Providing that there is no serious evidence of discrepancy, s^2, the variance per unit weight, may be taken as unity and fiducial limits calculated by use of normal instead of t-deviates. A significant value of χ^2 may indicate that the dose-response regression function is of a different form from that used, thus leading to non-linearity in the (x, y) plane, or it may indicate that through unavoidable, or perhaps avoidable, flaws in experimental technique the several batches of test subjects are heterogeneous. If the second explanation seems appropriate, an adjustment may be made by writing

$$s^2 = \chi^2/(\text{degrees of freedom})$$

instead of unity, and reverting to the use of t-deviates.

TABLE 3

Transformation of Percentages to Logits

Percentage	0	1	2	3	4	5	6	7	8	9
0	—	2·702	3·054	3·262	3·411	3·528	3·624	3·707	3·779	3·843
10	3·901	3·955	4·004	4·050	4·092	4·133	4·171	4·207	4·242	4·275
20	4·307	4·338	4·367	4·396	4·424	4·451	4·477	4·503	4·528	4·552
30	4·576	4·600	4·623	4·646	4·668	4·690	4·712	4·734	4·755	4·776
40	4·797	4·818	4·839	4·859	4·879	4·900	4·920	4·940	4·960	4·980
50	5·000	5·020	5·040	5·060	5·080	5·100	5·121	5·141	5·161	5·182
60	5·203	5·224	5·245	5·266	5·288	5·310	5·332	5·354	5·377	5·400
70	5·424	5·448	5·472	5·497	5·523	5·549	5·576	5·604	5·633	5·662
80	5·693	5·725	5·758	5·793	5·829	5·867	5·908	5·950	5·996	6·045
90	6·099	6·157	6·221	6·293	6·376	6·472	6·589	6·738	6·946	7·298
	0·0	0·1	0·2	0·3	0·4	0·5	0·6	0·7	0·8	0·9
97	6·738	6·756	6·774	6·792	6·812	6·832	6·853	6·874	6·897	6·921
98	6·946	6·972	7·000	7·029	7·060	7·092	7·127	7·165	7·205	7·249
99	7·298	7·351	7·410	7·477	7·555	7·647	7·759	7·903	8·106	8·453

Maximum and Minimum Working Logits, Range, and Weighting Coefficient

Minimum working logit.		Range	Maximum working logit		Weighting coefficient
Expected logit Y	$y_0 = Y - \frac{1}{2Q}$	$\frac{1}{2PQ}$	$y_{100} = Y + \frac{1}{2P}$	Expected logit Y	w
1·1	0·5998	1221·3	9·4002	8·9	·00164
1·2	0·6997	1000·1	9·3003	8·8	·00200
1·3	0·7997	819·0	9·2003	8·7	·00244
1·4	0·8996	670·7	9·1004	8·6	·00298
1·5	0·9995	549·3	9·0005	8·5	·00364
1·6	1·0994	449·92	8·9006	8·4	·00445
1·7	1·1993	368·55	8·8007	8·3	·00543
1·8	1·2992	301·92	8·7008	8·2	·00662
1·9	1·3990	247·38	8·6010	8·1	·00808
2·0	1·4988	202·72	8·5012	8·0	·00987
2·1	1·5985	166·15	8·4015	7·9	·01204
2·2	1·6982	136·22	8·3018	7·8	·01468
2·3	1·7977	111·71	8·2023	7·7	·01790
2·4	1·8972	91·64	8·1028	7·6	·02182
2·5	1·9966	75·21	8·0034	7·5	·02659
2·6	2·0959	61·759	7·9041	7·4	·03238
2·7	2·1950	50·747	7·8050	7·3	·03941
2·8	2·2939	41·732	7·7061	7·2	·04793
2·9	2·3925	34·351	7·6075	7·1	05822
3·0	2·4908	28·308	7·5092	7·0	·07065
3·1	2·5888	23·362	7·4112	6·9	·08561
3·2	2·6863	19·313	7·3137	6·8	·10356
3·3	2·7833	15·999	7·2167	6·7	·12501
3·4	2·8796	13·287	7·1204	6·6	·15053
3·5	2·9751	11·068	7·0249	6·5	·18071
3·6	3·0696	9·2527	6·9304	6·4	·21615
3·7	3·1629	7·7690	6·8371	6·3	·25743
3·8	3·2546	6·5569	6·7454	6·2	·30502
3·9	3·3446	5·5679	6·6554	6·1	·35920
4·0	3·4323	4·7622	6·5677	6·0	·41997
4·1	3·5174	4·1075	6·4826	5·9	·48692
4·2	3·5991	3·5775	6·4009	5·8	·55906
4·3	3·6767	3·1509	6·3233	5·7	·63474
4·4	3·7494	2·8107	6·2506	5·6	·71158
4·5	3·8161	2·5431	6·1839	5·5	·78645
4·6	3·8753	2·3374	6·1247	5·4	·85564
4·7	3·9256	2·1855	6·0744	5·3	·91514
4·8	3·9648	2·0811	6·0352	5·2	·96104
4·9	3·9906	2·0201	6·0094	5·1	·99007
5·0	4·0000	2·0000	6·0000	5·0	1·00000
5·1	3·9893	2·0201	6·0107	4·9	·99007
5·2	3·9541	2·0811	6·0459	4·8	·96104
5·3	3·8889	2·1855	6·1111	4·7	·91514
5·4	3·7873	2·3374	6·2127	4·6	·85564
5·5	3·6408	2·5431	6·3592	4·5	·78645
5·6	3·4399	2·8107	6·5601	4·4	·71158
5·7	3·1724	3·1509	6·8276	4·3	·63474
5·8	2·8234	3·5775	7·1766	4·2	·55906
5·9	2·3751	4·1075	7·6249	4·1	·48692
6·0	1·8055	4·7622	8·1945	4·0	·41997
6·1	1·0875	5·5679	8·9125	3·9	·35920
6·2	0·1885	6·5569	9·8115	3·8	·30502
6·3	−0·9319	7·7690	10·9319	3·7	·25743
6·4	−2·3223	9·2527	12·3223	3·6	·21615
6·5	−4·0428	11·0677	14·0428	3·5	·18071

The working logit, y, may be obtained as

$$y = Y - \frac{1}{2Q} + \frac{p}{2PQ}$$

$$y = Y + \frac{1}{2P} - \frac{q}{2PQ}, \text{ whichever is the more convenient,}$$

where p ($= 1 - q$) is the observed proportion killed.

Example: Miller *et al.* (1939) have given the results of a comparison of the toxicity of digitalis to frogs when injected in two different ways—intramuscularly and into the ventral lymph sac. The computations that follow show the logit method for the estimation of the potency of intramuscular injection relative to lymph-sac injection. The dose levels are obviously too few to provide any adequate test of whether the logit transformation is as good as or better than the probit; indeed, as will be seen from Table 7, almost identical results are obtainable without any transformation of P. The example in itself does not justify the introduction of the complications and refinements of the probit or the logit technique. The full calculations are given, however, as an illustration of the systematic arrangement of the analysis. More extensive data can be analysed in exactly similar form, using the tables appropriate to the probit or logit transformation or to any of the others summarized on p. 60.

In the first three columns of Table 5 are given the logarithm of dose (in c.c. of digitalis preparation per kg. of body-weight), the number of frogs tested at that dose, and the number killed, from which the proportionate mortalities in column 4 are calculated. Empirical logits corresponding to these proportions are read from Table 3 and graphed against x in Fig. 2. Two parallel

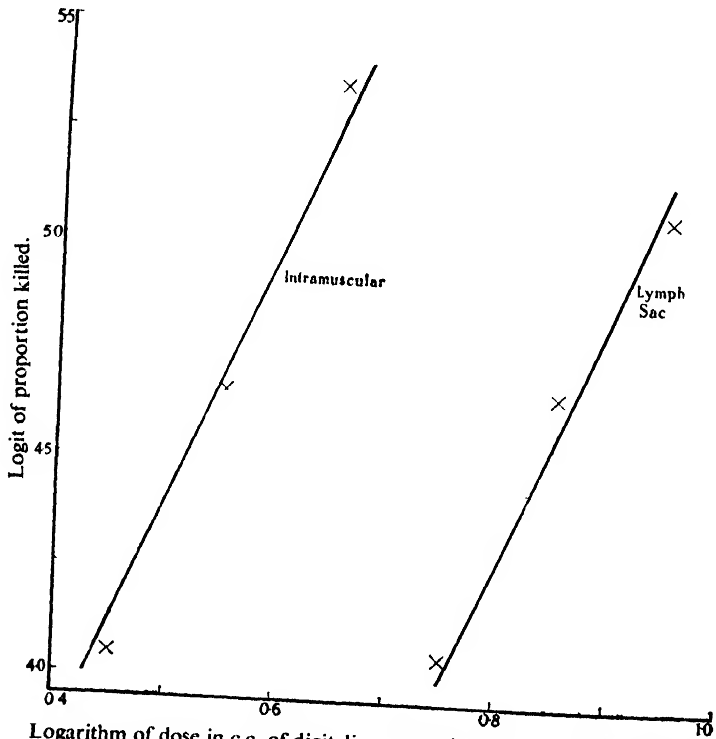


FIG. 2.

Logit response diagram for comparison of two methods of injection of digitalis into frogs.

TABLE 5

Preliminary Logit Computations for Comparison of Potency of Two Methods of Injection of Digitalis in Frogs

(Data of Miller *et al*)

Log dose x	Number of frogs n	Number killed r	Proportion killed p	Empirical logit	Y	nw	y	nwx	nwy
Lymph sac:									
·75	15	2	·13	4·05	4·0	6·3	4·05	4·725	25·515
·85	15	5	·33	4·65	4·6	12·8	4·65	10·880	59·520
·95	15	8	·53	5·06	5·1	14·9	5·06	14·155	75·394
						34·0		29·760	160·429
Intramuscular:									
·45	15	2	·13	4·05	4·1	7·3	4·05	3·285	29·565
·55	15	5	·33	4·65	4·7	13·7	4·65	7·535	63·705
·65	15	10	·67	5·35	5·3	13·7	5·35	8·905	73·295
						34·7		19·725	166·565

$\bar{x}_1 = 0{\cdot}8753 \qquad \bar{y}_1 = 4{\cdot}7185$

$\bar{x}_2 = 0{\cdot}5684 \qquad \bar{y}_2 = 4{\cdot}8001$

	$Snwx^2$	$Snwxy$	$Snwy^2$
Lymph sac:			
	26·23900	141·3526	761·597
	26·04875	140·4226	756·984
	0·19025	0·9300	4·613
Intramuscular:			
	11·41075	95·9838	808·095
	11·21255	94·6828	799·536
	0·19820	1·3010	8·559

TABLE 6

Computations for Relative Potency on Data of Table 5

	S_{xx}	S_{xy}	S_{yy}	$S_{yy}-S_{xy}^2/S_{yy}$
Lymph sac	0·19025	0·9300	4·613	0·067
Intramuscular.	0·19820	1·3010	8·559	0·019
Total	0·38845	2·2310	13·172	0·359

Heterogeneity: $\chi^2_{[2]} = 0{\cdot}067 + 0{\cdot}019 = 0{\cdot}086$

Parallelism: $\chi^2_{[1]} = 0{\cdot}359 - 0{\cdot}086 = 0{\cdot}273$

$b = 2{\cdot}2310/0{\cdot}38845 = 5{\cdot}7433$

Regression equations	log $LD50$
$Y_1 = -0{\cdot}309 + 5{\cdot}743x$	0·924
$Y_2 = 1{\cdot}536 + 5{\cdot}743x$	0·603

$\log R = 0{\cdot}321 \pm 0{\cdot}042$

$$V(\log R) = \frac{1}{b^2}\left[0{\cdot}02941 + 0{\cdot}02882 + \frac{(0{\cdot}321 - 0{\cdot}307)^2}{0{\cdot}38845}\right]$$

$$= (0{\cdot}0422)^2$$

$$g = 3{\cdot}84/0{\cdot}38845b^2 = 0{\cdot}300$$

$$\text{Fiducial limits} = 0{\cdot}321 + \frac{0{\cdot}3 \times 0{\cdot}014}{0{\cdot}7} \pm \frac{1{\cdot}96}{0{\cdot}7b}\sqrt{0{\cdot}7 \times 0{\cdot}05823 + 0{\cdot}00050}$$

$$= 0{\cdot}327 \pm 0{\cdot}099$$

$$= 0{\cdot}426,\ 0{\cdot}228$$

lines are then fitted by eye to the points of this diagram and the expected Y corresponding to each x is tabulated. The values of Y are used in conjunction with Table 4 to give the weights, nw, and the working logits, y. Since n is 15 for every dose, the multiplication by 15 could have been deferred until later, but the more general pattern of computations is shown here; the provisional lines in this example fit the data so closely that the working logits are the same as the empirical. Sums of squares and products are obtained as in the remainder of Table 5; the numbers of decimal places shown at all stages are sufficient for logit and probit computations unless the numbers of test subjects are much greater than here.

Table 6 shows the remaining stages in the computation of the estimate of log R. The two sets of sums of squares and products are added and χ^2 tests of heterogeneity and of parallelism are made. The heterogeneity test is identical with a χ^2 test on the observed and expected numbers of dead and survivors for each dose. Consequently, when one or more small expectations occur the χ^2 may be unduly large; this point has been discussed more fully elsewhere (Finney, 1947), but causes no alarm here since the calculated χ^2 is so small. The average value of the regression coefficient is next found, and is used to give the two regression lines and estimates of log median lethal doses. Values of Y calculated from these lines are the same as those already used in Table 5, so that no further cycle of computations is needed. The estimate of log R is the difference between the two log median lethal doses, and its variance is found from equation (24), putting $N_s = 34{\cdot}0$, $N_t = 34{\cdot}7$; the fiducial limits are similarly found from (27). The conclusion to be drawn is that intramuscular injection has shown no significant difference from lymph-sac injection in the form of the response curve, except that the potency has been increased to 209 per cent. of the lymph sac value; the 5 per cent. fiducial limits of this ratio are 267 per cent. and 169 per cent. Table 7 shows comparative results for four different response metameters; even the fourth of these, implying a linear relationship of P and log dose, here gives results practically the same as the others since the data are so very regular.

TABLE 7

Comparison of Four Response Metameters for the Analysis of the Assay in Table 5

Response metameter	Log *LD*50 Lymph sac	Log *LD*50 Intramuscular	Log *R*	*R*	5 per cent. fiducial limits for *R*
Probit	0·924	0·603	0·321	2·09	1·70 – 2·65
Logit	0·924	0·603	0·321	2·09	1·69 – 2·67
$P = \sin^2 Y$	0·922	0·603	0·319	2·08	1·70 – 2·62
$P = Y$	0·919	0·605	0·314	2·06	1·72 – 2·53

In many forms of quantal assay, allowance has to be made for a "natural mortality" amongst the test subjects, or the equivalent phenomenon for responses other than death. Thus in insecticide trials a proportion of insects would probably die in the period between testing and classifying of subjects even though they received no poison, and in assays of fungicides by spore germination counts a proportion of untreated spores would fail to germinate. Providing that this natural mortality occurs randomly and is not correlated with susceptibility to the poison, the proportion of subjects whose death is due to the treatment, P, will be related to P', the total proportion killed at any dose, by the equation

$$P' = C + P(1 - C), \qquad (54)$$

where C is the natural mortality rate. If not all subjects are susceptible to the materials under test, a proportion K being immune whatever the dose given, then

$$P' = C + P(1 - K - C). \qquad (55)$$

In reality, natural mortality may be greatest amongst the subjects of lowest tolerance, so that P derived in this manner will not truly represent the effect of the poison alone acting on the whole population, but equation (54) or the more general equation (55) may be taken as a first approximation to the truth.

If C and K were known for the population from which the subjects were selected, values of p could be obtained from the observed mortalities p' by means of equation (55). The only alteration then required in the assay procedure already described is that equation (47) must be replaced by

$$w = \frac{[f'(Y)]^2}{\left(P + \frac{C}{1 - K - C}\right)\left(Q + \frac{K}{1 - K - C}\right)} \quad . \quad . \quad . \quad . \quad . \quad (56)$$

which change, even for small values of C and K, will reduce w considerably when P or Q is near zero. For the probit transformation, Finney (1944, 1947) has tabulated this function with $K = 0$, the most important case. In practice C and K have usually to be estimated from experimental data, a situation which greatly complicates the maximum likelihood equations. Finney has shown a practicable computational procedure of successive approximations for $K \doteq 0$, and this can easily be extended for use when $K \neq 0$; apart from the additional labour, there are no essentially new features in the analysis.

6. Bacterial Assays from Dilution Series

A slightly different type of quantal assay that may formally be analysed by the general method of this paper is the estimation of a bacterial population from a dilution series. A series of dilutions (usually in geometrical progression) of a bacterial suspension is prepared and several plates are inoculated from each dilution; each plate is later classified as positive or negative according to whether or not growth occurs, but no colony count is made. From this dichotomous classification the density of bacteria in the original suspension is to be estimated. On the assumptions that the original suspension was sufficiently well mixed to give a Poisson distribution of the numbers of bacteria in equal units of volume, and that presence of a single bacterium on a plate will cause growth, the probability of a negative plate at a dilution of x parts of suspension per unit volume is

$$P = e^{-\beta x}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

where β is the density of bacteria in the original suspension reckoned as the number in a volume equal to that used for the unit inoculum.

Fisher (1922) has shown that, when equal numbers of plates are tested at each of a series of dilutions with values of x in geometrical progression (a convenient and popular practical technique), an estimate of β with 88 per cent. efficiency can be made by equating the total number of sterile plates to its expectation, provided only that the range of dilutions tested is sufficiently extensive to give a very low probability of a sterile plate at one end and a very low probability of a fertile plate at the other. Fisher and Yates (1947, Table VIII 2) have given a table to assist rapid estimation by this method. So high an efficiency is satisfactory for most purposes, and solution of the maximum likelihood equations given by Fisher is seldom necessary unless either the series of dilutions is short, or the values of x are not in geometrical progression.

Halvorson and Ziegler (1933) and Swaroop (1938) have given tables of solutions of the maximum likelihood equation for β when equal numbers of plates have been inoculated at each of three dilutions in a ten-fold series. In general, the solution can be performed by the method of Section 5: by writing

$$P = e^{-Y} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

equation (57) is transformed into .

$$Y = \beta x. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (59)$$

For this transformation

$$f'(Y) = -P$$

and therefore, from equations (46) and (47), if Y is determined from a first approximation to β, a new approximation may be obtained as the weighted linear regression coefficient of the working response

$$y = Y + 1 - pe^{Y} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (60)$$

on x; p is the proportion of sterile plates observed at dilution x, and

$$w = \frac{1}{e^{Y} - 1} \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (61)$$

is the weighting coefficient per plate tested at this dilution. If only one plate is set up for each dilution, p is necessarily always 0 or 1; the procedure can still be followed, though the first approximation may not be very satisfactory. The fitted regression line,

$$Y = bx, \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (62)$$

must be constrained to pass through the origin.

The range of dilutions will usually be sufficient to give expected probabilities of sterile plates at one end very close to zero and at the other very nearly unity; consequently Y will run from very large values to very small, w from very small to very large. Retention of the right numbers of digits in the computations is often made easier by writing

$$Y' = Y/x, \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (63)$$

$$y' = Y' + \frac{1}{x} - \frac{pe^{xY'}}{x}, \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (64)$$

and

$$w' = \frac{x^2}{e^{xY'} - 1}; \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (65)$$

b is then the weighted mean value of y', using weights nw'. This modification is particularly easy when the dilutions differ by powers of 10.

Example: Halvorson and Ziegler (1933) discuss the example shown in Table 8, relating to a test with 160 plates at each of six dilutions in a ten-fold series. For convenience, the scale of x is chosen to give unit dilution somewhere in the middle of the range. The numbers of sterile plates are shown in the column headed r, and the proportion sterile is shown as p. A first approximation to the estimate, b, of the mean number of organisms per unit of inoculum at the dilution $x = 1$ is the average of the empirical values of Y', $-\dfrac{1}{x}\log_e p$. Here this function is readily seen to have the greatest weight at $x = 10$ and at $x = 1$, much less at $x = 10^{-1}$, and a negligible weight elsewhere; hence a first approximation, Y', is taken as a little less than the empirical value for $x = 1$.

TABLE 8

Estimation of Populations of Bacteria by Dilution Method

(Data of Halvorson and Ziegle)

x	n	r	p	$\frac{1}{x}\log p$	Y'	y'	nw'	$nw'y'$
10^3	160	0	0	∞	·47	·471	0	0
10^2	160	0	0	∞	·47	·480	0	0
10	160	2	·012	·442	·47	·438	147	64·386
1	160	99	·619	·480	·47	·480	267	128·160
10^{-1}	160	152	·950	·513	·47	·513	33	16·929
10^{-2}	160	160	1·000	0	·47	—·001	3	—0·003
							450	209·472

$Snw'y'$

98·402
97·508

$0{\cdot}894 = \chi^2_{[3]}$

$$b = 209{\cdot}472/450$$
$$= 0{\cdot}4655 \pm 0{\cdot}0471.$$

With the aid of a table of exponentials, y' and nw' were calculated from equations (64) and (65) for $Y' = 0{\cdot}47$; three, or at most four, significant digits in y' and three in nw' are quite sufficient since the values of p have been determined from less than 200 plates. The weighted sum of squares of deviations of y' may be taken as a χ^2, here with 3 degrees of freedom since only four levels have really been used in the analysis. This test of the agreement between the observations and the estimated population density is the same as could be calculated from observed and expected numbers of sterile and fertile plates. The estimated number of bacteria per unit of inoculum at the dilution $x = 1$ is

$$b = Snw'y'/Snw',$$

and its variance is $1/Snw'$; the numerical result from Table 8 is

$$b = 0{\cdot}4655 \pm 0{\cdot}0471,$$

and the close agreement with the first approximation shows there to be no necessity for a further cycle of computations. Halvorson and Ziegler, using only the three most informative dilutions, obtained an estimate 0·4669; their estimation equation is the same as that used here, but they do not describe any successive approximation process for its solution. Estimation from the total number of sterile plates, by means of Fisher and Yates' table, gives $0{\cdot}472 \pm 0{\cdot}047$, in close agreement with the maximum likelihood figure. For so large a number of plates, the standard error can probably be used safely for assigning fiducial limits to the population density, but had there been only a few plates at each dilution an uncritical calculation of limits might be misleading.

In most practical applications of the dilution series technique, the small loss of efficiency involved in Fisher's method of estimation is outbalanced by the great simplicity of the calculations. If the number of plates is not the same for every dilution, or if the series of dilutions is other than the two-, three-, and ten-fold series for which Fisher and Yates have given tables, the method outlined here provides a moderately speedy way of obtaining the maximum likelihood estimate. The functions needed in the solution can easily be derived from tables of exponentials and natural logarithms: the excellent tables from the New York Works Project Administration are very suitable. Swaroop (1938) has tabulated $(e^{0{\cdot}1n} - 1)^{-1}$, $(e^{0{\cdot}01n} - 1)^{-1}$, and $(e^{0{\cdot}001n} - 1)^{-1}$ for integral values of n from 1 to 1,000, and his table may be used for many of the values of w' required in ten-fold series. Barkworth and Irwin (1938) have also shown how the maximum likelihood estimate and its variance can be obtained by successive approximation, but their computations are less conveniently arranged than those in Table 8.

7. THE DESIGN OF ASSAYS

The chief purpose of every biological assay of the general type discussed in Sections 2 and 3 is the estimation of a relative potency with sufficient precision for appropriate action to be based upon it. Usually the assay is also required to provide both an estimate of this precision and a test of the validity of the statistical principles involved. In a laboratory in which a particular form of assay is being used as a routine analytical method, a system of control charts may be preferable to the treatment of each assay as self-contained in these respects (Knudsen, 1945; Knudsen and Randall, 1945). In other circumstances, the normal experimental procedure of planning each assay as a unit yielding its own estimate of error and its own validity tests may be adopted.

For some types of assay, the demands that for a given amount of experimentation and measurement both the most reliable estimate of ρ and a valid estimate of its variance shall be obtained are compatible. On the other hand, few assays can provide tests of validity unless their design is so modified as to be of less than optimal efficiency for the estimation of relative potency. Consequently, in a well-planned assay, the dose levels and numbers of test subjects at each dose must be chosen in relation to pre-existing knowledge of the potency of the material being assayed, of the validity of Conditions I and II, and of the appropriate dose and response transformations; if previous experience makes the experimenter certain of the validity of his statistical technique, he need not incorporate a test of validity in his design and can concentrate on reducing the variance of R, but otherwise, though he must not lose sight of his main objective, he must sacrifice some precision in R in order to make a sufficiently detailed test of validity. In planning for the precision of R, previous information on the magnitude of ρ is often valuable.

The methods of analysis of assay data outlined in this paper have all involved the expression of the relationship between response and dose by means of a straight line, which is estimated from the measurements either directly or after their transformation to suitable metameters. Departure from linearity does not of itself mean that an assay is invalid: it may be only an indication that the wrong metameters have been used. The chief requirement for validity is that Condition II shall be satisfied, or in other words that a single "relative potency" shall suffice for the evaluation of the test preparation in terms of the standard. Nevertheless non-linearity is at least a warning that the wrong analytical procedure has been adopted; unless there are very sound reasons for believing that the right metameters for rectifying the dose-response relationship are known in advance, and that no serious deviations will occur, an assay should provide data for a test of linearity as well as of Condition II.

In the choice of dose levels, the chief problem confronting the experimenter is how to make the best use of a specified total, N, of test subjects. Though a large number of levels may be inconvenient, the decision between, say, two doses of each preparation and five can often be based primarily on statistical considerations of which is likely to give the more valuable results. For simplicity of computation, the doses are usually taken as equally spaced on the scale of that dose-metameter which previous experience suggests as appropriate to the assay. Discussion of the principles governing the selection of dose levels in the several types of assay described earlier will make these points clearer.

One method of improving the precision of an assay, which will not be discussed in any detail here, is by a covariance analysis of the results with one or more concomitant measurements either made on the test subjects before the doses are given or known to be unaffected by the type of stimulus used. Adjustments for the initial weight of the test subject or the response it has shown in a pre-experimental period may substantially reduce the variance of R, and should always be considered when suitable measurements are either already available or can be made with little extra trouble. The calculations, however, present no novel features of special interest for the statistician; the analysis of variance of responses is expanded into analyses of variance and covariance, and the responses are adjusted to equality of the concomitant measurements by means of a simple or multiple regression equation. The estimate of relative potency is formed from these adjusted responses in the usual way, though the calculation of its variance is complicated by the adjustments.

(a) *Logarithmic Dose Metameter.*—Equation (24) shows that when the logarithm of the dose is used as the metameter in an assay based upon quantitative responses, for any specified numbers of subjects used for the two preparations the variance of M will be least if

$$M = \bar{x}_s - \bar{x}_t;$$

hence, so far as previous information on M permits, the doses should be chosen to give a mean difference equal to M. Unless the preparation to be assayed is very unusual, this condition can generally be satisfied sufficiently well to make the third term in equation (24) much smaller than the first two. The best use of a total of N test subjects will then be to assign $\frac{1}{2}N$ to each preparation. The linear relationship between x and the response will usually hold only over a limited range of doses; both the variance of b and the variance of M would be minimized by using $1/4N$ subjects at the highest and lowest doses of each preparation that could be employed without fear of going outside the limits of linearity, and the variance of the estimate would then be

$$V(M)_{min.} = \frac{4s^2}{Nb^2}\left[1 + \frac{2(M - \bar{x}_s + \bar{x}_t)^2}{d_s^2 + d_t^2}\right], \qquad (66)$$

where d_s, d_t are the intervals between the higher and lower values of x for the two preparations. Had the $\frac{1}{2}N$ subjects for the two preparations been divided between k_s, k_t equally spaced dose levels respectively, evaluation of S_{xx} shows that the variance would have been

$$V(M) = \frac{4s^2}{Nb^2}\left[1 + \frac{6(M - \bar{x}_s + \bar{x}_t)^2}{d_s^2\left(\dfrac{k_s + 1}{k_s - 1}\right) + d_t^2\left(\dfrac{k_t + 1}{k_t - 1}\right)}\right], \qquad (67)$$

which reduces to equation (66) when $k_s = k_t = 2$, but otherwise takes a higher value.

Purely from the point of view of obtaining the most precise estimate of relative potency, the popular " four-point " assay, involving equal numbers of test subjects at the highest and lowest doses of each preparation consistent with the hypothesis of a linear regression of y upon x, is the ideal. The validity of the analysis rests upon the assumption that the regression is linear and that the lines for the two preparations are parallel; Gridgeman (1943) and Wood (1944*a*) have indeed shown that if the true regression equations are quadratic the same estimate of M would be obtained from a four-point assay (for the special case $d_s = d_t$), but the estimate might be seriously misleading if the regression function were very different in form. The four-point assay provides data for a test of the significance of departures from parallelism, or, in other words, the significance of the discrepancy from Condition II, but gives no test of the adequacy of the metameters used for rectifying the dose-response relationship. One or more intermediate levels of dose must be included if a test of linearity is required; three doses of each preparation will suffice to test the possibility that the relationship is simply concave or convex, but more will be needed if sigmoidal tendencies are feared.

In this discussion it has been assumed that g, as defined by equation (26), is small for all the designs considered. The spreading of a specified number of subjects over an increased number of doses will tend to lower the precision of b and hence to increase g. No examination of the effect of changes in k_s and k_t on the expression for fiducial limits, (27), has yet been made, but it is clear that an increase in g will widen the true limits to a greater extent than the approximate. Providing that g remains sufficiently small for this effect to be ignored, the loss of efficiency in the estimation of M through using k levels of dose instead of two can be assessed from comparison of equations (66) and (67). The simplest and most important case is that in which the extreme range of values of x is the same for both preparations ($d_s = d_t$), for even if better knowledge of the standard permitted a wider range to be used, practical convenience would often suggest the contraction of this to d_t. The upper limit to the percentage efficiency of an arrangement with k levels of each preparation, equally spaced logarithmically and with equal numbers of subjects at each, may then be written

$$E_k(M) = \frac{100 \times \left[1 + \dfrac{(M - \bar{x}_s + \bar{x}_t)^2}{d^2}\right]}{1 + \dfrac{3(k-1)}{k+1} \times \dfrac{(M - \bar{x}_s + \bar{x}_t)^2}{d^2}} \quad . \quad . \quad . \quad . \quad (68)$$

Unless previous information on M is particularly poor, the mean values of x can usually be so chosen as to make $(M - \bar{x}_s + \bar{x}_t)$ a small proportion of d, and the loss of efficiency with moderate values of k is then not serious; Table 9 shows some values of the efficiency.

TABLE 9

Upper Limit to Percentage Efficiency of Symmetrical 2k-*Point Assay Relative to Four-point, Calculated from Equation* (68)

(g assumed to remain small)

	Values of $(M - \bar{x}_s + \bar{x}_t)/d$					
k	0·0	0·1	0·2	0·3	0.5	1·0
2	100	100	100	100	100	100
3	100	100	98	96	91	80
4	100	99	97	94	86	71
5	100	99	96	92	83	67
10	100	99	95	89	77	58
∞	100	98	93	86	71	50

In practice, assays in which $(M - \bar{x}_s + \bar{x}_t)/d$ exceeded 0·3 would not be considered very satisfactory, and values of this quantity greater than 0·5 are scarcely ever encountered. Consequently the inclusion of even as many as five dose levels instead of two will cause at most a 15 per cent. reduction in the precision of M, yet it will enable a thorough examination of departures from linearity to be made. If the only cause of curvature to be feared is a decrease in slope as the dose is increased, the most sensitive test will be achieved by the use of three dose levels for each preparation, taking the additional level half-way between the two extremes (on the logarithmic scale); one advantage of using a greater number, however, is that if the curvature is apparent only at the highest doses the results for these may be rejected and the remainder treated as showing a linear relationship. If the possibility of a sigmoidal curve has to be taken into account, four or five dose levels will be needed. The best number of levels to take for any particular assay must be decided from existing knowledge of the materials being assayed and of the type of deviation from linearity that may occur. Only if there is a practical certainty of linearity should the four-point assay be used, since the loss in precision resulting from the precaution of using more levels should be slight unless the change also causes a marked increase in g.

The variance of M is also reduced by any procedure which increases the regression coefficient, b. This, however, is a characteristic of the test subject, the stimulus and the experimental conditions, though it may be altered advantageously by using a carefully selected and more sensitive strain of subject or by some slight modifications in other conditions. When the assay is of the type previously described as analytical, more radical changes in the technique may be permissible if they promise to give a greater slope, since the same quantity is still being estimated; a comparison of the potency of two poisons, on the other hand, may give entirely different results for two different species of test subject, or under different experimental conditions, since, unless the one poison can be described as a dilution of the other in an inert diluent, there is no reason to suppose that the true value ρ is unaltered by the change.

(b) *Power Dose Metameter.*—When the dose metameter is a power of the dose, and providing that the linearity extends down to zero dose, the mean response to zero dose should lie both on the standard and on the test regression line. In microbiological assays, at present the most important of this class, linearity down to $x = 0$ can usually be ensured (Wood, 1946*a*); the further simplification that the dose as measured is itself a satisfactory metameter (in equation (9), $\lambda = 1$) is also general, but does not seriously affect the problem of efficient design.

The circumstance that one point can be found common to both lines leads to some economy in dose levels. Wood and Finney (1946) have shown that a common-zero three-point design, including zero dose and the highest levels of each preparation consistent with linearity, is very much more efficient than any four-point design using two non-zero levels of each preparation. The minimum variance of the estimated relative potency obtainable from a total of N tubes would occur with an assay having

$$\frac{n_0}{1 - R} = \frac{n_s}{R} = n_t = \tfrac{1}{2}N, \qquad (69)$$

where the two preparations are measured on arbitrary scales which make the doses of each one unit, and n_0, n_s, n_t are the numbers of tubes in the blank, standard and test dose-groups respectively; here and in the following paragraphs R is assumed to be less than or equal to unity, as would always be true in a satisfactory assay of this kind. Thus, if R were nearly unity, the best design would have practically no tubes at $x = 0$ and about equal numbers at $x = 1$ for each preparation.

But, just as for assays with a logarithmic dose metameter, additional dose levels are needed if the assay is to provide tests of validity and linearity. Wood and Finney have discussed this matter at length, and their conclusions need only be summarized here. The simplest design giving information on departures from linearity is what Wood (1946*a*) has called the common-zero five-point design, consisting of single and double doses of each preparation as well as zero-dose; the double dose of the standard preparation is chosen as the highest that can be used without risk of going beyond the limit of linearity, and the double dose of the test preparation is chosen as an amount expected to give a mean response close to, but not exceeding, that for the double dose of the standard. In these units R should be almost unity, and in a satisfactory assay the estimate should certainly lie between 1·0 and 0·7. Wood and Finney have given the complex expres-

sion for the percentage efficiency of this design relative to that specified by equation (69), namely:

$$E(R) = \frac{100\left\{ n_0(n_s' + 4n_s)(n_t' + 4n_t) + n_s'n_s(n_t' + 4n_t) + n_t'n_t(n_s' + 4n_s)\right\}}{N\left[R^2\left\{N(n_t' + 4n_t) - (n_t' + 2n_t)^2\right\} - 2R(n_s' + 2n_s)(n_t' + 2n_t) + \left\{N(n_s' + 4n_s) - (n_s' + 2n_s)^2\right\}\right]} \quad . \quad . \quad (70)$$

where n_0 is the number of tubes at zero dose, n_s', n_t' the numbers at the single doses of the two preparations, and n_s, n_t the numbers at the double doses. Again g is assumed to remain small throughout, a condition that will usually be satisfied without difficulty in microbiological assays. The simplest and most convenient arrangement is to take $N/5$ tubes in each group, whereupon equation (70) reduces to

$$E(R) = 350/(8R^2 - 9R + 8), \quad . \quad . \quad . \quad . \quad . \quad (71)$$

a function which increases from 50 at $R = 1{\cdot}0$ to 62 at $R = 0{\cdot}7$. This efficiency seems poor by comparison with the values for $k = 3$ in Table 9, but, as may be seen from Table I of Wood and Finney's paper, no re-distribution of the tubes increases it appreciably without seriously reducing the sensitivity of the linearity test. Seldom, if ever, is there any advantage in distributing the tubes unequally between the standard and the test preparations, but occasionally the increase in the precision of R caused by assigning a greater proportion of the tubes to the highest doses at the expense of zero dose (say 10 per cent. of all tubes at $x = 0$, 15-20 per cent. at $x = 1$, 30–25 per cent. at $x = 2$ for each preparation) more than compensates the experimenter for the less satisfactory linearity test. For most routine purposes the fully symmetrical five-point assay is likely to be suitable, but once again a decision must be based on previous knowledge of the potency and of the validity of the technique.

The five-point assay is adequate for the detection of simple concavity or convexity of the response curve, but more points will be necessary in order to detect sigmoidal departures from linearity. The simplest and most important class of design is the fully symmetrical common-zero $(2k + 1)$-point assay, in which $N/(2k + 1)$ tubes are assigned to zero dose, to the highest dose of each preparation, and to $(k - 1)$ equally spaced intermediate doses of each. Wood and Finney have given its percentage efficiency, when the results show satisfactory linearity up to the highest dose, as

$$E_k(R) = \frac{400(k + 1)(k^2 + k + 1)}{3k[(R^2 + 1)(5k^2 + 5k + 2) - 6Rk(k + 1)]}, \quad . \quad . \quad . \quad . \quad (72)$$

this being assessed relative to the arrangement specified by equation (69). Values of this function, which has a maximum at a point between $R = \frac{1}{2}$ and $R = \frac{3}{5}$ (depending upon the value of k), are shown in Table 10. The reduction in efficiency with increasing k is more marked than in

TABLE 10

Upper Limit to Percentage Efficiency of Symmetrical Common-Zero (2k + 1)*-Point Design Relative to Asymmetrical Three-point, Calculated from Equation* (72)

(g assumed to remain small)

k	*Values of R.* 1·0	0·9	0·8	0·7
1	67	73	79	84
2	50	55	59	62
3	44	49	52	55
4	42	46	49	51
5	40	44	47	49
10	37	40	43	45
∞	33	37	39	41

Table 9, but even so the loss through taking $k = 4$ instead of $k = 2$ may sometimes be unimportant by comparison with the gain in information on other points connected with the assay. The symmetrical nine-point design, indeed, is a useful arrangement for disclosing any sigmoidal trends.

In contrast with the logarithmic dose metameter, the precision of these assays is not necessarily increased by the choice of a strain of test organism which gives a greater standard regression coefficient; an improvement is only effected by an increase in the maximum expected response still on the linear portion of the response curve, and an increase in b_s the result of which is merely that the same maximum linear response is reached for a lower dose does not affect the precision attainable (Wood, 1946*a*).

(c) *Quantal Responses*.—The dependence of the weighting coefficient upon the expected response makes the efficient planning of assays based upon quantal responses more difficult than those discussed in (*a*) and (*b*). Again the variance of M will be least when

$$M = \bar{x}_s - \bar{x}_t,$$

and any previous information on M should be utilized so as to attempt to satisfy this condition; usually this will be done by choosing corresponding doses of the two preparations which differ by a constant amount, believed to be about the value of M, on the logarithmic scale. If this condition could be fulfilled exactly, the variance of M, equation (24), would be minimized (for the probit transformation) by having all dose levels of each preparation close to that which previous experience suggests as likely to give 50 per cent. kill, since at this point w is greatest. But if information as complete as this existed, the assay would not be needed! Conversely, the less the previous knowledge about the potencies of the two preparations, the more likely is M, determined from the assay, to differ appreciably from the difference in mean log dose; even a small difference might contribute largely to the variance of M if a very restricted range of doses made S_{xx} very small.

A middle course must therefore be found between the too restricted dose range of the last paragraph and the other extreme of using doses which give either zero or 100 per cent. kills, since these will have very small values of w. If expected kills can be kept within the limits of 5 to 95 per cent., or say $Y = 3{\cdot}5$ to $Y = 6{\cdot}5$, the weighting coefficient never falls below one-third of its value at 50 per cent.; very little widening of the limits reduces the weights much more seriously, and at 1 or 99 per cent. w is little more than one-tenth of its maximum. Often a suitable practical procedure will be to use three, four, or five dose levels of the standard preparation equally spaced logarithmically over the range expected to give 10 to 90 (or perhaps 5 to 95) per cent. kill, and corresponding levels of the test preparation at these amounts increased by the value of M which previous information suggests. These doses may have to be selected by guess as much as by knowledge, but they will be the best that can be chosen on the evidence available; if so little is known of the potencies in advance, only by good fortune will a precise assay be obtained at the first attempt.

Of course, the observed kills at any dose may deviate widely from their expectations, especially when the groups of subjects are small. Indeed, as an extreme instance, only one subject might be tested at each dose, though of course many more than five levels would then be needed, and every observation would be of zero or 100 per cent. kill. For example, in some techniques for the assay of insecticides, the feeding of a specified amount of poison is impracticable, and instead the amount ingested by an individual must be measured as the difference between the amount supplied and the amount remaining, a quantity which will generally not be the same for any two individuals. The statistical technique is not altered materially, though the estimation of provisional regression lines is more difficult and the computations are generally lengthy; the χ^2 tests, however, are likely to be unsatisfactory unless some grouping is adopted (Garwood, 1940; Finney, 1947).

8. SUMMARY

Every biological assay depends upon a response curve, which is a regression relationship between the dose of a stimulus applied to a test subject and the magnitude of the expected response; for one material to be assayable in terms of the other, the two regression equations must be identical except for a multiplicative factor in the dose scale, which factor is the potency of the one preparation relative to the other. The central statistical problem of biological assay is the esti-

mation of this relative potency from experimental results and the assessment of its precision. In most types of assay the regression equations must first be estimated, and usually the data must also be tested for their agreement with the basic assumption that the true equations are identical.

In this paper an attempt has been made to show the unity of all assay problems. The statistical analysis of a large and important class can be simplified by means of transformations of dose and response which make the regression relationship between the transformed quantities linear. The maximum likelihood equations for the estimation of relative potency in these assays have been developed in general form, and a computational procedure has been described by means of which their solution becomes a process of successive approximation to the linear regression. The method is a generalization of the familiar technique of probit analysis, and includes all the simple types of macro- or microbiological assay, based either upon quantitative or upon quantal responses, in regular use to-day. It also includes various transformations for quantal responses which have been proposed as alternatives to the probit. The maximum likelihood estimation procedure follows the same pattern for all of these, so that when once certain preliminary tables have been prepared, none has any advantage in respect of ease of computation. The only serious competitor of the probit is perhaps the logit, based upon the logistic curve; a little consideration shows that the apparent advantages of this are largely due to a misconception of the basis of the transformation. Nevertheless, the logit possesses some interest, and tables to assist its use, analogous to the familiar tables for the probit, are presented together with a numerical example of the computations. The regression procedure is shown also to be applicable to the formation of the maximum likelihood estimate of a bacterial population from a dilution series.

The last section of the paper is concerned with the design of assays, especially in respect of the choice of the number and location of dose levels and of the number of test subjects at each dose. Usually an assay is limited to a specified total number of subjects, and the experimenter has to distribute these in the best possible manner between several doses of the standard and test preparations. The design from which the most precise estimate would be obtained generally does not permit a sensitive test of the validity of the assay; a compromise must then be sought which provides an adequate validity test without requiring too great a sacrifice in the main objective, the precise estimation of potency. Tables are given to show the reduction in precision consequent upon an increased number of dose levels. Three or more points must be determined for each regression line if any test of linearity is to be made, and the choice of doses can be much assisted by any previous information on the potency. There is seldom any reason for departing from a symmetrical design using equal numbers of test subjects at corresponding doses of the standard and test preparations.

ACKNOWLEDGMENTS

This paper owes much to discussion and correspondence on the nature of biological assay with many who are directly concerned in assay practice. Particularly am I indebted to Dr. E. C. Wood, whose stimulating suggestions and whose refusal to be satisfied with any incomplete answer have assisted the clarification of many problems.

Table 3 is taken, with consent of the authors, from Table XII of *Standard Four-Figure Mathematical Tables* by L. M. Milne-Thomson and L. J. Comrie; a trivial change of the argument and the addition of 5 to the tabular value are the only alterations.

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APPENDIX (added after the Meeting)

A FURTHER NOTE ON ASSAY DESIGN, EFFICIENCY, AND RELIABILITY

SINCE this paper was completed, it has been pointed out to me that the discussion of assay design in Section 7 underestimates the ill-effects of dividing a specified total number of test subjects between many doses. The results given in that section are true without qualification provided that b (or b_s) is very large compared with its standard error. On the other hand, if this condition is not satisfied, any increase in k, the number of dose levels, causing an increase in the variance of the regression coefficient, may so increase the quantity g that the fiducial range assigned to the relative potency is widened to a greater extent than is indicated by the "efficiencies" listed in Tables 9 and 10. In this Appendix the effect on the fiducial range of changes in g, induced by changes in k, will be briefly considered.

For quantitative responses and a logarithmic dose metameter, the efficiency of a symmetrical $2k$-point assay relative to a four-point has been expressed by equation (68). This expression involves the use of equation (24) for the variance of M. Since equation (24) can be used in assessing the fiducial limits for M only when g is small, a more reasonable measure of the reliability with which M can be estimated is perhaps given by consideration of the magnitude of the fiducial range. If $\frac{1}{2}N$ subjects are assigned to each preparation, expression (27) shows that

$$\text{Square of fiducial range} \propto \left[\frac{4(1-g)}{N} + \frac{(M - \bar{x}_s + \bar{x}_t)^2}{S_{xx}}\right] \div (1-g)^2.$$

For the symmetrical $2k$-point assay

$$S_{xx} = \frac{Nd^2}{12} \times \frac{k+1}{k-1},$$

whence, from equation (26)

$$g = \frac{k-1}{k+1} \times \frac{12s^2t^2}{Nd^2b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (73)$$

If the value of t is chosen to correspond with a selected probability, the second factor in this expression for g is unaffected by changes in k (except for sampling variation). An increase in k might also alter the subdivision of degrees of freedom in the analysis of variance and allow fewer degrees of freedom for the estimation of s^2, so increasing t; when N is moderately large, however, a slight change in the degrees of freedom has little effect on t. From equation (73), as k increases from 2 to a large number, the value of g is almost trebled, and this may result in a widening of the fiducial range by a greater amount than equation (68) suggests. In fact, by substitution in the earlier formula,

$$\text{Square of fiducial range} \propto \frac{1 - \dfrac{k-1}{k+1}(A - B)}{\left(1 - \dfrac{k-1}{k+1}A\right)^2}, \quad . \quad . \quad . \quad . \quad . \quad (74)$$

where

$$A = \frac{12s^2t^2}{Nd^2b^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (75)$$

and

$$B = \frac{3(M - \bar{x}_s + \bar{x}_t)^2}{d^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (76)$$

If A is zero, the ratio of the squares of the fiducial ranges for $k = 2$ and for any other value of k is the same as the "efficiency" of the $2k$-point assay stated in equation (68) and tabulated in Table 9. The introduction of A into equation (74) shows how the fiducial range is affected by the increased variance of b resulting from increase in k; the ratio of this expression to its value for $k = 2$ may be termed the *reliability* of the assay. Table 11 shows the percentage reliability of the

$2k$-point assay, relative to the corresponding four-point, for a series of values of A and two extreme values of $(M - \bar{x}_s + \bar{x}_t)/d$. Since A is a function of t, the reliability as here defined depends upon the level of probability chosen for determining the fiducial limits; the efficiency (Table 9) may be regarded as the limit to which the reliability tends as the probability is increased to unity and t therefore decreased to zero.

TABLE 11

Percentage Reliability of Symmetrical 2k-*Point Assay Relative to Four-point, Derived from Equation* (74)

(i) $(M - \bar{x}_s + \bar{x}_t)/d = 0$

	Values of A [equation (75)]					
k	0	0·2	0·4	0·6	0·8	1·0
2	100	100	100	100	100	100
3	100	96	92	88	82	75
4	100	94	88	80	71	60
5	100	93	85	75	64	50
10	100	90	78	64	47	27
∞	100	86	69	50	27	0

(ii) $(M - \bar{x}_s + \bar{x}_t)/d = 0{\cdot}3$

	Values of A [equation (75)]					
k	0	0·2	0·4	0·6	0·8	1·0
2	100	100	100	100	100	100
3	96	92	87	82	75	67
	94	87	80	71	61	48
	92	84	75	64	52	37
10	89	78	65	49	32	14
∞	86	70	53	33	13	0

Inspection of Table 11 indicates that in general there will be no very serious loss through using three levels of dose instead of two, unless A, a characteristic of the assay technique and test subject (and of the selected level of probability) is large. Providing that A is less than 0·4, use of as many as five dose levels instead of two will at worst reduce the reliability to 75 per cent. of its maximum, unless an unfortunate choice of doses leads to an exceptionally large value of $(M - \bar{x}_s + \bar{x}_t)$. This, admittedly, compares unfavourably with the statement in Table 9 that for $k = 5$ and $(M - \bar{x}_s + \bar{x}_t)/d = 0{\cdot}3$ the loss of efficiency is only 8 per cent.; nevertheless, the increase in the fiducial range corresponding to the values in Table 11 (a maximum of 15 per cent. of its value for $k = 2$) may be a small price to pay for data leading to adequate tests of assay validity. The contention that at least three dose levels of each preparation should be used, unless there is practical certainty of linearity, may still be maintained, for even a 33 per cent. loss in reliability (for example, with $(M - \bar{x}_s + \bar{x}_t)/d = 0{\cdot}3$, $A = 1{\cdot}0$) is likely to be preferable to the absence of any linearity test.

A similar argument may be developed for the situation in which the dose metameter is a power of the measured dose, and, in particular, when the dose itself may be used as the metameter. From (32) above,

$$\text{Square of fiducial range} \propto [v_t - 2Rc + R^2 v_s - g(v_t - \frac{c^2}{v_s})] \div (1 - g)^2.$$

In a symmetrical $(2k + 1)$-point assay, in which the range of doses for each preparation is from 0 to d, it may be shown (Wood and Finney, 1946) that

$$v_s = v_t = \frac{3k(5k^2 + 5k + 2)}{Nd^2(k + 1)(k^2 + k + 1)}$$

and

$$c = \frac{9k^2}{Nd^2(k^2 + k + 1)},$$

whence, using equation (33),

$$g = \frac{k(5k^2 + 5k + 2)}{(k + 1)(k^2 + k + 1)} \times \frac{3s^2t^2}{Nd^2b_s^2} \quad . \quad . \quad . \quad . \quad . \quad (77)$$

As in equation (73), the second factor in equation (77) is independent of k, except for sampling variation and possibly small changes in t due to alterations in degrees of freedom. This expression for g increases by a factor of 2·5 as k increases from 1 to infinity. Substitution for v_s, v_t, c and g now gives

$$\text{Square of fiducial range} \propto \frac{K_1(1 + R^2) - K_2R - K_3A}{(1 - K_1A)^2}, \quad . \quad . \quad . \quad . \quad . \quad (78)$$

where

$$A = \frac{3s^2t^2}{Nd^2b_s^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (79)$$

$$K_1 = \frac{k(5k^2 + 5k + 2)}{(k + 1)(k^2 + k + 1)},$$

$$K_2 = \frac{6k^2}{k^2 + k + 1},$$

$$K_3 = \frac{4k^2(2k + 1)^2}{(k + 1)^2(k^2 + k + 1)}.$$

In Section 7, the efficiency of the symmetrical $(2k + 1)$-point assay was expressed relative to an asymmetrical three-point assay with the subdivision of N test subjects specified by equation (69). This subdivision was determined so as to minimize the variance of R expressed in equation (31); when R is near to unity, it assigns very few subjects to zero dose, and thus makes the precision of estimation of the regression coefficients very much lower than for a symmetrical three-point design. For many types of microbiological assay, s^2, the variance of responses about their regression lines, is very small; with a moderate level of probability, even a reduction to a single tube at zero dose may then not make g sufficiently large to affect the use of equation (31) in assessing fiducial limits. (Nevertheless, to assign no tubes to tests at zero dose, as would be required by equation (69) with $R = 1$, would clearly make the whole assay useless.) For these assays, the efficiency, given by equation (72) and Table 10, is also a satisfactory measure of the reliability. For other classes of assay in which the response is directly proportional to the dose, and when R is near to unity, the effect of using the three-point assay specified by equation (69) may be so to increase the variance of b_s that g becomes large; equation (31) is then not adequate as a means of assessing the fiducial range. In such assays it is scarcely of practical importance to discuss the subdivision of subjects given by equation (69), as this would never be used; more usefully, perhaps, the efficiency of the $(2k + 1)$-point design may then be expressed relative to the symmetrical three-point, which involves replacing equation (72) by—

$$E'_k(R) = \frac{200(R^2 - R + 1)(k + 1)(k^2 + k + 1)}{k[(R^2 + 1)(5k^2 + 5k + 2) - 6Rk(k + 1)]} \quad . \quad . \quad . \quad (80)$$

Table 10 would be replaced by Table 12, and, over the range studied, the efficiency so defined is seen to be practically independent of R.

TABLE 12

Upper Limit to Percentage Efficiency of Symmetrical Common-Zero (2k + 1)-*Point Design Relative to Symmetrical Three-point, Calculated from Equation* (80)

(g assumed to remain small)

k	Values of R 1·0	0·9	0·8	0·7
1	100	100	100	100
2	75	75	74	74
3	67	67	66	65
4	62	62	62	61
5	60	60	59	58
10	55	55	54	54
∞	50	50	49	49

When g cannot be neglected, the reliability may be defined as before by means of ratios of squares of fiducial ranges obtained from equation (78). This reliability becomes the same as the efficiency stated in equation (80) when A is put equal to zero; the introduction of non-zero values for A makes allowance for the increased variance of b_s resulting from an increase in k. The percentage reliability of the $(2k + 1)$-point assay relative to the three-point is given in Table 13 for a series of values of A and two extreme values of R. As for the logarithmic dose metameter, A is a function of t and the reliability depends upon the level of probability chosen for the fiducial limits; again, the efficiencies of Table 12 are limits to which the reliabilities tend as the probability is increased to unity and t decreased to zero.

TABLE 13

Percentage Reliability of Symmetrical Common-Zero (2k + 1)-*Point Assay Relative to Symmetrical Three-point, Derived from Equation* (78)

(i) $R = 1·0$

k	Values of A [equation (79)] 0	0·04	0·08	0·12	0·16	0·20
1	100	100	100	100	100	100
2	75	71	66	60	52	42
3	67	61	55	47	37	24
4	62	56	49	40	29	16
5	60	54	46	36	25	11
10	55	48	39	29	16	4
∞	50	42	33	22	9	0

(ii) $R = 0·7$

k	Values of A [equation (79)] 0	0·04	0·08	0·12	0·16	0·20
1	100	100	100	100	100	100
2	74	70	66	62	56	48
3	65	61	56	49	41	30
4	61	56	50	43	34	22
5	58	53	47	39	30	17
10	54	48	41	32	21	8
∞	49	42	35	25	14	0

Between $R = 1{\cdot}0$ and $R = 0{\cdot}7$ the reliability is practically independent of R. Table 13 confirms the indications of Tables 10 and 12 that the increase in the fiducial range consequent upon an increase in k is more serious for these assays than for assays with a logarithmic dose metameter. Unless there is almost a certainty of linearity over the whole range of doses, however, some test of linearity must be included and $k = 2$ is the least value to be considered; the further loss when k is increased to 4 is not great, providing that A is small (though it may be important for large A), and is often a useful insurance against the possibility that the highest doses are beyond the range of linearity. In any satisfactory microbiological assay, db_s is likely to be at least of the order of 5 or 10 times s (in the nicotinic acid assay discussed in Section 4, for example, about 55 times); inspection of equation (79) shows that A will then normally be below 0·04, and probably below 0·01. For this most important class of slope-ratio assays, the reliability of the symmetrical five-point design is about 75 per cent. of that for the symmetrical three-point, essentially the same result as for the efficiency, and there is a further loss of about 15 per cent. if a symmetrical nine-point design is used; these figures represent increases of about 15 per cent. and about 30 per cent. in the fiducial ranges by comparison with the symmetrical three-point.

No detailed discussion of the fiducial range for a potency estimate from an assay based on quantal responses will be presented here. Considerations arise similar to those for quantitative responses with a logarithmic dose metameter; an increase in k may again increase g and consequently widen the fiducial limits to a greater extent than mere consideration of variances derived from equation (24) would suggest, but the situation is complicated by the differential weighting at different levels of mortality. Once again three levels of dose for each preparation, rather than two, are practically essential, unless there is almost a certainty of linearity; whether or not more are used in any assay must depend upon the particular circumstances, upon existing knowledge in respect of linearity, slope, and variability, and finally upon the discretion of the experimenter.

DISCUSSION ON MR. FINNEY'S PAPER

MR. E. C. FIELLER: Mr. Finney has given us a stimulating synthesis, with some elegant and useful additions of his own, and it is a pleasure to propose this vote of thanks to him. He has written, he says, for statisticians rather than for biologists or executives; I find the boundaries of these three classes increasingly difficult to trace, but in any case I am sure that Mr. Finney will deservedly have a wider public than he claims.

Although the paper stresses the formal analogy between assays in which the response is quantal, and those in which it is continuous, I find it more convenient to discuss them separately. One of the most surprising features of the statistical theory of quantal-response assays is that although it can be formulated quite concisely, as Mr. Finney has shown, it nevertheless took some sixteen years to reach its present refined state. Trevan and Gaddum, I think, broke the back of the problem; in 1927 Trevan showed that in a quantal-response assay the problem is that of comparing two frequency distributions of individual effective doses, and in 1933 Gaddum reduced it to the weighted-regression form by using as ordinate the equivalent deviation, in place of the percentage reacting. In 1935 Bliss, writing from the Galton Laboratory, inverted the Working-Hotelling argument to obtain fiducial limits for the dose corresponding to an arbitrarily-fixed percentage-reacting; at the same time Fisher applied the method of maximum likelihood to deal with the extreme cases in which, in one or more of the dosage-groups, the observed percentage-reacting is 0 or 100. Irwin's discussion in 1937 led, as Bliss has said, to a reconsideration of the problem, and in 1938 the full maximum likelihood method for dealing with a single dosage-mortality line was announced. Irwin and Cheeseman in 1939, and Garwood in 1940, gave a mathematical discussion of this case; the former authors dealt also with the assay problem. We had to wait until 1943 for a specific discussion, by Irwin, of the fiducial limits for a quantal-response assay, although it might be claimed, I suppose, that the result happened to be contained in a more general one that I had given in 1940.

In 1934 or 1935 something else happened that seems to me less useful. Gaddum's normal equivalent deviations were increased by 5, and rechristened *probits*. Mr. Finney has dutifully done the same for the logistic equivalent deviation, but I doubt whether it is worth while. He avoids negative numbers in his illustrative Table 5 (but not in his basic Table 4) at the expense of writing more digits; I think that I would myself have introduced negative numbers into the example anyway, by using -1, 0 and $+1$ as working abscissae. For routine calculations I have found it helpful to form a table giving w and wy, rather than w and y, and here the addition of 5 to the equivalent deviation would be a real inconvenience, since it would double the size of such a table.

I should like to mention here one difficulty that I feel about quantal-response theory, even though it may be of little practical importance. It is customary in such assays to calculate the fiducial limits from a formula analogous to Mr. Finney's equation (27), as he had done in his Section 5. Now when we are dealing with a continuous-response assay this formula can be rigorously established; we need only consider, in the light of the experimental data, what range of values we can accept for a quantity estimated as the ratio

$$\frac{\bar{y}_s-\bar{y}_t}{b}$$

where the numerator and denominator are both linear functions, with constant coefficients, of the observed responses. In the quantal case, however, there is the added complication that our ratio involves not only the adjusted equivalent deviations but also the estimated weights, the sampling errors of which are ignored in the usual derivation of the formula for the fiducial limits. Mr. Finney's formula (27) can of course be written in a variety of ways; I have found it convenient to put (in his notation)

$$C = \frac{1}{1-g} = \frac{b^2}{b^2-t^2V(b)},$$

so that the limits are $M' \pm s'_M t$ where

$$M' = (\bar{x}_s - \bar{x}_t) - C\frac{\bar{y}_s-\bar{y}_t}{b}$$

and

$$t^2 s'^2_M = C\frac{t^2s^2}{b^2}\left[\frac{1}{N_s}+\frac{1}{N_t}+\frac{C}{S_{xx}}\left(\frac{\bar{y}_s-\bar{y}_t}{b}\right)^2\right]$$

An attractive alternative form for this last equation,

$$t^2 s'^2_M = (C-1)\left[S_{xx}\left(\frac{1}{N_s}+\frac{1}{N_t}\right)+C\left(\frac{\bar{y}_s-\bar{y}_t}{b}\right)^2\right],$$

was suggested to me by a remark of H. P. Marks, who will be well remembered as an inspiring and very practical-minded worker in the field of biological assay.

In discussing quantitative-response assays, Mr. Finney has recalled that in the symmetrical four-point assay the assumption of a parabolic log (dose)-response line, instead of a straight one, leaves the formulae unaltered. It seems best to regard this equivalence as a fluke. If the doses of test are in the same ratio as the doses of standard, then whether the dosage-groups are equal or not, the parabolic assumption leads us to estimate the log (potency ratio) as

$$\{(y_{t_1}+y_{t_2})-(y_{s_1}+y_{s_2})\}/\{(y_{t_1}-y_{t_2})+(y_{s_1}-y_{s_2})\},$$

multiplied by the appropriate scaling factor. Here y_{s_1} and y_{s_2} are the mean responses to the higher and lower doses of the standard, and y_{t_1} and y_{t_2} have similar meanings for the test preparation. It is easily seen that if the variance is independent of the dose, there will in general be a correlation between the numerator and denominator of this fraction, which will modify the calculation of fiducial limits, and that the formulae for interpreting the assay will coincide with those derived from the assumption of linearity only when the four dosage-groups are all equal. Some years ago I used this modified procedure on one set of data, but only one; I finished up with wider fiducial limits than I would have had, if I had rejected the dose that appeared to fall in the flat portion of the log (dose)-response curve. This is of course no basis for argument, but I still class the procedure as one that is nice to know, but dangerous to use; it seems desirable at any rate, if one does use it, to verify that the turning point of the resulting parabola lies outside the relevant dose-range, so that Mr. Finney's Condition I is not violated.

As Mr. Finney has mentioned in a footnote to his third section, I prefer myself, when dealing with an assay method using quantitative responses, to begin by finding a response metameter that equalizes the variance, rather than one that straightens the log (dose)-response line. Of course, if there is a metameter that does both jobs (and this is what is assumed of the metameter, in applying the standard procedure), the route by which we arrive at it does not matter. The difference between our approaches is due, I think, to the fact that Mr. Finney has primarily considered the needs of the research worker who wants, at the end of his experiment, to make full use of all the information he has obtained; whereas I have had in the past to consider the needs of the laboratory engaged in routine assay, where the objective must be to ensure beforehand that every animal used will supply a reasonable amount of information. I believe that in most macro-biological assay-methods we shall find, if we measure responses on a scale that stabilizes their variance, that the log (dose)-response line is logistic in form, with the steep central portion effectively linear. Outside this portion changes in dose will produce relatively little change in response,

so that the regular occurrence of curved log (dose)-response lines is an indication that some of the animals are being used inefficiently.

I want, in conclusion, to make one general point. I think that it is the business of the mathematical statistician to provide objective methods of drawing conclusions from experimental data, and for that reason I hope that none of Mr. Finney's readers will imagine that he advocates graphical methods as more than a preliminary to doing the sums. There is not much point in doing an assay unless some decision has to be based on its outcome, and the decisions that have to be made are usually of the type: "What is a fair price for this oil?" "What is the therapeutic value of this preparation?" "How much ought we to dilute this concentrate?" "Does this preparation conform to specification?" I believe that rational methods of answering such questions can be given in terms of estimates of activity and fiducial limits, and not in any other terms, and accordingly that the calculation of these quantities should be regarded as an integral part of the assay procedure.

I have much pleasure in proposing a vote of thanks to Mr. Finney for his informative and stimulating paper.

Dr. IRWIN: Before seconding this vote of thanks, may I say what a pleasure it is to see here two of the real pioneers of the subject, Sir Percival Hartley and Dr. Trevan.

It was the stimulus of Professor Gaddum's now famous report on biological assay using quantal responses which first led me to get interested in the subject, and so to the survey which I gave this Society 10 years ago. I little imagined at the time that interest would spread in the way in which it has. In their review in 1943 Bliss and Cattel listed 275 papers, and there must be many more by now.

To Mr. Finney we are enormously indebted for re-stating lucidly the old points and introducing us to the new. His distinctive contributions are the general treatment of the problem of transforming the response curve into a straight line and the work on microbiological assays where the response is linearly related to the metameter. He has dealt with both problems in an extremely able way; I particularly admire the ingenuity of using a double regression function to express simultaneously the arithmetic relations in test and standard.

I quite agree with Mr. Finney that approximate methods of evaluating the result of an assay are often quite good enough for routine purposes, also that drawing a graph of the data is always an instructive procedure. I do not, however, think that a graphical method should be used for routine estimation; some simple method such as Behren's or Kärber's in the quantal case is more satisfactory because it can be agreed upon by several workers who would get the same result for the same data.

In the purely formal treatment of the general problem of rectification on p. 51 it was unnecessary for him to assume that the variances of the untransformed response are the same at all dosage levels. If they are not, the effect is simply to introduce $1/\sigma^2$ under the summation signs in equation (17) with a consequent modification of the weights in equation (19), otherwise everything is unchanged. This brings the treatment of quantitative and quantal responses completely into line with one another. There remains the difference that in the quantitative case we can actually examine the variances of the *transformed variate*. If they are found not to differ significantly at the different dosage levels, unequal weights and successive approximations are unnecessary, as Mr. Finney has indicated.

One always hopes that this will prove to be so; the successive approximation procedure, which one is bound to employ in the quantal case, is rather tedious; I have found many cases in which four approximations are necessary to get 2 decimal accuracy in b, and the worse the data the more the number of approximations usually required. The example in Table 5 is, compared with my general experience, more than usually favourable.

I agree with Mr. Finney that the logistic response curve has no advantage over the normal for quantal responses, but it may occasionally be useful for continuous responses, and Dr. Emmens has employed it for that purpose.

I have sometimes wondered whether the introduction of the probit was any improvement over the original normal-equivalent deviation. We avoid negative quantities at the expense of using larger numbers, and double the number of entries necessary in a table of maximum or minimum working probits. Also the habit of adding 5 seems to necessitate a new jargon. Statisticians brought up in the Victorian tradition liked using long words with Greek derivations. For example, the late Professor Karl Pearson introduced the term "heteroscedasticity" for unequal scatter. Now the pendulum has swung the other way. We have *probits* and *logits* and *rankits*. What are we to call the equivalent deviation of Section 6? The term *logit* being already appropriated, should it be an *expit*? There is a danger that the unitiated may come to think that they are all *rackits*. I would put in a plea for the simple abbreviation E.D. in all cases.

Before I leave quantal responses may I pay a tribute to Professor Gaddum's intuition. In his 1933 paper he introduced the correct test (at any rate the best test we have up till now) of goodness of fit by taking $\chi^2 = \dfrac{Sn(p-P)^2}{PQ}$ which fits in with the maximum likelihood solution.

Actually there remains the point that the distribution of this χ^2 is not precisely the same as that tabulated, since we are dealing with binomial, not normal distributions, and if we have, say, only 3 doses with one very large and another very small expected mortality, the disturbance may be appreciable. We discussed this point ten years ago, and I don't think it is quite settled yet. Bliss thought that for very small and very large doses the contribution to χ^2 would tend to be too small and I thought the expectation would be correct. On p. 66 Mr. Finney says χ^2 may be unduly large. I think that we were all three right. I have worked out the distribution of the contribution to χ^2 for a mortality of 0·05 and 10 animals. In 91 per cent. cases the contribution is ·526; in the remaining 9 per cent. it exceeds 4·7. Its expectation is unity. Thus the contribution will most often be too small, but it will also be too large more often than the tables of χ^2 allow for; were the distribution of the square root (χ) of the contribution normal, the former would exceed 4·7 in only 3 per cent. of cases. It is the intermediate values that are lacking. Just after working this out, I received the results of a number of assays of an antigen with which we have lately been concerned. There were 6 experiments, comprising 26 tests, and 3 doses with 15 animals each to a test. There were no significant differences in slope, and the analysis of variance of the 26 slope estimates is as follows:

		S. of S.	*D.F.*	*Mean Sq.*
Between experiments		2·6387	5	0·5278
Within Experiments	Between tests	19·1364	20	0·9568
	Within tests	31·5252	29	1·0871

The mean square between experiments is somewhat below its expected value but not significantly so. However, the striking feature of the table is the closeness of the mean squares to unity, the theoretical value which they should have if the χ^2 distribution holds.

May I now make a remark about the four point assay with equal dose intervals and equal weights mentioned on p. 57. It is easy to show that the contribution to the sum of squares in the analysis of variance of the responses made by the quadratic term is precisely the same as that due to the difference between slopes when the quadratic term is ignored. In other words if linearity is assumed, the assay provides a test of parallelism; if parallelism is assumed, the assay provides a test of departure from linearity. The position is much clarified if the two parabolas are fitted in orthogonal form. If we give 1 and 2 units of standard and 1 and 2 assumed units of test, or constant multiples of these doses, and take our origin mid-way between the upper and lower doses, the two response curves are easily found to be—

Standard $Y = \bar{y} + bx - \tfrac{1}{2}Mb + c\{(x - \tfrac{1}{2}M)^2 - \tfrac{1}{4}(1 + M^2)\}$.

Test $Y = \bar{y} + bx + \tfrac{1}{2}Mb + c\{(x + \tfrac{1}{2}M)^2 - \tfrac{1}{4}(1 + M^2)\}$.

with $b = \tfrac{1}{2}(-y_1 + y_2 - y_3 + y_4)$, $Mb = \tfrac{1}{2}(-y_1 - y_2 + y_3 + y_4)$

$$c = \frac{1}{2M}(y_1 - y_2 - y_3 + y_4)$$

or $Y = \bar{y}\,\xi_0 + b\xi_1 + (Mb)\,\xi_2 + c\xi_3$

where we have

y	ξ_0	ξ_1	ξ_2	ξ_1
y_1	1	$\frac{1}{2}$	$\frac{1}{2}$	$+\frac{1}{2}$M
y_2	1	$+\frac{1}{2}$	$\frac{1}{2}$	$-\frac{1}{2}$M
y_3	1	$-\frac{1}{2}$	$+\frac{1}{2}$	$\frac{1}{2}$M
y_4	1	$+\frac{1}{2}$	$+\frac{1}{2}$	$+\frac{1}{2}$M

whence, by the usual rule, we may write

$$\therefore c = \tfrac{1}{2}\mathrm{M}(y_1 - y_2 - y_3 + y_4)/\mathrm{M}^2$$

and its contribution to the sum of squares

$$\therefore \tfrac{1}{4}(y_1 - y_2 - y_3 + y_4)^2$$

In actual practice the most important thing is to make sure that conditions I and II are satisfied. Even then there may be complications in practice. For example, 0·6 μg of β carotene is by definition equivalent to 1 unit of vitamin A, and the two are in fact equivalent for rats, but they

are anything but equivalent for man, who only utilizes about $\frac{1}{4}$ of the β carotene. Similarly vitamin D and vitamin D_3 are equivalent for rats, but anything but equivalent for chickens.

In such cases writers should be careful about the terminology they use; otherwise much confusion can result.

I have very great pleasure in seconding the vote of thanks to Mr. Finney for a stimulating and original paper.

Sir PERCIVAL HARTLEY said it would be impossible for him adequately to discuss the paper. He had read it with such intelligence as he possessed, and in course of time he thought he would be familiar with it, but he was one of those people present who belonged to biology and not to statistics. He felt that it was a great privilege to be there among the advance guard of that new scientific activity. He seemed to have spent the greater part of his own time with the mopping-up party in the back areas, dealing with questions which he supposed those present considered to be settled and done with, and not requiring any further attention; nevertheless he still found that he had to be quite active in such elementary things as pointing out the importance of material standards for this work. Had he known that he was going to be called upon to speak, he would have shown an exhibition case which was in his possession containing samples of 35 biological standards, representing one line of progress in this field which had been achieved during the inter-war period.

Further, it was still necessary to point out the difficulties which arose in attempting to define a unit in terms of animal reaction; and he had had many discussions in the "back areas" in which he had pointed out that the error of a biological assay, which was just as important as the assay itself, and was really a part of it, ought to be recognized, estimated and stated. He had also had much work at times in pointing out to his biological colleagues that the proper time to consult the statistician was before a large piece of work was undertaken, and not when it was finished; and that experimental design was greatly benefited by these preliminary consultations.

He would like to say what a pleasure it was to him to be at this meeting because his own first contacts with this subject dated back to forty years ago, when he was working at the Lister Institute as a young scholar. At that time Sir Charles Martin had been Director of the Institute for a year or two, and one of his early activities was the preparation of the Report of the Plague Commission based on the enormous mass of data accumulated in India and in this country. He realized that the proper interpretation of the results called for the expert statistician, and it was in that connection that Professor Major Greenwood, who came to handle the data, became his friend and colleague at the Lister Institute. Personally, after listening to Greenwood, he felt that he would always have to examine tables and figures more critically than he had ever done before. When he recalled those early days at the Lister Institute, and his young contemporaries who enjoyed the same privilege, he thought that it was this kindly interest and encouragement of Greenwood which was the beginning of a new activity of which this inspiring gathering to-night was but one manifestation.

It was the starting-point for the journey which had brought them to this gratifying present position. He could see some of the milestones along that road. One of the earliest and most outstanding was the circumstance, whatever it might have been, which took Professor R. A. Fisher to Rothamsted. Fisher would have adorned and enriched any part of the scientific field, in which he is a recognized master; but biologists were extremely fortunate in securing his interest and his help in their problems.

Another milestone on this road was the paper published in 1927 by Trevan in the *Proceedings of the Royal Society* on "The Error of Determination of Toxicity." This gave a real "shake-up" to research workers in the field of serology and immunology; it made them stop and think what the meaning of the terms they used might be. From this many advantages had followed, not the least being that first Burn, and then Gaddum, had become interested in this field and they, in turn, had aroused the interest and practical assistance of others. Burn and Gaddum had been his colleagues at the National Institute for Medical Research at Hampstead, and he had learned a great deal from them. Burn was with them at Hampstead for many years before going to the College of the Pharmaceutical Society to start the pharmacological laboratory there. Gaddum followed Burn at these laboratories, and he thought that a tribute should be paid to that Society and to its laboratories because, through the work and leadership of Burn and Gaddum and Katherine Coward they had been provided with their raw material in the shape of a magnificent quantity of first-rate biological assay data. At Hampstead they had also had the privilege of the services of Dr. Irwin as their adviser for a long time, and they had always found him most helpful. He owed more than he could ever repay to his intimate colleagues and assistants in his department, most particularly to the late H. P. Marks, whose tragic death had robbed them of a first-rate worker, to Mrs. Trevan, who was with him for many years during the time his department was

developing rapidly, and to Dr. C. W. Emmens, who only this year had come to his help in connection with some awkward questions requiring solution. Looking back on these three friends of his, he could not help realizing what a great deal they had got out of their lives which he himself had missed because, while they came into this matter as biologists interested in the actual carrying out of assays, they appreciated that they could go much further with their problems by learning how to use some of these statistical implements.

He hoped that, in the future, statisticians would perhaps come in closer touch with biologists in the actual conduct of their assays. In that way the statistician might learn something by seeing how biologists did their work, how they sometimes had to face up to rather difficult situations, and how they had to obtain answers with small groups of animals. Biologists had had a great deal of help from statisticians, but they wanted more. On the other hand, he thought that people who began life as he did, and spent most of it in the biological field, would be at a great advantage and would reap a rich harvest if, like several of his colleagues, they went on to learn something about the use of statistical methods.

Mr. N. T. GRIDGEMAN said that he spoke as a biochemist, not as a statistician, and the paper was so comprehensive in its scope and penetrating in its treatment that he had little to say about it. But in connection with Table 9 he wondered whether the author had considered the effect on the efficiency of assays of unequal numbers of subjects at the different points. It was possible, for instance, to have the extreme dosage levels more heavily weighted than the intermediate levels, which would lead to different "efficiencies" than equally weighted levels, although the total weight, i.e., the total number of subjects, would be the same in each case. At the same time, it would, of course, complicate the tests for linearity, etc.

Dr. Irwin had mentioned that the vitamin-D situation was complicated by the fact that vitamins D_2 and D_3 were equipotent for rats, but not for other animals—for example, chickens. He believed that they were also inequipotent for rats, but to a lesser degree than for chicks. Thus the situation would be of even greater complexity than Dr. Irwin believed.

Dr. J. W. TREVAN said that it was a great pleasure to hear this paper. He hoped that statisticians would bear in mind that the vast majority of these quantitative assays, both research and routine, were still carried out by people who belonged to that 90 or 95 per cent. of the community who found it difficult to think in algebra, and a little more elaboration from the statistician as to what was being done would be welcome. The bestowal of the mathematical gene in human beings was very rare.

He wished to raise one question which had bothered him. What was the effect of the discontinuity which became increasingly pronounced with smaller groups? Suppose one wished to estimate the error of determination of the value of b by an experiment on two groups of 8 animals which had given a mortality of 1/8 with one dose and 7/8 with another dose 1 log. unit higher. The figure he got, using the ordinary probit method, was that the probability was about 0·06 that b differed significantly from zero. If, however, he dealt with the figures by what Fisher called the "exact method" he got $p \doteq 0·005$.

He laid stress on this because groups as small as these were in fact efficient for certain biological assays, particularly those in the serological field, and he threw it out as a suggestion that perhaps someone might find an opportunity of dealing with this point.

Dr. C. W. EMMENS said that everyone whose business it was to perform biological assays should read Mr. Finney's paper, if only to impress on himself the depth of his ignorance. This communication was, of course, written for statisticians, and the author made that clear at the outset. He was sure that the author realized that the majority of people actually doing these assays needed much more in the way of detailed explanation, and needed also to be told more about what to do in practice. These remarks were not, however, intended to cast any reflection on a masterly treatment of the statistical position.

Two main points struck the speaker as needing further elucidation by the statistician. The first concerned the equalizing of variances by transformations of the response. In many assays we had a satisfactory linear relationship between dose or log.-dose and response, but the variance was not constant at all levels of response. If we equalized the variance, we had a curved dose-response line. Now it might be argued that we could always choose a narrow enough range of doses to make the curvature unimportant, but we could not always, in practice, guarantee to hit this range, nor would the estimate of the slope be very precise with a very narrow range. In many assays, time to time variation in response was such that, even with a wide range over which the dose-response line was straight, we sometimes overshot or undershot this range, and the narrower it was, the more trouble we should encounter from that source. So would the statisticians please

consider the relative merits of metameters which gave a wide linear dose-response range and unequal variances at different response levels, and those with a narrow range and equal variances —remembering the increasing use of such designs as the Latin square in these assays and the trouble one had if some responses had to be rejected.

The second point concerned quantal responses. When dealing with a graded response, we could take advantage of the greater similarity in the responses, of, say, litter-mates, or of the same animal at different times, as compared with responses from different animals. We arranged the assay so that differences between litters or animals could be segregated in an analysis of variance. How could we take advantage of this similarity of response when dealing with quantal responses? Some quantal assays did not, for instance, involve death of the animal, and it could be used repeatedly. We could, therefore, do cross-over tests as in the assay of insulin by the rabbit method in which a graded response was used. If we did this, estimates of differences between standard and test, of the slope, etc., which depended in part or whole on sums of differences of responses of the same group of animals at different times, would have a lower variance than would be the case had we not used a cross-over design. The statistical problem was, therefore, to estimate the correlation between responses from the same animal when these responses were quantal and not quantitative. In cases where a cross-over test was not possible, the equal distribution of litter mates between dosage groups should help to guarantee a more precise estimate of potency than would otherwise be possible. Once more, how do we take advantage of this in analysing quantal assays?

Mr. A. L. Bacharach said that he was neither a statistician nor a biologist but a mere chemist. Many years ago he was pitchforked into a position where he had to learn some biology; from that he was pitchforked into a position where he had to learn how to use and interpret statistics. He would like to say there, what he had said before in writing, that the amount of assistance he had had from statisticians—a number of them were in that room—had been very great and had always been given with the utmost willingness and with a patience which he thought could not be exceeded. He remembered being with a medical colleague who asked a medical man from across the Atlantic during the war, "May I pick your brains?" The American visitor replied, "You are welcome to anything you can find there." The same spirit had been shown in this field.

Although it would be quite impertinent for him to discuss either biology or statistics after making this disclaimer, and certainly to discuss the merits of the paper, which he hoped gradually to absorb, he wished to call attention to a question arising out of one of the author's *obiter dicta.* He had pointed out, as others had done previously, that the most efficient distribution of subjects for test, for the maximum precision of potency ratio, had sometimes to be modified because of the need for also establishing the validity of the assay by tests of linearity and parallelism and so on. Consequently it was often necessary to distribute the animals in such a way as to run something more than a risk—almost a certainty—that some of the responses would fall outside the range of linearity. If he might borrow an analogy he thought there was in assays a "time-number *continuum,*" and one could increase the time with decrease of numbers or increase the numbers with decrease of time! Unless time was the essence of the contract (and the assays took at least several days) it might be more economical and ultimately result in a smaller number of calculations if one cut the Gordian knot in a different way and carried out the work by first undertaking a preliminary assay over a wide range of doses for a shorter time—even for a much shorter time—than the ordinary period of the assay. It was possible by taking, say, three doses in the ratio of 1 to 4 to 16, to carry out a preliminary vitamin A assay for a week only, instead of three, and to get therefrom sufficient information to enable one to undertake subsequently a valid four-point assay, in which information from all the animals could be used.

These preliminary investigations—probe-tests, as he called them—seemed to be an integral part of assay procedure when dealing with a substance the potency of which one could not foretell with any degree of accuracy. It would be of assistance to have advice from the statisticians on how such probe tests could most economically be carried out, and to what extent the data supplied by them could be incorporated with the results obtained from the subsequent four-point or other kind of routine assay.

With that observation on a purely practical point he desired to say how much he had appreciated the invitation to come to that meeting and the value of the paper and the discussion.

The vote of thanks to the author was put to the meeting and carried unanimously.

The following contributions were received in writing:

Dr. O. L. Davies: Mr. Finney's publications on biological assays have been of considerable value to those who are interested in the statistical interpretation of these assays, and the present paper represents another valuable contribution. It illustrates very forcibly the advantages gained in applying statistical methods to biological assay. The advantages lie, not only in the calculation

of activities and their precision, but also in the design of investigations undertaken to determine the conditions of the assay which give the most satisfactory result, and ultimately, in the design of the assay itself. Any method of assay which is likely to be used repeatedly requires at the outset or in the early stages to be examined fully for at least all the main sources of error which arise, and in such investigations there is abundant scope for the application of the statistical principles of "planning of experiments." Attention to these matters may well result in major economies in the long run and in greater precision per unit of effort. The statistician should be called in at the earliest possible stage.

In most biological assays involving a growth factor, the extent of growth varies with the time allowed for growth and also with other conditions of the experiment, e.g. temperature of incubation for microbiological assays. The slope of the response-log dose curves often depends on the time allowed for growth and on temperature of incubation. Other sources of error being constant, the highest precision is given by the conditions which give rise to the highest slope. If the highest precision is given by an inconveniently long time of incubation, then the loss in precision by a shorter incubation time may have to be compensated for by a higher degree of replication. The relation between precision and time of incubation will enable the optimum economic conditions to be derived. Other conditions can be tackled in a similar way.

In chemotherapy we frequently require to compare the activities of several compounds. It is necessary to compare the regression of the various compounds in order to assess whether or not the actions of the compounds are similar, and when the regression of response on log dose is linear (this is usually the case over a sufficiently wide range of doses) the analysis of the responses may be conveniently expressed in the following form:

Source of variation	*Sum of Squares*	*Degrees of Freedom*	*Variance*
Between regression coefficients	.	.	.
About individual regressions	.	.	.
About common regression .	.	.	.
Within doses	.	.	.
Total	.	.	.

From this we can readily assess the significance of the variation between regression coefficients and the variation about the regressions.

This leads to my next point. I find myself in disagreement with the previous statement that a significant variation about a linear regression, when compared with the variance amongst individual responses for a fixed dose, is necessarily an indication of a departure from linearity. This would be the case if the assay had been carried out under conditions of complete randomization. For routine work this is usually impracticable, and any attempt at complete randomization would probably introduce mistakes in dosing. In practice the subjects are usually dosed or treated in groups, and an apparent significant variance about a linear regression may be due to an error arising between the groups. Errors in dosing or preparation of the dose common to the individuals of each dose would also give rise to an apparent variation about the linear regression. The variance within doses would then be an underestimate of the error variance. This might be the case in the analysis given in Table 2. The variance about the regressions, although not significant, is more than twice the variance within doses. Unless the pairs of results for each dose are genuine replicates, i.e. prepared from independent weighings, dilutions, etc., and unless the dosing was carried out in a completely random order, I would prefer to take the variance about the linear regression as my estimate of error for this particular assay. In routine work there would be many such assays, and the question of the significance of the variance about the regression could then be tested more precisely by pooling the information from a number of these assays.

Dr. Eric C. Wood: I am much impressed by the clear manner in which Mr. Finney has demonstrated the essential unity of the theory underlying all biological assays. Considering the widely varying types of assays that are in use for all manner of purposes to-day, and the apparently very different methods of computation used in calculating the results, this is a real achievement. I would not presume to cross swords with Mr. Finney on the purely statistical sections of his paper. The remarks which follow are concerned only with that part of his paper which deals with the design of assays.

I think it should be emphasized—indeed, it cannot be too often proclaimed as a cardinal principle—that assay design is a matter requiring the close co-operation of the statistician and the experimenter. It is impossible for either to decide by himself what is the best experimental design

to use in any given set of circumstances. Theoretical and practical considerations interact to a very material extent; moreover, it is unfortunately true that they are often in conflict, so that the assay design eventually adopted has to be in the nature of a compromise. A good example of this necessity for compromise is encountered when endeavouring to decide how many different dose-levels of the Standard and Test Preparations to use. Mr. Finney has pointed out that an assay design in which only two dose-levels of each preparation are used is inferior to designs in which there are three or more, since in the former case the linearity of the response curves cannot be examined. This, of course, is perfectly true, and wherever it is practicable to do so, the 6-point assay design is definitely preferable to the 4-point design, but there is a very real practical difficulty. More often than not it is desired to assay more than one Test Preparation simultaneously, and in a very large number of assay techniques, particularly assays of vitamins and other growth factors using rats, inter-litter variance is found to be highly significant. It is therefore very desirable to distribute litter-mates evenly between one's dose-groups. If, therefore, two Test Preparations are to be assayed, it is not possible to have three dose-levels of Standard Preparation and of each of the Test Preparations unless one has available several litters of at least nine animals, and in our colony (and I think I may say in those of other people as well) such large litters are not readily forthcoming. Consequently, in order to be able to use litters of six or seven animals, one is driven to adopt an assay design in which there are only two dose-levels of each preparation. This kind of difficulty is greatly intensified in assays such as those of Vitamin A, in which the responses of the two sexes are so different in degree and kind that it is almost essential to work with animals of one sex only.

I also endorse the desirability of so choosing the doses of one's Test Preparation that the mean responses obtained are as nearly as possible equal to those obtained from the corresponding doses of Standard Preparation. Mr. Finney has advanced one reason for this, namely, that the precision of the assay is increased, but there is also the important point that if the responses to the doses of Test Preparation are materially removed from the responses to the Standard Preparation, one or more of the Test Preparation doses may be outside the range of linearity of the assay technique. In an assay design in which there are three or more doses of each preparation, it may be possible to detect from the internal evidence of the assay itself that this has occurred. If, however, there are only two doses of each preparation, it may not be possible to distinguish between invalidity caused by a real qualitative difference between the Standard Preparation and the Test Preparation in their action on the Test animals, and apparent invalidity caused by one of the Test responses falling outside the range of linearity. It is, of course, not always possible with unknown materials to guess sufficiently shrewdly the correct doses to use, but it should certainly be taken as a working principle that the result of any assay in which the Test responses are far removed from the Standard responses should be regarded as suspicious, and it is highly desirable in such an event to carry out a second assay in which the Test doses are adjusted in the light of the experience gained in the first assay so as to be more nearly equivalent to the Standard doses. I must admit, however, that this is a counsel of perfection. A single biological assay may well involve the use of 40-60 rats for several weeks; and unless one has a gigantic colony at one's disposal, one is often forced to omit repeat assays and other investigations which are clearly desirable because one simply has not got enough animals available for the purpose. This is one of the strongest arguments for employing statistical methods to ensure that one uses in one's assays just enough animals to obtain the required degree of precision, *and no more*; it is in this direction that the application of statistics can return a very real dividend in hard cash.

It is greatly to be wished that the official and semi-official bodies which are responsible for producing standard procedures of biological assay would pay at least as much attention to the method of computation of the result of the assay as they do to the description of the practical details of the assay itself. It is very surprising, and indeed deplorable, that so many published assay procedures are seriously at fault in this respect. I will not comment on the assay methods of the British Pharmacopoeia (1932) because after all this is fifteen years old, and I understand that the new Pharmacopoeia which is soon to be published will leave little to complain of from the statistical aspect. The United States Pharmacopoeia, however, which is much more recent, uses assay designs for Vitamins A and D in which there is but one dose of Standard Preparation and one dose of Test Preparation—a 2-point assay, in fact—and the assay is regarded purely as a limit test, no attempt being made to compute the actual potency of the preparation. Even more recent is the 1946 edition of the *Official Methods of Analysis of the American Association of Official Agricultural Chemists*. It is most disappointing to find that the method given for the biological assay of Vitamin B_1 by the rat-growth method contains many careful details designed to ensure the physical similarity of the groups on the Standard Preparation and the groups on the Test Preparation, but that when it comes to the examination of the results, it is stated only that if the average gain in weight in a Test Group is equal to or greater than that in a Standard Group,

then the Vitamin B_1 content of the total Test material fed is equal to or greater than that of the total Standard material fed. It seems extraordinary that this is the best that can be done in the 1946 edition of a book published under the auspices of an Association which could command the services of the best statistical brains of the United States.

Mr. FINNEY, in reply, said that he was grateful for the kind remarks made by Mr. Fieller, Dr. Irwin and other speakers. He felt honoured by the presence of some whose names are illustrious in the history of biological assay, especially Sir Percival Hartley and Dr. Trevan, though he feared that, from their point of view, his paper must have seemed a riot of mathematics, with little relevance to assay practice. He proposed to answer some of the comments more fully in print, but a few points he would deal with immediately.

As a mathematician, he shared the views of Mr. Fieller and Dr. Irwin that the addition of 5 to normal and other equivalent deviates was unnecessary. On the other hand, the probit was now so firmly established that an attempt to revert to the normal equivalent deviate seemed scarcely worth while, and, however little advantage the addition of 5 might give, it seemed to him desirable to define logits or other similar transformations according to the same convention. He would not venture to join issue with Dr. Irwin on nomenclature, and would plead that he himself had not invented the words referred to.

With reference to Mr. Fieller's comment on equation (27), he hoped that the misprints would be removed before publication. The form of the equation seemed to him a matter of personal preference ; he had found his version both convenient to compute, and indicative of the extent to which an approximation based on equation (24) could safely be used.

He had been interested to see Dr. Irwin's discussion of the four-point assay, but, like Mr. Fieller, he felt that the property of giving the same result for a parabolic as for a linear regression was a "mathematical fluke." He considered that very great care should be exercised in the interpretation of results from assays of this design.

Mr. Gridgeman had enquired about the use of unequal numbers of subjects at different dose levels. The effect of this on the precision of the assays could be investigated in the manner of Section 7 and the Appendix, but he doubted whether this would lead to improvements in design that could not be predicted from the Tables already given. Full tabulation would be very laborious, but he would be glad to examine any more definite suggestions that Mr. Gridgeman might put forward.

Though he did not claim to be a geneticist, he had always denied the existence of the mathematical gene, to which Dr. Trevan had referred. He thought that with a little patience almost anyone could become a competent mathematician, whereas biochemistry or pharmacology required something akin to intelligence. Nevertheless, in writing for the biologist, a statistician encountered the difficulty of guessing how much familiarity with mathematical and statistical technique he could assume in his readers. The present paper endeavoured to survey a wide field; in order to keep it within reasonable length he had written primarily for statisticians, and had not given details of practical applications of illustrative examples as fully as he would have liked. He would like to assure Dr. Trevan, however, that he shared his belief that the statistician should not be content to give expositions of statistical theory, but should also endeavour, on suitable occasions, to make the essentials of his theory intelligible to the users of statistical technique.

He hoped for future discussion with Mr. Bacharach on "probe-tests" and the time factor in assays. He had not met that particular type of problem before, but obviously it was important to the most economical use of material. Possibly recent developments in sequential sampling techniques might be adapted for use here.

In preparing the paper, his outlook had been influenced by the wide range of problems in which the statistical methods developed for assay purposes had been applicable, even though these were not biological assays in the strict sense. He recalled that recently, on one day, four requests for advice, from different Departments of the University of Oxford and on problems otherwise unrelated, had been best answered by reference to quantal responses and the probit method.

Mr. FINNEY later added the following remarks in writing:

I fully agree with Dr. Irwin's comment on the behaviour of χ^2 for small numbers. I fear that I expressed myself rather loosely in my paper, but I have discussed the point more fully elsewhere (Finney, 1947). The statistic calculated from the data has the same expectation as a true χ^2, but its sampling distribution has a greater variance; it seems to me, however, that only the larger values need careful watching, since the others are unlikely to lead to faulty conclusions.

The use of a two-variate regression equation for slope-ratio assays, which Dr. Irwin rightly admires, was first proposed by my friend Dr. E. C. Wood. I have suggested to him that additional

information relevant to the validity of an assay might be obtained from tests of mixtures of doses of the two preparations, which should give responses corresponding to other parts of the regression "plane," but I have not yet found a biochemist with time to spare for making trial of this.

I cannot agree entirely with Mr. Fieller's claim that equalizing the variance is more important than rectification, though, as I have said in a footnote on p. 51, I do not think our points of view completely opposed. Dr. Irwin pointed out (as I had of course realized) that the analysis for quantitative responses can be modified so as to take account of unequal variances, but his proposal breaks down unless the relative magnitudes of these variances are known. When σ^2 is clearly dependent upon U, I would prefer to follow Mr. Fieller in seeking first a transformation of response which will give a constant variance, but I should want to follow this with a further transformation according to the rules of my paper, unless linearity were obtained, by chance, at the same time as the constant variance. Non-linearity may easily result in biased estimation of potency, whereas neglect of a variation of σ^2 with U is unlikely to have consequences worse than wrong assessment of precision—a less serious fault.

Perhaps one reason why Mr. Fieller and I differ in outlook is that he has had considerable experience of routine commercial assays, whereas my interests have been more restricted to research problems. I think that both points of view are important, though they may lead to different conclusions on such questions as the best form for computations and the economics of the division of labour between experimental and statistical work. For this reason, I have not attempted to discuss recent American work on the use of control charts in routine assays, and on condensed computing techniques for assays of standard design. In spite of what Mr. Fieller has said, I believe that graphical methods can be very useful. I would always encourage the research worker to draw his dose-response diagram. The experienced worker can often draw lines by eye that will give him an estimate very close to that he would obtain by calculation, and which, as he can see by inspection, lies within his limits of tolerance for precision. For many purposes an exact statement of fiducial limits is not needed, providing that there is clear evidence that they are not wider than a specified amount. Furthermore, the diagram may draw attention to systematic departures from linearity that could pass unnoticed in a purely mechanical calculation. I should not like my remarks on graphical methods to be interpreted as excuse for lack of care and rigour in statistical analysis, but I do suggest that graphical techniques may properly be employed when they not only save time, but also can be seen to give results indistinguishable from those of a full analysis. In any case of doubt, I would insist on the arithmetical procedure, and I have no wish to discourage any analyst who is prepared *always* to undertake the arithmetic.

Dr. Emmens's question about the use of litter-mates in assays based on quantal responses is very interesting. I have never encountered an assay in which this control of variation was employed, but I agree that it ought to improve the precision of estimation. One method of performing the calculations would be to introduce concomitant variates $c_1, c_2, c_3 \ . \ . \ .$ such that $c_i = 1$ for all members of the i^{th} litter and zero for all other subjects. A multiple co-variance analysis on the c_i, followed by adjustments based on the regression equation, would then remove the component of variation attributable to litter differences. This procedure, however elegant mathematically, would be arithmetically very laborious, and the problem merits closer examination from the practical point of view.

I am glad that Dr. Davies has drawn attention to the danger that lack of randomization in the testing of different dose-levels may be responsible for an apparently significant deviation from linearity. Assays that I have examined since the presentation of my paper suggest that appreciable underestimation of error variance may frequently arise from this source. Dr. Davies's recommendation to use deviations from the linear regression to give an estimate of error variance should overcome the difficulty, providing that sufficient degrees of freedom are available to make this estimate worth having. I should like to see experimental investigation of this point made in a number of laboratories, as the personal factor may be important and theory cannot predict the results of incomplete randomization.

Dr. Wood has mentioned the difficulty of using litter-mate control of variation when several preparations are to be assayed simultaneously, so that the number of doses of all preparations exceeds the size of litter normally available. I deliberately avoided discussion of simultaneous assays, so as not to complicate my argument still further; they do not, I think, introduce new points of principle, though they may require modifications of design for optimal efficiency. Dr. Wood's problem may be solved by the adoption of the incomplete block designs which have been found suitable for dealing with analogous difficulties in field experimentation. The balanced, and perhaps also the partially balanced, incomplete block-schemes, in which restrictions of symmetry are placed on the number of litters providing comparisons of any pair of doses, seem particularly appropriate, and doubtless special variants of the symmetry conditions would ensure maximum precision for the most important contrasts.

THE RANDOM DIVISION OF AN INTERVAL

By P. A. P. MORAN

IN a recent address to the Royal Statistical Society[1] Professor M. Greenwood has proposed the following problem, which is of interest in providing a test of significance in the statistical study of infectious diseases. Let n points be distributed at random in the unit interval (0, 1), thus forming $n+1$ intervals $I_1, \ldots I_{n+1}$. What is the statistical distribution of—

$$S = \sum_{i=1}^{n+1} |I_i|^2 ?$$

In the present paper we discuss this problem.

The theory of various distributions connected with the random division of an interval has been discussed by many writers (Clifford,[2] v. Bortkiewicz,[3] Morant,[4] Fisher,[5,6] Garwood,[7] and others) and has many applications to statistical problems. Both Clifford and Fisher remark that this problem is equivalent to considering $n+1$ quantities $x_i = |I_i|$ which are non-negative and satisfy—

$$\sum_{1}^{n+1} x_i = 1,$$

and whose probability distribution is such that equal areas on the positive part of the n-dimensional plane defined by this equation correspond to equal probabilities for the corresponding divisions of the interval.

The part of this plane common to all the half-spaces $x_i > 0$ is an n-dimensional regular simplex. S lies between unity and the square of the perpendicular from the origin on to the plane $\sum_{1}^{n+1} x_i = 1$. The perpendicular is of length $(n+1)^{-\frac{1}{2}}$. When S is greater than this the $(n+1)$-sphere defined by $S =$ constant, intersects $\sum_{i=1}^{n+1} x_i = 1$ in an n-sphere of radius r where $r^2 = S - (n+1)^{-1}$. The problem of finding the distribution of S therefore reduces to that of finding the volume common to a regular n-simplex and a sphere of varying radius whose centre is the circumcentre of the simplex.

This representation of the problem gives us a certain amount of information about the nature of the distribution. The simplex is bounded by $n+1$ $(n-1)$-dimensional planes (the faces), $\frac{1}{2}(n+1)n$ "edges" of dimension $n-2$, $\frac{1}{6}n(n+1)(n-1)$ "edges" of dimension $n-3$, and so on down to $n+1$ zero-dimensional vertices. When r is small the n-sphere lies entirely within the simplex. As r becomes larger than the radius of the in-sphere, the sphere becomes intersected by the $(n-1)$-planes and the analytic form of the distribution changes. It changes again when the sphere becomes large enough to intersect the $(n-2)$-dimensional "edges," and so on. Thus the distribution is analytic in n stretches, and at the ends of these the higher derivatives are discontinuous.

We may also consider the distribution in the following way: Let $g_1(r)$ be the volume of the sphere cut off by one of the $(n-1)$-planes in which the faces of the simplex lie, $g_2(r)$ the volume cut off by two of these planes, and so on. $g_i(r)$ will be zero until r is large enough to intersect the element of the boundary of dimension $n-i$. Then the volume of the region common to the sphere and the simplex will be—

$$\tfrac{4}{3}\pi r^3 + \Sigma \binom{n+1}{i}(-)^i g_i(r),$$

the sum extending over all non-zero $g_i(r)$'s. When $g_i(r)$ is greater than zero it is an increasing analytic function and not all of its derivatives are zero at the point where it is zero itself.

It might be possible to calculate the $g_i(r)$ exactly by using n-dimensional hyperspherical trigonometry but this would be difficult. However, it is possible to express the distribution in the n-dimensional case in terms of the result for $(n-1)$ dimensions. To see this write $v_n(r)$ for the volume common to an n-sphere of radius r and an n-simplex of unit side and the same centre. Then writing $\Phi_n(x)$ for the probability that $S \leqslant x$, we obtain by a little calculation that—

$$\Phi_n(x) = 2^{-\frac{1}{2}n}\, n\,!\,(n+1)^{-\frac{1}{2}}\, v_n\left[\frac{\{x-(n+1)^{-1}\}^{\frac{1}{2}}}{\sqrt{2}}\right].$$

With each of the $n+1$ faces of the simplex the centre forms an n-dimensional pyramid. Let ρ_n be the radius of the in-sphere of the simplex. It is easy to see that—

$$\rho_n^2 = \{2n(n+1)\}^{-1}.$$

Consider the part of the sphere of radius r which lies inside one of these pyramids, and take an $(n-1)$-plane parallel to the base of the pyramid and distant x from the centre. The $(n-1)$-dimensional measure of the part of this plane lying inside both the pyramid and the sphere is—

$$(x\rho_n^{-1})^{n-1}\, v_{n-1}\left(\rho_n x^{-1}\sqrt{(r^2-x^2)}\right)$$

where we write $v_{n-1}(r) = 0$ when r is imaginary and $v_{n-1}(r) = 1$ when r is greater than $[2n(n-1)^{-1}]^{-\frac{1}{2}}$, the circumradius of an $(n-1)$-dimensional simplex of unit side. It follows that, when r is less than $[2n^{-1}(n+1)]^{-\frac{1}{2}}$, the radius of the circumsphere—

$$v_n(r) = (n+1)\int_0^{\rho_n}(x\rho_n^{-1})^{n-1}v_{n-1}\left(\rho_n x^{-1}\sqrt{(r^2-x^2)}\right)dx$$

$$= (n+1)r^n\int_{\sqrt{(r^2-\rho_n^2)}}^{\infty}(\rho_n^2+t^2)^{-\frac{n+2}{2}}\rho_n\, t\, v_{n-1}(t)dt$$

and $v_n(r) = 1$ when r is greater than this circumradius.

Using the above formula it would be possible to evaluate the distribution for the smaller values of r, starting from the solution for $n = 2$, which is easy to calculate by elementary geometry (given by Greenwood[1]). However, there is a loss of accuracy at each stage, and the process would probably not be suitable in cases where n is greater than 10. We also remark that for any n the distribution is easy to calculate when r is less than the radius of the in-sphere of the simplex. This was done by Isserlis (Greenwood[1]), and had been done previously by Clifford.[2]

The Moments of the Distribution

In what follows it is convenient to cast the problem into a slightly different form. Let $x_1, \ldots x_{n+1}$ be quantities independently distributed on $(0,\infty)$ with the probability function $\beta\, e^{-\beta x}$. Then the joint distribution of the quantities $x_i(x_1 + \ldots + x_{n+1})^{-1}$ where $i = 1, \ldots n+1$ is equivalent to the joint distribution of the lengths $|I_i|$. This was pointed out by Fisher[5] and had been previously proved by Clifford. Fisher was concerned with the distribution of the largest of the intervals, whereas we wish to discuss the distribution of the sum of squares, that is, the distribution of—

$$(x_1^2 + \ldots + x_{n+1}^2)(x_1 + \ldots + x_{n+1})^{-2}.$$

We write A for the region common to all the half-spaces $x_i \geqslant 0$, $B(T)$ for the part of this region cut off by the "plane" $x_1 + \ldots + x_{n+1} = T$, and B for $B(1)$. The moment μ_r' about the origin is then given by—

$$\mu_r' = \beta^{n+1}\int (x_1^2 + \ldots + x_{n+1}^2)^r(x_1 + \ldots + x_{n+1})^{-2r}e^{-\beta(x_1 + \ldots + x_{n+1})}dx_1 \ldots dx_{n+1}$$

$$= \beta^{n+1}\lim_{T\to\infty}\int_{B(T)}(x_1^2 + \ldots x_{n+1}^2)^r(x_1 + \ldots x_{n+1})^{-2r}e^{-\beta(x_1 + \ldots x_{n+1})}dx_1 \ldots dx_n +$$

and writing—

$$x_i = Ty_i \qquad (i = 1, \ldots n + 1)$$

$$\mu_r' = \beta^{n+1} \lim_{T \to \infty} \int_{B(T)} (y_1^2 + \ldots y_{n+1}^2)^r (y_1 + \ldots y_{n+1})^{-2r} T^{n+1} e^{-\beta T(y_1 + \ldots y_{n+1})} \, dy_1 \ldots dy_{n+1}.$$

Using Dirichlet's integral (Whittaker and Watson,[9] p. 258) we get—

$$\mu_r' = \beta^{n+1} \lim_{T \to \infty} \Sigma \frac{r!}{\alpha_1! \ldots \alpha_{n+1}!} \cdot \frac{(2\alpha_1)! \ldots (2\alpha_{n+1})!}{(n + 2r)!} \int_0^1 T^{n+1} \tau^n e^{-\beta T \tau} d\tau$$

where the sum is taken over all partitions of r into non-negative integers α_i. Then we have—

$$\mu_r' = \lim_{T \to \infty} \Sigma \frac{r!(2\alpha_1)! \ldots (2\alpha_{n+1})!}{\alpha_1! \ldots \alpha_{n+1}!(n + 2r)!} \beta^{n+1} \int_0^1 T^{n+1} \tau^n e^{-\beta T \tau} d\tau$$

$$= \lim_{T \to \infty} \Sigma \frac{r!(2\alpha_1)! \ldots (2\alpha_{n+1})!}{\alpha_1! \ldots \alpha_{n+1}!(n + 2r)!} \int_0^{\beta T} t^n e^{-t} dt$$

$$= \frac{n!r!}{(n + 2r)!} \Sigma \frac{(2\alpha_1)! \ldots (2\alpha_{n+1})}{\alpha_1! \ldots \alpha_{n+1}!}$$

For $n \geq 3$ we obtain—

$$\mu_1' = 2(n + 2)^{-1}$$

$$\mu_2' = 2^2(n + 6)\{(n + 2)(n + 3)(n + 4)\}^{-1}$$

$$\mu_3' = 2^3(n^2 + 17n + 90)\{(n + 2)(n + 3)(n + 4)(n + 5)(n + 6)\}^{-1}$$

$$\mu_4' = 2^4(n^3 + 33n^2 + 434n + 2520)\{(n + 2)(n + 3)(n + 4)(n + 5)(n + 6)(n + 7)(n + 8)\}^{-1}$$

and reducing to the mean as origin—

$$\mu_2 = 2^2 n(n + 2)^{-2}\{(n + 3)(n + 4)\}^{-1}$$

$$\mu_3 = 2^3(10n^2 - 4)(n + 2)^{-3}\{(n + 3)(n + 4)(n + 5)(n + 6)\}^{-1}$$

$$\mu_4 = 2^4(3n^4 + 303n^3 + 42n^2 - 24n)(n + 2)^{-4}\{(n + 3) \ldots (n + 8)\}^{-1}$$

A Non-linear Central Limit Theorem.

We now prove that as n becomes large, the above distribution can be expressed asymptotically in terms of the normal distribution, as we would naturally expect from the asymptotic behaviour of the above moments. To do this we use the following theorem:

Theorem: Let the quantities x_i $(i = 1, \ldots)$ be independently distributed in a distribution law which has its first four moments finite and $\mu_1' > 0$. Write—

$$S_1^n = \sum_1^n x_i, \quad S_2^n = \sum_1^n x_i^2.$$

Write σ_1^2 for the variance $\mu_2' - \mu_1'^2$ of x_i, and σ_2^2, say, for the variance $\mu_4' - \mu_2'^2$ of x_i^2 which will have a mean $m = \mu_2'$. Then the quantity—

$$t_i = m^{-1}x_i^2 - 2x_i(\mu_1')^{-1} + 1$$

has zero mean and a variance—

$$v = \mu_4' m^{-2} - 4\mu_3'(m\mu_1')^{-1} + 2\mu_2'(2\mu_1'^{-2} + m^{-1}) - 3,$$

and the distribution of—

$$\phi = n^{\frac{1}{2}} v^{-1} \left\{ \frac{n\mu_1'^2 S_2^n}{m(S_1^n)^2} - 1 \right\}$$

tends, as n increases, to a normal distribution with zero mean and unit variance.

Proof :

$$S_2^n(S_1^n)^{-2} = \left\{ nm + n^{\frac{1}{2}}\sigma_2 \frac{S_2^n - nm}{n^{\frac{1}{2}}\sigma_2} \right\} \left\{ n\mu_1' + n^{\frac{1}{2}}\sigma_1 \frac{S_1^n - n\mu_1'}{n^{\frac{1}{2}}\sigma_1} \right\}^{-2}$$

$$= mn^{-1}\mu_1'^{-2} \left\{ 1 + \frac{\sigma_2(S_2^n - nm)}{nm\sigma_2} \right\} \left\{ 1 + \frac{\sigma_1(S_1^n - n\mu_1')}{n\sigma_1\mu_1'} \right\}^{-2}.$$

Now if $y \neq -1$, $(1 + y)^{-2} = 1 - 2y + \theta y^2$

where—
$$|\theta| = |(2y + 3)/(1 + y)^2| < 8,$$

say, whenever $|y| < 4^{-1}$. Not assuming anything as yet about the denominator in equation (1) except that it be non-zero, we have—

$$\frac{n\mu_1'^2}{m} \cdot \frac{S_2^n}{(S_1^n)^2} = \left\{ 1 + \frac{\sigma_2(S_2^n - nm)}{nm\sigma_2} \right\} \left\{ 1 - \frac{2\sigma_1(S_1^n - n\mu_1')}{n\sigma_1\mu_1'} + \theta \frac{\sigma_1^2(S_1^n - n\mu_1')^2}{n^2\sigma_1^2\mu_1'^2} \right\}$$

Then—

$$\frac{n\mu_1'^2 S_2^n}{m(S_1^n)^2} - 1 = \left\{ \frac{S_2^n - nm}{nm} - \frac{2(S_1^n - n\mu_1')}{n\mu_1'} \right\} - 2\left\{ \frac{(S_2^n - nm)(S_1^n - n\mu_1')}{n^2 m\mu_1'} \right\}$$
$$+ \theta \left\{ \frac{(S_1^n - n\mu_1')^2 S_2^n}{n^3 m\mu_1'^2} \right\}$$

$$= \frac{v}{n^{\frac{1}{2}}} \left\{ \frac{\sum_1^n t_i}{vn} \right\} - 2\left\{ \frac{(S_2^n - nm)(S_1^n - n\mu_1')}{n^2 m\mu_1'} \right\} + \theta \left\{ \frac{(S_1^n - n\mu_1')^2 S_2^n}{n^3 m\mu_1'^2} \right\}$$

and so—

$$\phi = \frac{n^{\frac{1}{2}}}{v} \left[\frac{n\mu_1' S_2^n}{m(S_1^n)^2} - 1 \right] = (v\sqrt{n})^{-1} \sum_1^n t_i - 2A + \theta B,$$

where—

$$A = (S_2^n - nm)(S_1^n - n\mu_1')(n^{\frac{3}{2}} vm\mu_1')^{-1}$$

$$B = (S_1^n - n\mu_1')^2 S_2^n (n^{\frac{5}{2}} vm\mu_1'^2)^{-1}.$$

Now, given any number a, we show that the probability that $\phi \leqslant a$ converges uniformly in a, as n tends to infinity, to—

$$(2\pi)^{-\frac{1}{2}} \int_{-\infty}^{a} e^{-\frac{1}{2}t^2} dt$$

Let ε, $\eta < \frac{1}{4}\varepsilon$ be given positive constants. By using Tchebycheff's inequality we can choose K so large that the probabilities that—

$$|S_2^n - nm| > K\sigma_2 n^{\frac{1}{2}}$$

$$|S_1^n - n\mu_1{}'| > K\sigma_1 n^{\frac{1}{2}},$$

are each less than $\frac{1}{4}\varepsilon$, independently of n. Now choose N so large that—

$$2K^2\sigma_1\sigma_2 < \tfrac{1}{2}\,\eta\, n^{\frac{1}{2}} \nu m \mu_1{}'$$

$$8\left\{K^3\sigma_1{}^2\sigma_2(nm)^{-1} + K^2\sigma_1{}^2 n^{-\frac{1}{2}}\right\} < \tfrac{1}{2}\eta\nu\mu_1{}'^2$$

for all $n \geqslant N$, and so that the probability $p(t)$ that $n^{-\frac{1}{2}} \sum\limits_{1}^{n} t_i < t$ satisfies—

$$\left| p(t) - (2\pi)^{-\frac{1}{2}} \int_{-\infty}^{t} e^{-\frac{1}{2}x^2} dx \right| < \tfrac{1}{4}\varepsilon,$$

for all t and all $n \geqslant N$. We can do this by applying the ordinary central limit theorem to $n^{-\frac{1}{2}} \sum\limits_{1}^{n} t_i$, because the t_i are distributed in the same distribution independently and this distribution has all its moments finite.

Now write $P(a)$ for the probability that $\phi \leqslant a$.

We then have—

$$P(a) < p(a + \eta) + \tfrac{1}{2}\varepsilon$$

and—

$$P(a) > p(a - \eta) - \tfrac{1}{2}\varepsilon$$

Then—

$$P(a) < \tfrac{1}{2}\varepsilon + \tfrac{1}{4}\varepsilon + (2\pi)^{-\frac{1}{2}} \int_{-\infty}^{a+\eta} e^{-\frac{1}{2}t^2} dt$$

$$< \varepsilon + (2\pi)^{-\frac{1}{2}} \int_{-\infty}^{a} e^{-\frac{1}{2}t^2} dt$$

Similarly—

$$P(a) > (2\pi)^{-\frac{1}{2}} \int_{-\infty}^{a-\eta} e^{-\frac{1}{2}t^2} dt - \tfrac{1}{4}\varepsilon - \tfrac{1}{2}\varepsilon$$

$$> (2\pi)^{-\frac{1}{2}} \int_{-\infty}^{a} e^{-\frac{1}{2}t^2} dt - \varepsilon$$

and thus—

$$\left| P(a) - (2\pi)^{-\frac{1}{2}} \int_{-\infty}^{a} e^{-\frac{1}{2}t^2} dt \right| < \varepsilon$$

for all $n \geqslant N$.

This proves the theorem which we can now apply to our problem. For consider quantities $x_i (i = 1, 2, \ldots)$ each independently distributed with the probability density function $\beta e^{-\beta x_i}$. This distribution has all its moments finite and we apply the theorem to—

$$S_2^n(S_1^n)^{-2} = (x_1^2 + \ldots + x_n^2)(x_1 + \ldots + x_n)^{-2}$$

It follows that—

$$\phi = \frac{n^{\frac{1}{2}}}{\nu}\left\{\frac{n\mu_1'^2 S_2^n}{m(S_1^n)^2} - 1\right\}$$

$$= n^{\frac{1}{2}}\left\{\frac{nS_2^n}{2(S_1^n)^2} - 1\right\}$$

tends, as n increases, to be distributed normally with unit variance about zero mean, i.e. that $S_2^n(S_1^n)^{-2}$ tends to be distributed normally about mean $2n^{-1}$ with variance $4n^{-3}$, quantities which are asymptotically equal to the exact values calculated above:

Although the distribution does tend to normality ultimately, this happens rather slowly, as is shown by the following values of β_1 and β_2:

n	β_1	β_2
5	1·59	6·83
10	1·71	8·35
50	1·22	6·39
100	·93	5.03
1000	·31	3·24

The method of proof above enables us to prove more general results about the products and quotients of symmetric functions $S_r^n (r = 1, 2, \ldots)$ of quantities x_i which are all independently distributed in the same distribution provided either that, if $\mu_1'(x_i) = 0$, only symmetric functions of even order occur in the denominator. Moreover the moments of the distribution of the x_i must be finite up to an order that will make the method of the proof work. By using the full force of the central limit theorem itself we can naturally relax the above conditions somewhat. We also remark that v. Mises,[9,10] has considered generalizations of the central limit theorem to non-linear functions of n independent quantities, but his theorems seem to be of a different nature from the above.

Supposing for the sake of simplicity that all the moments of the distribution of the x_i are finite, we can then deduce the following results. The sampling distribution of the statistics b_1 and b_2, the "studentized" moment $m_r(m_2)^{-\frac{1}{2}r}$, the cumulants, and the coefficient of variation, calculated for a sample of n, all tend to normality when n tends to infinity. These results are well known but proofs seem lacking in the literature. A very crude upper bound for the divergence of the distribution of these quantities for finite n from the normal can be found by applying Liapounoff's theorem (Cramer,[11] p. 77) and Tchebycheff's inequality in the above proof.

The normal function also occurs in other problems of mensuration in a large number of dimensions. Borel[12] (see also Castelnuovo[13]) has shown how to calculate approximately the volume of the part of an n-dimensional cube cut off by a plane distant d from the centre of the cube and not parallel to any of the faces. This, of course, is equivalent to applying the central limit theorem to a weighted mean of a sample of n from a rectangular population. The result

has applications to the asymptotic theory of the number of representations of a number as a linear form.

Similarly we may consider the volume common to a unit cube and a sphere in n dimensions. Write this $C(r)$ where r is the radius of the sphere. Then when n is large we can represent $C(r)$ approximately in terms of the normal distribution function by using the central limit theorem on the sum of squares of the quantities x_i, which are considered to be each distributed in a rectangular distribution. It follows that for large N, we can represent, after a little reduction, the numbers of partitions of numbers less than or equal to N into the sum of n squares each less than or equal to a number M, say, in terms of $C(r)$, where n is kept fixed and N and M increase. $C(r)$ can itself be calculated with a relative error which decreases as n increases.

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The Significance of Associations in a Square Point Lattice

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H. Todd (1940) has proposed an ingenious series of tests for examining the randomness of a set of points in a square lattice, to be used, for example, in determining whether deaths amongst trees planted at the corners of such a lattice occur independently or in "clumps." He considered a lattice of mn points in n rows of m, and found the probability that a random pair of them should be contiguous (including diagonal contiguity). In order to test the randomness of an observed set of μ points, he compared the expected and observed numbers of these doublets (using each point several times if it can form part of several doublets); the expectation is $\frac{1}{2}\mu(\mu-1)$ multiplied by the probability, P, for a single doublet, and Todd suggested that the sampling distribution on the null hypothesis of a random selection of points could be well approximated by a binomial based on P or, in the common situation that P was small, by the corresponding Poisson distribution.

Todd then extended his results to triplets and quadruplets, sets of three and four contiguous points, again comparing the total numbers of these with their expectations; he stated that "since the probabilities in the case of triplets are smaller than in the case of doublets, the distribution will be of the Poisson type to a high degree of approximation."

The values for the expected numbers of doublets, triplets, and quadruplets are correctly derived; the suggested tests of significance, however, seem liable to attach too low a probability to large deviations from expectation, through underestimation of the variance of the true sampling distribution. Except when μ is very small by comparison with the total number of lattice points, the discrepancy is likely to be more important, rather than less, for triplets and still more so for quadruplets. A test based on a binomial or Poisson distribution assumes that the separate occurrences of these contiguous sets are independent, whereas, even when the μ points are chosen entirely at random, the sets are not independent of one another, since a single "clump" may contribute a large number. For example, if the μ points happen to contain a clump of nine, arranged in a 3×3 square, these alone contribute 20 doublets, 48 triplets, and 85 quadruplets. Consequently, though Todd's values for the expectations of the numbers of doublets, triplets, and quadruplets are correct, the expectations of the squares of these numbers may be much greater than for the binomial distributions.

As an illustration, Todd's example of 3 points in a lattice of 3×3 ($m=n=3$, $\mu=3$) may be examined. Direct enumeration of the 84 possible selections of 3 points shows the frequencies of 0, 1, 2, and 3 doublets to be 8, 28, 32, 16 respectively. Hence the expected number of doublets is

$$E(d)=(28+64+48)/84$$
$$=\frac{5}{3}$$

which agrees with the result calculated from equation (1) below.

Also
$$E(d^2)=(28+128+144)/84$$
$$=\frac{25}{7},$$

and therefore
$$E[d-E(d)]^2=\frac{50}{63};$$

the binomial variance is $\frac{20}{27}$, so that the true variance exceeds the binomial by a factor of $\frac{15}{14}$. In general the difference between the two variances for doublets seems likely to be small, at least when μ is not very large by comparison with mn; when μ is increased beyond $\frac{1}{2}mn$ there must be a serious discrepancy, since the true variance will then decrease while the binomial variance continues to increase. But for triplets and quadruplets the difference may be of considerable practical importance, even for comparatively small values of μ, as will be shown below.

Exact evaluation of the variances would be a very laborious process, involving enumeration of all possible types of configuration of μ or less contiguous points. In an attempt to form some idea of the importance of the departure from the binomial distribution, a sampling experiment was undertaken. A lattice of 100 points ($m=n=10$) was chosen as a convenient size, the points were numbered from 1 to 100, and twenty of them were chosen with the aid of a table of random numbers (Kendall and Babington Smith, 1939). These were numbered in order of their being obtained, and the doublets, triplets, and quadruplets were then counted for the first 2, 3, 4, . . . 19, 20 points. The procedure was repeated with new sets of twenty random points, until in all 100 sets had been examined. For example, one set was:

8						17		19	
16	6				3				
				10	5		12		
			14						
		15		2					
	7					4	20		
				1					
					13	9			
							18		11

Here there are no doublets in the first four points, but the addition of 5 gives a doublet with 3; 6 and 7 give no new doublets, 8 makes a doublet with 6, 9 adds no more, 10 adds two doublets (with 3 or 5), and so on until 20 adds the thirteenth doublet. A similar count of triplets gives none until 10 is included to make a triplet with 3 and 5; a second is added by 13, and 14 gives two triplets (with 3 and 10 or 5 and 10), and so on. A third count gives the quadruplets. Thus a sample of 100 was obtained with $\mu = 2, 3, 4, \ldots 19, 20$ and for doublets, triplets, and quadruplets; the counts for successive values of μ are not, of course, independent, but they nevertheless give valid estimates of the means and variances for each μ.

Doublets.—Todd's formula for the probability that a random pair of points form a doublet is

$$P = \frac{2[4mn - 3(m+n) + 2]}{mn(mn-1)} \qquad \ldots \ldots \qquad (1)$$

$$= 0{\cdot}069091 \quad \text{for } m = n = 10.$$

The expectation of d, the number of doublets in a random selection of μ points, is

$$E(d) = {}^{\mu}C_2 P,$$

and, if the distribution of d were the same as a binomial distribution of ${}^{\mu}C_2$ events with P as the probability of "success," the variance of d would be ${}^{\mu}C_2P(1-P)$. In Table I, these quantities are compared with the estimates obtained from the sample,

$$\bar{d} = \frac{S(d)}{100},$$

$$s_d^2 = \frac{S(d-\bar{d})^2}{99}.$$

The standard error of $\bar{d}$ is $\pm\, s_d/10$; in general, the agreement between $\bar{d}$ and $E(d)$ may be judged satisfactory, though there is a tendency for $\bar{d}$ to be above expectation for the larger values of μ.

For values of μ greater than 7, the variance is less than that for the corresponding binomial, though scarcely sufficiently so to be considered significantly low. This last result was unexpected, and no reason can be suggested for the true variance of the distribution of d being below the binomial variance; the most reasonable explanation seems to be that up to $\mu = 20$ the distribution of d does not differ greatly from a binomial, and that the low variances in the experimental sampling are due to chance.

TABLE I.—*Sampling Results for Doublets*

μ	$E(d)$	d	${}^{\mu}C_2P(1-P)$	s_d^2
2	·069	·09	·064	·083
3	·207	·28	·193	·305
4	·415	·46	·386	·493
5	·691	·78	·643	·880
6	1·04	1·19	·965	·28
7	1·45	1·52	1·351	·40
8	1·93	2·02	1·801	·64
9	2·49	2·60	2·315	·70
10	3·11	3·30	2·894	·81
11	3·80	3·91	3·54	2·16
12	4·56	4·67	4·24	2·51
13	5·39	5·60	5·02	3·82
14	6·29	6·57	5·85	4·33
15	7·25	7·62	6·75	4·90
16	8·29	8·75	7·72	6·73
17	9·40	9·94	8·75	7·71
18	10·57	11·03	9·84	8·53
19	11·81	12·38	11·00	8·92
20	13·13	13·86	12·22	10·57

Triplets.—For triplets, Todd's formula is

$$P = \frac{6[20mn - 28(m+n) + 36]}{mn(mn-1)(mn-2)}$$
$$= 0{\cdot}009128 \quad \text{for } m = n = 10.$$

The expectation of t, the number of triplets, is

$$E(t) = {}^{\mu}C_3P$$

and the binomial variance is ${}^{\mu}C_3P\,(1-P)$. The sample estimates are compared with these in Table II. Though $\bar{t}$ also agrees well with its expectation, the results for the variances are very different from those for doublets. Even with a small value of μ, the variance is appreciably above that for the binomial, and when $\mu = 20$ it is nearly five times the binomial value. Seriously misleading conclusions as to the significance of the association in an observed set of μ points might be drawn if the number of triplets were compared with its expectation with the aid of the binomial distribution or the normal approximation thereto.

Quadruplets.—The probability for a single quadruplet is

$$P = \frac{24[100\,mn - 218\,(m+n) + 403]}{mn\,(mn-1)\,(mn-2)\,(mn-3)}$$
$$= 0{\cdot}0017961 \quad \text{for } m = n = 10.$$

The expectation of q, the number of quadruplets in μ random points, is

$$E(q) = {}^{\mu}C_4P,$$

TABLE II.—*Sampling Results for Triplets*

μ	$E(t)$	t	${}^{\mu}C_2P(1-P)$	s_t^2
3	·009	·03	·009	·029
4	·037	06	·036	·118
5	·091	·15	·090	·311
6	·183	·26	·181	·699
7	·319	·30	·317	·717
8	·511	·46	·507	·776
9	·767	·73	·760	1·088
10	1·095	1·17	1·085	1·839
11	1·51	1·57	1·49	2·41
12	2·01	2·08	1·99	3·85
13	2·61	2·80	2·59	6·59
14	3·32	3·65	3·29	7·46
15	4·15	4·70	4·12	11·89
16	5·11	5·88	5·07	17·3
17	6·21	7·07	6·15	23·5
18	7·45	8·17	7·38	30·7
19	8·85	10·00	8·76	41·3
20	10·41	11·97	10·31	51·4

and the binomial variance is ${}^{\mu}C_1P(1-P)$; the sample estimates of mean and variance are compared with these in Table III. There is a tendency for q to exceed its expectation, but this is probably due to the chance occurrence of several very high values. For all values of μ, the variance exceeds the binomial variance, and clearly the latter bears little relationship to the true variance of the statistic q; when $\mu = 20$, the experimental variance is eighteen times that for the binomial.

TABLE III.—*Sampling Results for Quadruplets*

μ	$E(q)$	q	${}^{\mu}C_4P(1-P)$	s_q^2
4	·002	·01	·002	·010
5	·009	·02	·009	·020
6	·027	·05	·027	·169
7	·063	·05	·063	·169
8	·126	·05	·126	·169
9	·226	·14	·226	·384
10	·377	·36	·377	1·041
11	·593	·58	·592	1 62
12	·889	·93	·887	3·60
13	1·284	1·41	1·282	7·58
14	1·798	2·00	1·795	9·29
15	2·452	2·99	2·447	19·59
16	3·27	4·16	3·26	26·6
17	4·27	5·44	4·27	43·0
18	5·50	6·65	5·49	72·4
19	6·96	8·92	6·95	118·6
20	8·70	11·38	8·69	159·8

The danger of assessing probabilities for these distributions by means of the binomial or Poisson "approximations" is well illustrated by comparison of observed frequencies in the random sampling experiment with expectations for the theoretical distributions. The contrast is most marked for quadruplets, but is also clear for triplets. For example, with $\mu = 15$ the expected number of quadruplets is $2 \cdot 45$, and the observed mean is $2 \cdot 99$; expected frequencies in samples of 100 from Poisson distributions with these means, taken roughly from Molina's table (1945), by comparison with the observed frequencies, are:

Frequency	*Poisson with mean* 2·45	*Poisson with mean* 2·99	*Observed*
0	9	5	28
1	21	15	29
2	26	22·5	8
3	21	22·5	7
4	13	17	10
5	6	10	2
6	3	5	2
>6	1	3	14

Of the fourteen instances of more than 6 quadruplets, one has 21, another 20, and five more exceed 10, a result virtually "impossible" for either Poisson distribution. For $\mu = 20$, there are four sets having more than 50 quadruplets (the highest being 63), and ten others exceeding 20; all of these would be judged very highly significant deviations from a Poisson distribution of mean $8 \cdot 7$, yet all have arisen in a random sample of 100 trials.

Enough has been said to emphasize the danger of using tests of significance for departures from random association based on the numbers of triplets and quadruplets, at least until fuller investigation of the sampling distributions of these statistics has been made. Though the distribution of the number of doublets in a random set of points may also be expected to depart from the binomial form, the results presented above suggest that the effect may be much less serious; even on this point, further theoretical or empirical evidence is needed before the test is adopted uncritically.

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THE OSCILLATORY PROPERTIES OF THE MOVING AVERAGE

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SUMMARY

THE process of eliminating trend from a time series by means of the method of moving averages is shown to be extremely dangerous if the series is being examined for oscillatory movements. It is shown that the "trend free" moving average difference series formed by the difference between the terms of the original series and the moving average is always liable to be oscillatory whether or not the original series contains oscillatory components. The expression for the serial correlation coefficients and variance of the moving average difference series is derived in terms of the serial correlation coefficients and variance of the original series. It is shown that for typical examples of auto-correlated non-oscillatory time series the method of moving averages is liable to introduce oscillations of considerable magnitude.

1. Most time series exhibit a long term movement or trend in addition to the short term variations. The analysis of such series is usually concerned with the short term variations, as they are expected to be a part of the mechanism of the series, whereas the trend may be due to the impact of changing conditions. The presence of this trend complicates the analysis of the short term variations and different methods have been devised to eliminate it, the most usual being to consider the trend as defined locally by a polynomial, in which case it may be determined by some form of a moving average. Kendall (1941) discussed the effect of this method of eliminating trend on the oscillatory movements in time series, and came to the conclusion that when the moving average extends over a considerable number of terms approximating to a multiple of the length of any cycles which are suspected to be present, taking the trend to be the moving average does not distort seriously any genuine effects, nor does it introduce very marked spurious oscillations, i.e. to an extent greater than would be produced on a random series.

Some recent work of mine threw some doubt on the validity of this conclusion and led to the investigation described below.

Kendall (1941) considered a series which comprises a trend, an oscillatory portion, and a residual random portion. The trend was assumed to be defined by the moving average, and therefore he only considered the effect of taking the moving average process on the oscillatory and random portions. It seems to me that this model of a time series has not a general application, and that in certain cases eliminating trend by a moving average method may introduce a pronounced oscillatory movement.

If trend is regarded in this way there is nothing more to be said, but this point of view will be shown to result in practically every series being regarded as the resultant of a moving average and an oscillatory series, as defined by Yule (1926).

2. Everyone has his own ideas about what trend means. For my part, I cannot help trying to visualize the processes of any time series on the analogy of the cross sections of a textile yarn. I make no apology for introducing a textile aspect to this paper, because I regard textiles as a very fruitful application of time series. In this type of time series, which is the same as a population series with a life corresponding to a fibre, variations in the mean density of the fibre ends produce a more or less smooth series which is very similar to the resultant of a smooth trend with random variations superimposed. But it is artificial to regard the series as made up in this way, because both features are produced by the same mechanism, and neither the smooth trend nor the short erratic fluctuations can be produced without the other.

This type of series can only be analysed as a whole, as I hope to show in a future paper. Of course such series do not contain very prolonged steady increases or decreases in the general values of the terms, as may happen in economic series, and where such movements occur the use of the moving average method may be valid.

In any time series the moving average will never correspond exactly to the terms of the series, but will exceed or be less than them with approximately equal frequency. When the series is autocorrelated, consecutive terms will tend to lie on the same side of the moving average, and thus terms further removed from each other will therefore tend to lie on opposite sides. Thus the moving average may be expected to oscillate about the original series, i.e. the series formed by the difference between the original terms and the moving average, which I shall term the moving average difference series, will be oscillatory whether the original series contains an oscillatory component or not.

3. Let the terms of any time series be represented by—

$$u_1, u_2, u_3, \text{etc.}, \ldots u_x, \text{etc.},$$

and suppose we take *a* $(2n + 1)$ term moving average with weights—

$$a_{-n}, a_{-n+1} \text{ etc.}, \ldots a_0, \ldots \text{etc., to } a_n,$$

$$\sum_{j=-n}^{j=n} a_j = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.1)$$

The moving average at x is—

$$_{2n+1}\bar{u}_x = a_{-n}u_{x-n} + a_{-n+1}u_{x-n+1} + \text{etc.}, + a_0u_x + a_1u_{x+1} + \text{etc., to} + a_nu_{x+n} \quad (3.2)$$

The value of the moving average difference series at x is therefore—

$$U_x = u_x - {}_{2n+1}\bar{u}_x = u_x - a_{-n}u_{x-n} - a_{-n+1}u_{x-n+1} - \text{etc.}, - a_0u_x - \text{etc., to} - a_nu_{x+n} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.3)$$

and at $x + s$ it is—

$$U_{x+s} = u_{x+s} - {}_{2n+1}\bar{u}_{x+s} = u_{x+s} - a_{-n}u_{x-n+s} - \text{etc., to} - a_nu_{x+n+s} \quad . \quad (3.4)$$

The terms of this series may be examined for oscillatory movements according to Yule's (1926) definition by evaluating the serial correlogram. We have—

$$\text{Covar. } U_xU_{x+s} = \frac{1}{N-s}\sum_{x=0}^{x=N-s} U_x \, . \, U_{x+s}$$

$$= \frac{1}{N-s}\sum_{x=0}^{x=N-s}\Big[u_xu_{x+s} - u_x\{a_{-n}u_{x-n+s} + a_{-n+1}u_{x-n+s+1} + \text{etc.}, + a_nu_{x+n+s}\}$$

$$- u_{x+s}\{a_{-n}u_{x-n} + a_{-n+1}u_{x-n+1} + \text{etc.}, + a_nu_{x+s}\}$$

$$+ \{a_{-n}u_{x-n} + a_{-n+1}u_{x-n+1} + \text{etc., to} + a_nu_{x+n}\}\{a_{-n}u_{x-n+s}$$

$$+ a_{-n+1}u_{x+s-n+1} + \text{etc., to} + a_nu_{x+s+n}\}\Big] \quad . \quad . \quad . \quad (3.5)$$

Provided that the series is long enough for the effect of the loss of the end terms to be negligible we may write—

$$\frac{1}{N-k}\sum_{x=0}^{x=N-k} u_x \, . \, u_{x+k} = r_k \operatorname{Var} u.$$

Where Var U is the variance of the original series and r_k is the serial correlation coefficient of the original series for interval k, the expression becomes—

$$\text{Covar. } U_t U_{t-s} = \text{Var } u \Big[r_s - 2\left\{ a_{-n} r_{s-n} + a_{-n+1} r_{s-n+1} + \text{etc.}, + a_0 r_s + \text{etc., to } a_n r_{s+n} \right\}$$

$$+ \Big\{ a_n \,.\, a_{-n} \,.\, r_{s-2n} + (a_{-n} \,.\, a_{n+1} + a_{-n+1} \,.\, a_n) r_{s-2n+1} + \text{etc.},$$

$$+ \sum_{m=0}^{m=p} a_{-n+m} \,.\, a_{n-p+m} \,.\, r_{s-2n+p} + \text{etc., to } + \sum_{m=0}^{m=2n} a_{-n+m} \,.\, a_{n-2n+m} \,.\, r_s$$

$$+ \text{etc.}, + \sum_{m=0}^{m=q} a_{n-m} \,.\, a_{-n+q-m} \,.\, r_{s+q} + \text{etc., to } a_n a_{-n} r_{s+2n} \Big\} \Big] \quad . \quad . \quad (3.6)$$

The variance of the moving average difference series, Var. U_x, is obtained by putting $s = 0$ in Equation (3.6) and the serial correlation coefficient R_s for interval s is of course—

$$R_s = \frac{\text{Covar. } U_t \,.\, U_{(T+s)}}{\text{Var. } U_x} \quad . \quad . \quad . \quad . \quad . \quad (3.7)$$

4. Probably the most common type of moving average is when the weights a_{-n}, a_{-n+1}, etc. to a_n are equal and have value $\frac{1}{2n+1}$, in which case—

$$\text{Covar. } U_x \,.\, U_{x+s} = \frac{\text{Var. } U_x}{(2n+1)^2} \Big[(2n+1)^2 r_s - 2(2n+1)\left\{ r_{s-n} + r_{s-n+1} + \text{etc.}, + r_0 + \text{etc.} + r_{s+n} \right\}$$

$$+ \left\{ r_{s-2n} + 2r_{s-2n+1} + \text{etc.}, + (2n+1) r_s + 2_n r_{s+1} + \text{etc.}, + r_{2n+s} \right\} \Big] \quad . \quad . \quad (4.1)$$

Yule's (1926) definition of an oscillatory series is one in which the serial correlation coefficient changes sign, i.e. that Covar. $U_t U_{r+s}$ is negative for some values of s. It will be seen that the expression in Equation (4.1) contains a negative term which may easily exceed the sum of the positive terms for certain values of s, even when the original series does not contain an oscillatory component.

Equations (3.5), (3.6) and (3.7) show that the serial correlation of the moving average difference series depends only on the serial correlation coefficients of the original series and the weights of the terms in the moving average; thus in considering whether the moving average difference series of any series is oscillatory we need only consider the different types of series in terms of their serial correlograms. For this purpose, it seems to me that there are four fundamental types of time series : (1) Random series, (2) Homogeneous series, (3) Oscillatory series and (4) Periodic series, which may be defined as follows:—

(1) Random series, for which all the serial correlations are zero.

(2) Homogeneous series, for which (*a*) all the serial correlation coefficients are positive or zero, and (*b*) the change in the serial correlation coefficient for unit increase in the interval $\frac{\Delta r_s}{\Delta s}$ is always negative or zero.

(3) Oscillatory series, as defined by Yule (1926), for which the serial correlation coefficient (*a*) changes sign, being alternatively positive and negative, and (*b*) the series formed by the successive maxima and minima of the correlogram converges to zero.

(4) Periodic series, a special case of oscillatory series, for which the serial correlations are undamped harmonic functions of s.

This represents a very slight modification of Yule's (1926) classification.

For the purpose of considering the serial correlograms only, series met with in practice may be regarded as the resultant of some or all of these types. Thus, for example, Wolfer's sunspot

numbers (*cf.* Spencer-Smith (1944)) may be regarded as the resultant of a homogeneous, an oscillatory and a periodic series.

5. We now consider the moving average difference series of any serially correlated time series represented in this artificial manner. Suppose the terms of the series to be capable of representation by—

$$f(x) = f_1(x) + f_2(x) + f_3(x) \quad . \quad . \quad . \quad . \quad . \quad (5.1)$$

where $f_1(x)$ represents the terms of a homogeneous series,

$f_2(x)$ represents the terms of an oscillatory series,

$f_3(x)$ represents the terms of a periodic series,

any of which may or may not be zero. There is no need to include a random series when the series has a serially correlated component. The serial correlation coefficient of the series for interval s is—

$$r_s = \frac{{}_1r_s \operatorname{Var.} f_1(x) + {}_2r_s \operatorname{Var.} f_2(x) + {}_3r_s \operatorname{Var.} f_3(x)}{\operatorname{Var.} f_1(x) + \operatorname{Var.} f_2(x) + \operatorname{Var.} f_3(x)} \quad . \quad . \quad . \quad (5.2)$$

where ${}_1r_s$, ${}_2r_s$ and ${}_3r_s$ are the serial correlation coefficients of $f_1(x)$, $f_2(x)$ and $f_3(x)$ respectively.

The terms $F(x)$ of the moving average difference series of $f(x)$ are given by—

$$F(x) = F_1(x) + F_2(x) + F_3(x) \quad . \quad . \quad . \quad . \quad . \quad (5.3)$$

where $F_1(x)$, $F_2(x)$ and $F_3(x)$ are the moving average difference series of $f_1(x)$, $f_2(x)$ and $f_3(x)$. The serial correlation coefficient R_s of $F(x)$ for interval s is—

$$R_s = \frac{{}_1R_s \operatorname{Var.} F_1(x) + {}_2R_s \operatorname{Var.} F_2(x) + {}_3R_s \operatorname{Var.} F_3(x)}{\operatorname{Var.} F_1(x) + \operatorname{Var.} F_2(x) + \operatorname{Var.} F_3(x)} \quad . \quad . \quad (5.4)$$

where ${}_1R_s$, ${}_2R_s$ and ${}_3R_s$ are the corresponding serial correlation coefficients of $F_1(x)$, $F_2(x)$ and $F_3(x)$.

From the practical point of view, viz. analysing the time series, the important consideration is : by how much will R_s differ from—

$$\frac{{}_2r_s \operatorname{Var.} f_2(x) + {}_3r_s \operatorname{Var.} f_3(x)}{\operatorname{Var.} f_2(x) + \operatorname{Var.} f_3(x)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.5)$$

which represents the serial correlogram of the oscillatory and periodic portions only?

6. To answer this question we examine the four basic types of component series, for oscillatory movements.

(1) *Random Series.*

Yule (1926) has shown that the moving average difference series of a random series is oscillatory in the sense defined above, and Kendall (1941) discussed its effect when an oscillatory series is also present. Provided that the moving average extends over a fair number of terms the negative correlations will always be small.

(2) *Homogeneous Series.*

The serial correlograms of homogeneous series vary so widely that it seems practically impossible to cover every case, but the following cases are worth considering.

When the serial correlogram of the original series is given by—

$$r_s = p^s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.1)$$

where $0 < p < 1$ it is easy to show that—

(*a*) R_s is negative and $\frac{\Delta r_s}{\Delta s}$ is positive when $s = n$,

(*b*) R_s is positive and $\frac{\Delta r_s}{\Delta s}$ is negative when $s \geq 2n$,

so that the moving average difference series is oscillatory. This will obviously hold also when r_s is the mean of a number of similar terms having different values of p.

In general, when r_s decreases more rapidly than p^s, the positive portion of Covar. $U_x U_{x+s}$ in Equation (3.6) decreases more rapidly than the negative portion as s increases, in the neighbourhood of $s = n$. Thus it may be concluded that the moving average difference series of a homogeneous series is usually oscillatory. This result is proved in a different way in Section 8.

The serial correlation coefficients R_s for interval s of the 9 term moving average difference series are given in Table 1 for the following cases of homogeneous series:

Case 1, in which the serial correlation coefficient is given by—

$$r_s = (0{\cdot}9)^s,$$

Case 2, in which— $r_s = (0{\cdot}7)^s,$

Case 3, in which— $r_s = 1 - (0{\cdot}1)s$ for $s \leq 10,$

and $r_s = 0$ for $s > 10,$

Case 4, for a series with an auto-regression equation—

$$u_x = 0{\cdot}9\, U_{x-1} - 0{\cdot}2\, U_{x-2} + E_x.$$

This series is not oscillatory by Kendall's (1943) criterion.

The serial correlation coefficients of the original series are also given in the table together with the relative variance of the moving average difference series, which is defined as the ratio of the variance of the moving average difference series to that of the original series.

TABLE 1.—*Serial Correlation Coefficients of the 9-term Moving Average Difference Series of Different Homogeneous Series*

	CASE 1. Series with serial correlation coefficients. $r_s = (0{\cdot}9)^s$		CASE 2. Series with serial correlation coefficients. $r_s = (0{\cdot}7)^s$		CASE 3. Series with serial correlation coefficients. $r_s = 1 - 0{\cdot}1s$ for $0 < s < 10$, $r_s = 0$ for $s > 10$		CASE 4. Series given by auto regression equation. $U_x = 0{\cdot}9u_{x-1} - 0{\cdot}2u_{x-2} + E_x$	
s.	Serial correlation coefficient of original series r_s.	Serial correlation coefficient of 9 term moving average difference series R_s.	Serial correlation coefficient of original series r_s.	Serial correlation coefficient of 9 term moving average difference series R_s.	Serial correlation coefficient of original series r_s.	Serial correlation coefficient of 9 term moving average difference series R_s.	Serial correlation coefficient of original series r_s.	Serial correlation coefficient of 9 term moving average difference series R_s.
0	1·000	1·000	1·000	1·000	1·000	1·000	1·000	1·000
1	0·900	0·398	0·700	0·316	0·900	0·400	0·750	0·481
2	0·810	−0·143	0·490	−0·102	0·800	−0·033	0·475	−0·100
3	0·729	−0·449	0·343	−0·332	0·700	−0·283	0·278	−0·412
4	0·656	−0·490	0·240	−0·353	0·600	−0·300	0·155	−0·437
5	0·590	−0·256	0·168	−0·141	0·500	−0·083	0·084	−0·210
6	0·531	−0·123	0·118	−0·031	0·400	0·100	0·044	−0·048
7	0·478	0·061	0·083	0·013	0·300	0·125	0·025	0·033
8	0·430	0·020	0·058	0·022	0·200	0·017	0·013	0·039
9	0·387	0·018	0·041	0·015	0·100	−0·200	0·007	0·024
10	0·348	0·016	0·029	0·011	0·000	−0·500	0·004	0·013
11	0·313	0·014	0·020	0·008	0·000	−0·200	0·002	0·000
12	0·281	0·013	0·014	0·006	0·000	0·017	0·001	−0·005
13	0·253	0·012	0·010	0·004	0·000	0·142	0·000	−0·002
14					0·000	0·167		
15	Tends to zero		Tends to zero		0·000	0·083	Tends to zero	
16					Tends to zero			
R.V.	0·138		0·437		0·148		0·453	

These results show that in each case the moving average difference series is oscillatory and may have by no means negligible variance. Case 3 corresponds to an experimental 1,000 term series which I had available, and for which I calculated the 9 term moving average difference series. The actual serial correlation coefficients of this series correspond very closely with the calculated values for all intervals, as the following few examples show:

$$r_1 = 0\cdot340,\ r_4 = -0\cdot256,\ r_7 = 0\cdot115,\ r_{10} = -0\cdot463 \text{ and } r_{11} = 0\cdot131.$$

The effect of the number of terms used in the moving average is illustrated in Table 2 for a series in which—

$$r_s = 1 - (0\cdot2)s \text{ for } s < 5$$
$$\text{and } = 0 \text{ for } s > 5$$

TABLE 2

Serial correlation coefficients of moving average difference series.

Interval.	3 term.	5 term.	7 term.	9 term.	11 term moving average.
1	−0·500	0·000	0·322	0·515	0·600
2	0·000	−0·400	−0·036	0·129	0·179
3	0·000	0·100	−0·250	−0·228	−0·152
4	0·250	0·000	−0·268	−0·457	−0·420
5	−0·500	−0·500	−0·535	−0·571	−0·620
6	0·250	0·000	−0·125	−0·228	−0·328
7	0·000	0·200	0·107	0·000	−0·110
8	0·000	0·050	0·178	0·121	0·035
9	..	0·000	0·072	0·143	0·110
10	..	..	0·018	0·072	0·128
11	..	..	0·000	0·029	0·069
12	..	..	..	0·007	0·035
13	..	..	..	0·000	0·014
14	..	..	..	..	0·003
15	..	..	..	..	0·0005
Relative variance	·089	·160	·237	·333	·470

In this case both the oscillatory nature and the relative variance of the moving average difference series increase as the number of terms in the moving average increases, as indeed follows from Equations (3.6) and (3.7). The introduction of two minimum values in the correlogram of the 3 and 5 term moving average difference series should be noted.

(3) *Oscillatory Series.*

When the original series is oscillatory, a number of the serial correlation coefficients will be negative; thus the value of Covar. $U_x \,.\, U_{x+s}$ in Equation (3.7), and therefore of the serial correlation coefficients of the moving average difference series, will be largely determined by the corresponding serial correlation coefficients of the original series. But if, as is usual, the correlogram of an oscillatory series is damped, the sum of the remaining terms in the expression will not be zero, and neither the correlogram nor the variance of the moving average difference series will equal those of the original series. This difference will increase with the degree of damping of the original correlogram, and is illustrated by the calculated values of the serial correlograms and relative variances of the 9 term moving average difference series for two oscillatory series with different degrees of damping, given in Table 3.

TABLE 3.—*Serial Correlation Coefficients of the 9 Term Moving Average Difference Series of Oscillatory Series*

Interval.	CASE 1. Oscillatory series for which $r_s = e^{-0\cdot2s}\cos\frac{2\pi s}{9}$		CASE 2. Oscillatory series for which $r_s = e^{-0\cdot4s}\cos\frac{2\pi s}{9}$	
	Serial correlation coefficient of original series r_s.	Serial correlation coefficient of 9 term moving average difference series R_s.	Serial correlation coefficient of original series r_s.	Serial correlation coefficient of 9 term moving average difference series R_s.
0	1·000	1·000	1·000	1·000
1	0·627	0·557	0·514	0·460
2	0·117	−0·092	0·078	−0·218
3	−0·278	−0·546	−0·150	−0·532
4	−0·422	−0·615	−0·190	−0·472
5	−0·346	−0·366	−0·127	−0·118
6	0·151	0·002	−0·046	0·087
7	0·043	0·245	0·011	0·169
8	0·155	0·262	0·031	0·128
9	0·165	0·196	0·027	0·063
10	0·103	0·140	0·014	−0 009
11	0·019	−0·040		
12	−0·045	−0·113		
13	−0·070	−0·089		
14	−0·057	0·065		
Relative variance		0·885		0·747

A comparison of these serial correlograms with those given in Table 1, which are derived from homogeneous series, a striking similarity between the two and illustrates the danger of using the moving average to eliminate trend.

(4) *Periodic Series.*

There is little need to mention the case of a strictly periodic series: when the number of terms in the moving average is equal to the length of the period, it is obvious that the moving average difference series will correspond exactly to the original series.

The conclusion is that when equal weights are given to the terms of the moving average, the moving average difference series is always oscillatory, whether the original series contains an oscillatory component or not. When the original series contains an oscillatory component, the serial correlogram of the moving average difference series may approximate to that of the oscillatory portion of the series, but such agreement must be regarded as a coincidence.

When the terms of the moving average have different weights, the problem is more complicated, but it is treated in another way in section 8. When the weights of the terms of the moving average rise to a pronounced maximum in the region of the central term it is obvious from Equation (3.7) that the relative variance of the moving average difference series will be much smaller than when equal weights are used.

7. Before considering the general case of a moving average having different weights it is interesting to investigate the moving average difference series of a homogeneous series in rather more detail. Such a series is usually oscillatory and might be expected to approximate to the type of series investigated by Kendall (1944), viz. that generated by the auto regression equation

$$u_{x+2} + au_{x+1} + bu_x + E_{x+2} = 0 \qquad (7.1)$$

where a and b are constants such that $4b > a^2$, and E_{x+2} is a random term. Kendall (loc. cit.) showed that the serial correlation coefficient for interval s, of—

$$r_s = \frac{p^s \sin(\theta s + \psi)}{\sin \psi}$$

where—

$$\theta = \tan^{-1} \sqrt{\frac{4b}{a^2} - 1} \text{ and } \tan\psi = \frac{1 + p^2}{1 - p^2} \tan\theta \Big\} \quad . \quad . \quad . \quad (7.2)$$

and— $p = +\sqrt{b}$

the values of a and b.

Yule (1927) and Walker (1931) showed that the values of a and b for an auto regression equation of the type of (7.1), from the difference equations of the serial correlogram, viz.:

$$r_{j+2} + ar_{j+1} + br_j = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.3)$$

where j = −1, 0, 1, 2, etc.

Kendall (loc. cit.) followed Yule and used j = −1 and 0 to derive his values of a and b, as follows:

$$a = -\frac{r_1 \cdot (1 - r_2)}{1 - r_1^2}; \quad b = \frac{r_1^2 - r_2}{1 - r_1^2} \quad . \quad . \quad . \quad . \quad .. \quad (7.4)$$

where r_1 and r_2 are the serial correlation coefficients for intervals 1 and 2 respectively. He also found that the oscillation period calculated from equations (7.2) and (7.4) was liable to be less than that estimated from the positions of the maxima of the correlogram, a fact which he showed might be attributed to the presence of a superimposed random series.

In applying equation (7.1) to the moving average difference series of the special homogeneous series considered in section 5, i.e. cases 1 to 4 in Table 1, the possibility of a residual random component cannot be ignored, and it seems preferable to calculate a and b from equation (7.3) using $j = 1$ and 2.

These values of a and b may be used to extrapolate the correlogram back to the value of r_0 for the oscillatory portion of the series for s 0, and thus the variance of the random component, Var. E, may be calculated as—

$$\text{Var. E} = (1 - r_0) \text{ Var. } u \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.5)$$

where Var. u is the variance of the series.

The values of a and b calculated by Equation (7.3) with $j = 1$ and 2 from R_1, R_2, R_3 and R_4 for the four moving average difference series considered in Table 1 are given in Table 4, with the values calculated by Yule's equation (7.4).

TABLE 4.—*Calculated Values of* a *and* b

	Calculated by Equation (7.3) with j = 1, 2.			Calculated by Kendall's Method. Equation (7.4).		
	− *a*.	*b*.	Oscillation period.	− *a*.	*b*.	Oscillation period.
Case 1	1·305	0·659	9·85	0·541	0·358	5·68
„ 2	1·260	0·635	8·85	0·387	0·215	5·50
„ 3	1·022	0·625	7·25	0·492	0·230	10·24
„ 4	1·209	0·606	7·98	0·690	0·431	6·17

The comparison of the serial correlation coefficients of these four moving average difference series with those calculated by Equation (7.3) from a and b as calculated from $j = 1$ and 2 and from R_1 and R_2 are given in Table 5.

TABLE 5

Interval.	Moving average difference Series of Series 1. $r_s = (0·9)^s$		Moving average difference Series of Series 2. $r_s = (0·7)^s$		Moving average difference Series of Series 3. $r_s = 1 - 0·1s$ for $s < 10$, $r_s = 0$ for $s \geq 10$		Moving average difference Series of Series 4 given by $u_{r+2} = 0·9u_{r+1} - 0·2u_r + E_{r+2}$	
	Serial correlation coefficient of moving average difference series R_s.	R_s calculated from $R_s = 1·31R_{s-1} - 0·66R_{s-2}$.	Serial correlation coefficient of moving average difference series R_s.	R_s calculated from $R_s = 1·26R_{s-1} - 0·635R_{s-2}$.	Serial correlation coefficient of moving average difference series R_s.	R_s calculated from $R_s = 1·022R_{s-1} - 0·625R_{s-2}$.	Serial correlation coefficient of moving average difference series R_s.	R_s calculated from $R_s = 1·209R_{s-1} - 0·606R_s$.
0	1·000	1·005	1·000	0·790	1·000	0·708	1·000	1·125
1	0·398	0·398*	0·316	0·316*	0·400	0·400*	0·481	0·481*
2	−0·143	−0·143*	−0·102	−0·102*	0·033	−0·033*	−0·100	−0·100*
3	−0·449	−0·449*	−0·332	−0·332*	−0·283	−0·283*	−0·412	−0·412*
4	−0·490	−0·490*	0·353	−0·353*	−0·300	−0·300*	−0·437	−0·437*
5	−0·256	−0·343	0·141	0·245	−0·083	−0·100	−0·210	−0·270
6	−0·123	−0·126	−0·031	−0·079	0·100	+0·069	−0·048	−0·068
7	0·061	0·061	0·013	+0·056	0·125	+0·134	0·033	+0·082
8	0·020	0·163	0·022	+0·121	0·017	+0·094	0·039	+0·140
9	0·018	0·182	0·015	+0·116	−0·200	+0·012	0·024	+0·119
10	0·016	0·131	0·011	+0·069	−0·500	−0·047	0·013	+0·059
11	0·014	..	0·008	+0·016	−0·200	−0·057	0·000	−0·001
12	0·013	..	0·006	..	0·017	..	−0·005	−0·037

* Used to calculate the other coefficients.

In each case there is fair agreement between the two values as fas as R_7, after which the two diverge. This divergence could, however, only be detected in practice if a very long series was available. The extrapolated value of R_0 for Case 1 is sensibly unity, so that this moving average difference series approximates closely to Kendall's auto regressive scheme. Cases 2 and 3 show similar fits, although here the extrapolated values of R_0 are less than unity, whilst in Case 4 it exceeds it.

I do not consider that this correspondence between the moving average difference series of these homogeneous series and Kendall's auto regressive type of series has any special significance, but it serves to emphasize the danger of using the moving average when searching for an oscillatory movement.

The difference between the values of a and b as calculated for Case 1 by the two methods seems surprising at first sight in view of the fact that the extrapolated value of r_0 for the oscillatory part of the correlogram approximates to unity. The reason is, of course, that the auto regression equation (7.1) does not hold: the difference equation,

$$r_{j+2} + ar_{j+1} + br_j = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.3)$$

only holds for $j \geq 0$. This particular moving average difference equation approximates to the second order process discussed by Bartlett (1946).

8. The conclusion in section 6 can be reached in a different way. Practically any serial correlogram of the three basic types described in section 4 can be produced by a series whose terms are formed by a moving sum with appropriate weights, of a series of random numbers, thus:

$$u_x = \sum_{i=1}^{i=m} a_i \, . \, E_{x-i} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.1)$$

where E_x, E_{x-i}, etc., are the terms of a random series, and . . . a_i, etc., are the appropriate weights.* Dodd (1939) has shown that the serial correlation coefficient of such a series for interval s is—

$$r_s = \sum_{i=0}^{i=m-s} a_i a_{i+s} \Big/ \sum_{i=0}^{i=m} a_i^2 \quad . \quad . \quad . \quad . \quad (8.2)$$

From this it follows that the conditions which limit the values of a_1, a_2, etc., for the basic types of series are:

(1) For a homogeneous series: a_i is positive or zero for all values of i and $\frac{\Delta^2 a}{(\Delta i)^2}$ does not change sign.

(2) For an oscillatory series: a_i changes sign, m is finite and $\sum_{i=0}^{i=m} a_i = 0$.

(3) For a periodic series: the series $m = \infty$, and the series a_0 a_1, a_2 . . . etc., a_i, etc., forms an infinite periodic series.

The moving average difference series of a series of the type given in Equation (8.1) may also be expressed as a moving average with different weights, of the same random series. But if, as is essential in all graduation formulae, the sum of the weights in the moving average is unity, the sum of the weights in the moving sum of the random series representing the moving average difference series will be zero. Thus by the second condition above the moving average difference series of any time series is always liable to be oscillatory, no matter what weights are given to the moving average.

From the foregoing remarks it is clear that the method of eliminating trend by means of a moving average is an exceedingly dangerous process for investigating oscillatory movements, since the method is in itself a sure way of generating an oscillatory series. The method may be applicable in certain series in which the terms show a general upward or downward tendency, but even in this case the oscillations in the residual series should be viewed with suspicion unless there is other evidence about them.

The question arises: How genuine are the oscillations in moving average difference series such as Kendall's (1943) statistics of British agriculture? Are they merely the result of the moving average process? Certainly they show a striking similarity to the oscillations of the moving average difference series of a homogeneous series, but this is not evidence that they are not genuine. The moving average difference provides no evidence either for or against, and until they are confirmed by other methods they must be treated with suspicion.

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* Dodd (1939) considers all series of this type to be oscillatory, but if we follow Yule's (1926) definition, this is only true if the second condition given below holds true.

THE FACTOR ANALYSIS OF A MATRIX OF 2 × 2 TABLES

By PATRICK SLATER

1. *Origin of the Present Paper*

A PAPER with the title "A method of factor analysis for application to matrices of 2 × 2 tables" was circulated privately to members of the Advisory Committee of the Directorate of Selection of Personnel, War Office, in January, 1946. It described the method of analysis and the test of significance given here. Less discussion was given of other alternative methods, more attention was paid to working methods, and the example used was an artificial one.

In this paper I have used experimental data referred to me for analysis by Dr. W. Mayer-Gross, but it is on the method of analysis and the test of significance, which are the same as in the original paper, that I would particularly welcome comments. The psychological interpretation of the results will be presented for discussion elsewhere.

2. *The General Form of the Problems to which Factor Analysis is Applicable*

Factor analysis is a statistical procedure often used in attempts to solve psychological problems. These problems are generally similar in form. They arise because there is no limit to the number of ways (variables or attributes) in which human variation may be observed and recorded, but it is commonly thought that the variations observed can be ascribed to underlying psychological characteristics (factors) the number of which is limited; or, at least, that some of these characteristics account for human behaviour to a greater extent than others, and that if accurate assessments of a limited number of them can be obtained, variations of human behaviour in a great many other respects can be predicted from them.

A hypothesis may therefore be set up, such as, that variation observed and recorded in terms of variables a, b, c, etc., is determined by factor 1; variation in terms of variables n, o, p, etc., by factor 2, and so on. An experiment may be conducted to observe a number of individuals and record their variation in terms of all the variables. The correlations are obtained between the individuals' measurements in terms of each variable and every other. Using i and j to indicate any two variables, the correlations form a matrix which may be written $[r_{ij}]$. Under the condition $i \neq j$ this is a square symmetrical matrix with no entries in its leading diagonal. Applied to this, factor analysis derives one or more independent sets of loadings, each of which may be written as a column matrix $[l_1]$, $[l_2]$, . . ., etc. In each column there is one loading for each of the variables, and loadings of different variables may vary between +1 and −1. The sum of

$$[l_1]\,[l_1]' + [l_2]\,[l_2]' + \ldots, \text{etc.}$$

(when $[l]'$ is $[l]$ written as a row instead of a column) provides a matrix of expected correlation coefficients $[e'_{ij}]$. The hypothesis is considered confirmed if the variables associated with a particular factor are found to have significant loadings in the same column, and if, under the condition $i \neq j$, $[e'_{ij}]$ does not differ significantly from $[r_{ij}]$.

More often the hypothesis is not precisely defined. If a new psychological test or a new method of observation has been designed, an experiment may be made to find with what factors the variations it records are associated. Other variables, with factorial associations known from previous experiments, are included with it, and its factor loadings are found and compared with theirs. Or no prior hypothesis advanced: a psychological explanation is sought, *post hoc*, for the fact that a group of variables prove on analysis to have significant loadings in the same column. Factor analysis can also be applied to correlations between persons, and persons with similar factor loadings are described as belonging to the same type.

Various methods of factor analysis have been described. Useful reviews of alternative methods are given by Holzinger and Harman,[1] Burt[2] and Thomson[3]. Whatever method is used, the problem must arise of testing how well the hypothetical matrix $[e'_{ij}]$ fits the observed matrix $[r_{ij}]$. Various solutions have been proposed. Spearman's use of tetrad differences is of historical

interest,[4] and Kelley's contributions to the subject should be noted ([5] and [6]), but Holzinger and Harman's recent review shows that no solution has yet been accepted as authoritative.

If two methods of analysis are applied to the same matrix, the one which gives the poorer fit after a given number of factors have been extracted will leave the larger residual correlations for the extraction of further factors. Paradoxically, therefore, the more inefficient the method used, the more factors will be discovered. The test of goodness of fit proposed by Burt (*op. cit.*, p. 339) makes due allowance for the increasing stringency of the requirements for a good fit as the number of factors postulated increases.

In the analysis described here, the method for computing the terms used in place of factor loadings is adapted from Thurstone[7], and the method for testing goodness of fit from Burt. My chief aim in the adaptations I have made has been to eliminate unnecessary intermediate processes in the argument and in the method of computation, and to illustrate the essential steps clearly. I hope that as a result, attention can be concentrated on the problem of finding a satisfactory test of goodness of fit, and this problem can be brought nearer to a complete solution.

The essential function of factor analysis is to classify—to test, for instance, whether variables a, b, c, etc., belong in a single class, whether this is the same class as that which includes variables n, o, p, etc., or not; whether an indeterminate variable properly belongs to one class or another, and so on. In this way it may subserve psychological arguments concerning characteristics supposedly common to different variables; but it may also be applied as a classificatory procedure when the characteristics postulated are not psychological. It has possible applications outside psychology.

3. *Its Application to a Matrix of* 2 × 2 *Tables*

Table 1 shows a 2 × 2 table with the notation which will be used subsequently.

TABLE 1.—*Attribute I*

		Present.	Absent.	Total
Attribute J	Present	o_{ij}	$a_j - o_{ij}$	a_j
	Absent	$a_i - o_{ij}$	$n - a_i - a_j + o_{ij}$	$n - a_j$
	Total	a_i	$n - a_i$	n

This table summarizes the statements that, of n individuals, a_i possess attribute I, a_j attribute J, and o_{ij} both attributes, and shows the logical consequences of these statements.

Such a table is the simplest and most general form in which the association between two attributes can be shown. Tables showing more complex forms of association, such as those used for calculating product moment correlation coefficients, can be reduced to 2 × 2 tables, though not necessarily without some loss of accuracy. A method of factor analysis suitable for matrices of such tables will therefore have the utmost generality, but will not necessarily be the most efficient in particular instances.

The method adopted in the present instance is applied to observations of the occurrence or non-occurrence of symptoms and pairs of symptoms which have been found to differentiate officers who have suffered some form of neurosis from other officers in the forces. The problem to be considered is whether the frequencies with which the pairs of symptoms were observed concomitantly afford evidence of any special syndromes among the symptoms, such as might be used to distinguish different forms of neurosis.

Table 2 gives a descriptive heading for each item (symptom), and shows how often each was observed in a composite group of 256 officers, 201 of whom had received treatment for neurosis. The values of χ^2 given in the final column show to what extent each discriminates the neurotics from the normals. a_i, the number of times item I occurs, = 90, when $I = 1$, 54 when $I = 2$, etc. The total of such numbers, 1,101 in this example, will be indicated as Σa.

Table 3 shows how often each item occurred concomitantly with every other. o_{ij}, the number of times items I and J were observed concomitantly, = 24 when $I = 1$ and $J = 2$ (or *vice versa*), etc. The matrix $[o_{ij}]$ would be shown in full by Table 3 if its lower left-hand half were filled in

TABLE 2.—*Frequency of Occurrence of Individual Symptoms (items)*

Symptom.	(a) Among 201 neurotic officers.	(b) Among 55 normal officers.	(c) Among 256 all cases.	(d)* χ^2.
1. Heredity	83	7	90	14·23
2. Physical ill-health	54	0	54	17·15
3. Neurotic traits in childhood	91	9	100	13·97
4. Former psychiatric illness	74	3	77	18·73
5. Shy, solitary, etc., in childhood	80	6	86	14·89
6. Difficulty in making social contact	66	4	70	12·95
7. Emotional instability	146	5	151	69·48
8. Obsessional features	74	7	81	10·50
9. Apprehensiveness	118	1	119	53·92
10. Dependence	107	2	109	41·44
11. Unstable work record	47	4	51	6·05
12. Marriage or sexual difficulties	68	3	71	15·96
13. Alcoholism	42	0	42	12·27
Total	..	..	1,101	

* Using Yates' correction.

TABLE 3.—*Frequency of Concomitance of Pairs of Symptoms (items)*

Item.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Total of row.
1	—	24	48	32	36	29	68	31	56	48	19	30	17	438
2		—	29	25	31	19	42	20	38	32	16	17	10	279
3			—	48	56	42	78	42	75	60	28	35	18	482
4				—	33	28	63	33	50	57	23	33	21	341
5					—	54	67	34	62	57	26	36	13	349
6						—	55	32	52	47	24	29	12	251
7							—	54	91	82	38	59	37	361
8								—	50	43	16	27	9	145
9									—	72	30	44	24	170
10										—	29	43	20	92
11											—	23	15	38
12												—	17	17
13													—	—
Total of column	—	24	77	105	156	172	373	246	474	498	249	376	213	2,963
Total of row	438	279	482	341	349	251	361	145	170	92	38	17	—	2,963
Total of row plus column	438	303	559	446	505	423	734	391	644	590	287	393	213	5,926

Note.—The sum of the row totals checks with the sum of the column totals.

symmetrically. Σo_i, the total number of times item I was observed in concomitance with any other item, = 438 when $I = 1$, 303 when $I = 2$, etc. Σo, the sum of all the terms in $[o_{ij}]$ = 5,926. The application of factor analysis directly to this matrix is described below. Some reasons for preferring $[o_{ij}]$ to any matrix of derived coefficients are given afterwards.

Assuming no association between items I and J, their expected frequency of concomitance, $e_{ij} = a_i a_j / n$. This can also be written as $n p_i p_j$ where p_i is the probability of finding I in a single case ($= a_i/n$). To test whether o_{ij} differs significantly from e_{ij}, we can calculate

$$\chi^2_{ij} = \frac{(o_{ij} - e_{ij})^2}{e_{ij}} + \frac{(e_{ij} - o_{ij})^2}{a_i - e_{ij}} + \frac{(e_{ij} - o_{ij})^2}{a_j - e_{ij}} + \frac{(o_{ij} - e_{ij})^2}{n - a_i - a_j + e_{ij}} \quad . \quad . \quad (1)$$

with one degree of freedom. Thus all the values of e_{ij} obtainable from the values of a_i and a_j in Table 2 can be compared with the corresponding values of o_{ij} in Table 3, and 78 values of χ^2 will be obtained, each with one degree of freedom. In general, where there are m items, there are $\frac{1}{2}m(m-1)$ independent values of o_{ij} and the same number of values of χ^2 can be obtained

without duplication. On other assumptions other values of e_{ij}, and accordingly of χ^2, can be obtained.

The sum of such values of χ^2_{ij} ($\Sigma\chi^2$) is used here to test the goodness of fit of various hypothetical matrices $[e_{ij}]$ to the observed matrix $[o_{ij}]$. On the assumption of no association $\Sigma\chi^2$ is allotted $\frac{1}{2}\, m(m-1)$ degrees of freedom; and as other hypothetical matrices are obtained by drawing on information contained in the observed matrix $[o_{ij}]$, corresponding reductions are made in the degrees of frêedom allotted to $\Sigma\chi^2$. Similar procedures have been proposed by Kelley[6] and Burt. Burt gives a general indication of the extent to which different hypotheses reduce the degrees of freedom of $\Sigma\chi^2$. There may, however, be some valid objection to summing values of χ^2_{ij}. This is a point on which I would particularly welcome comments.

4. *The Extraction of a Single General Factor*

The assumption of no association is so wildly improbable in this instance that there is no need to compute the value of $\Sigma\chi^2$ for testing it.

The first assumption which deserves serious consideration is that all the items belong to a single general class, and that there is no need to differentiate any subclasses. Accordingly let us suppose that no item has a higher probability of occurring concomitantly with one item than with a second, and let us use p'_i to indicate the general probability that item I will occur concomitantly with any other item J. Then every value of $o_{ij} - e_{ij}$ (where $e_{ij} = a_i a_j/n$) will serve as an estimate of $np'_i p'_j$, and all the observed values of $o_{ij} - e_{ij}$ for a given I can be used to compute an average value of p_i'. After computing p_i' for each item in turn, we can compute the matrix $[e'_{ij}]$, where $e'_{ij} = a_i a_j/n + np'_i p'_j$; and if our assumption is correct, we shall find no significant difference between $[e'_{ij}]$ and $[o_{ij}]$.

For the sake of convenience, the values to compute are those of $p'_i\sqrt{n}$. They can be described as "factor loadings." Table 4 shows the worksheet used for computing them in this instance,

TABLE 4.—*Worksheet Used in Computing* $[p_i'\sqrt{n}]$ *on the Assumption of a Single General Factor*

Item	1. Σo_i.	2. Σe_i.	3. Difference $np'_i(\Sigma p' - p'_i)$	4. Guessed communality.	5. First estimate of $p'_i\sqrt{n}$	6. First estimate of communality.	7	8	9. Third estimate of $p'_i\sqrt{n}$.
1	438	355 4297	82 5703	4 4	2 0748	4 3048	. . .	. . .	2 0738
2	303	220·8516	82 1484	4 3	2 0623	4 2532	. . .	. . .	2 0626
3	559	391 0156	167 9844	21 0	4 5085	20 3263	. . .	. . .	4 4925
4	446	308 0000	138 0000	13 0	3·6023	12 9766	. . .	. . .	3 6044
5	505	340 9766	164 0234	19 0	4 3663	19·0642	. . .	. . .	4 3716
6	423	281 9141	141·0859	14 0	3 6998	13 6883	. . .	. . .	3 6939
7	734	560 3516	173 6484	22 0	4 6674	21 7850	. . .	. . .	4·6649
8	391	322 7344	68 2656	2·9	1 6977	2·8823	. . .	. . .	1·6985
9	644	456 4766	187 5234	26·0	5 0939	25·9476	. . .	. . .	5 0965
10	590	422·3750	167·6250	21 0	4 4999	20 2490	. . .	. . .	4 4817
11	287	209·1797	77 8203	3·8	1·9472	3·7914	. . .	. . .	1·9483
12	393	285 6641	107·3359	7·5	2 7396	7·5052	. . .	. . .	2·7417
13	213	173·7422	39·2578	9	·9580	·9178	. . .	. . .	9591
	5,926	4,328 7112	1,597 2888	159 8	41 9177	157 6917	. . .	. . .	41·8895
Estimate of $\Sigma p'\sqrt{n}$			41 9176			41 8925			

Note.—Computations taken to 6 decimal places, reproduced here to 4.

and the final values obtained. The following notes explain the entries in the columns. Numbers refer to columns.

1. Values of Σo_i are taken from the bottom row of Table 3.
2. Σe_i is used to indicate the sum of all values of $a_i a_j/n$ for a given item. It is computed as $a_i(\Sigma a - a_i)/n$, from the appropriate entries in Table 2.
3. On our assumption, the difference between Σo_i and Σe_i, shown in this column, is equal to $np'_i(\Sigma p' - p'_i)$, the sum of all values of $np'_i p'_j$ for a given item. (In this notation $\Sigma p'$ indicates the sum of all values of p', including p'_i.)

The total of the column is equal to

$$n(\Sigma p')^2 - n\{(p'_1)^2 + (p'_2)^2 + \ldots (p'_i)^2 + \ldots (p'_m)^2\}$$

i.e. to the sum of all the terms in the matrix $n[p'_i p'_j]$ under the condition, as before, that $i \neq j$.

4. A set of arbitrary values are entered in this column, which are guesses at the correct value of $n(p'_i)^2$. Their customary designation is "guessed communalities."

If the guesses are correct, the term for item I in this column, plus the corresponding term in the previous column, is equal to $np'_i\Sigma p'$, and the total of this column plus the total of the previous column is equal to $n(\Sigma p')^2$. Its square root is $\Sigma p'\sqrt{n}$.

If the guesses are incorrect, the estimate of $np'_i\Sigma p'$ obtained from this column and the previous one, when divided by the estimate of $\Sigma p'\sqrt{n}$, will give a closer approximation to the correct value of $p'_i\sqrt{n}$ than that implied by the guessed communality.

5. This argument is used to compute the terms in this column, which are first approximations to the correct values of $p_i\sqrt{n}$. The estimate of $\Sigma p'\sqrt{n}$ is first calculated, then its reciprocal, and finally, the products of this reciprocal with the successive estimates of $np'_i\Sigma p'$.

The sum of the column can be checked against the estimate of $\Sigma p'\sqrt{n}$, with which it should agree.

6. These are the squares of the terms in the previous column.

7 and 8. The terms in column 7 are obtained from the entries in columns 3 and 6 by the same procedure as that used to obtain the terms in column 6 from entries in columns 3 and 4. The squares of the terms in column 7 are entered in column 8. The two columns thus give second estimates of the factor loadings and the communalities.

9. The third estimate of the factor loadings, shown in this column, which have been obtained by applying the same procedure to the entries in columns 3 and 8, are sufficiently close to the estimates that would be reached after further repetitions of the same procedure to be used as final estimates in this instance.

The final estimates of $p'_i\sqrt{n}$ form a column matrix which can be treated exactly like a set of factor loadings. They can be used to calculate values of e'_{ij} and hence of $o_{ij} - e'_{ij}$, the differences shown in Table 5.

TABLE 5.—*Differences Between the Observed Frequencies (shown in Table 3) and the Frequencies Expected on the Assumption of a Single General Factor*

Item.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12	13
1 .	0 74	3·53	-2·55	−3·30	3·27	5·24	−1·00	3·59	0·39	2·97	−0·65	0·25
2 .		- 1·36	1·32	3·84	3·38	0 53	−0·59	2·39	0·24	1 22	−3·63	−0·84
3 .			1 73	2 77	-1·94	−1·94	2·73	5 62	−2·71	−0·67	−5·05	−2·71
4 .				- 8 63	- 6·37	0 77	2·51	−4·16	8·06	0·64	1·76	4 91
5 .					14·34	−4 12	− 0 64	−0 26	0 79	0·35	0·16	−5·30
6 .						3 52	3·58	0·63	0·64	2·86	−0·54	−3 03
7 .							−1·70	−2·97	−3·20	−1·17	4·33	7·73
8 .								3·69	0·90	−3·45	−0·12	−5·92
9 .									−1·51	−3·64	−2·98	−0·41
10 .										- 1 45	0·48	- 2·18
11 .											3·51	4·76
12 .												2·72

Each is an average value. The method of computation ensures, and Table 5 can be used to confirm, that the sum of the deviations from each average is nil, i.e. that $\Sigma o_i - \Sigma e'_i = 0$ within the limits implied by the number of decimal places to which the computations have been carried. This also shows that one degree of freedom is used in calculating $p'_i\sqrt{n}$; that the total loss is equivalent to eliminating any one row and corresponding column from the matrix $[o_{ij}]$; and that therefore, when χ^2_{ij} is calculated by applying formula (1) with e'_{ij} substituted for e_{ij}, the number of degrees of freedom of the resulting $\Sigma\chi^2$ will be $\frac{1}{2}(m - 1)(m - 2)$. This has already been pointed out by Burt.

Thurstone's method has been used here because, when applied with successive approximations, it yields values of e'_{ij} which satisfy this condition. (Usually the analysis is applied to $[r_{ij}]$ and the condition satisfied would be written analogously as $\Sigma r_i - \Sigma e'_i = 0$.) Other methods do not necessarily do so. But in every case when a general factor is extracted, one loading is obtained for each item, and therefore the number of degrees of freedom lost is equal to the number lost by eliminating one row and corresponding column from the matrix analysed.

Table 6 shows the values of χ^2_{ij} calculated to test how well our assumption fits the observations. Table 7 illustrates the worksheet used in calculating them. The following notes explain working methods and checks:

TABLE 6.—*Values of* χ^2 *Used for Testing the Significance of the Differences Given in Table 5*

Item.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Total of row.
1	—	·05	·89	·51	·82	·88	2·11	·08	·91	·01	·90	·34	·01	7·52
2		—	·18	·18	1·45	1·19	·03	·04	·59	·01	·19	1 38	·11	5·33
3			—	·24	·60	·32	·33	·56	2·42	·54	·05	2·09	·85	8·00
4				—	6·12	3 55	·06	·52	1·50	5 27	04	·27	2·98	20·32
5					—	17 95	1·78	·03	·01	·05	·01	·00	3·33	23·16
6						—	1·56	1·10	·04	·04	·88	·03	1·17	4·81
7							—	·23	·71	·83	·18	1·90	7·87	11·72
8								—	1·01	·06	1·26	·00	4 41	6·74
9									—	17	1·42	·76	02	2·37
10										—	21	·02	·55	·78
11											—	1·35	3 62	4·96
12												—	·96	·96
13													—	—
Total of column	—	05	1 07	·93	8·99	23 90	5·88	2 55	7·18	6 97	5 14	8 14	25 88	96 68
Total of row	7 52	5·33	8·00	20·32	23·16	4 81	11·72	6 74	2 37	·78	4 96	·96	–	96·68
Total of row plus column	7 52	5·39	9·07	21·25	32·15	28·71	17 60	9·29	9·55	7 75	10 11	9 10	25·88	193·36

Note.—Computations taken to 4 decimal places, reproduced here to 2.

TABLE 7.—*Worksheet Used in Computing* χ^2_{ij} *on the Assumption of a Single General Factor*

Paired Items	1. e_{ij}	2. $np'_ip'_j$	3. e'_{ij}	4. $a_i - e'_{ij}$	5. $a_j - e'_{ij}$	6. $n - a_i - a_j + e'_{ij}$	7. $o_{ij} - e'_{ij}$	8. $(o_{ij} - e'_{ij})^2$	9. χ^2_{ij}
1, 2	18 9844	4 2775	23·2619	66 7381	30·7381	135 2619	0·7381	0·5448	0 0533
1, 3	35·1563	9 3156	44 4719	45·5281	55·5281	110 4719	3 5281	12·4475	0·8901
1, 4	27 0703	7·4752	34 5455	55·4545	42·4545	123·5455	−2·5455	6·4796	0·5095
1, 5	30 2344	9·0664	39·3008	50 6992	46·6992	119·3008	−3·3008	10·8953	0·8168
1, 6	24·6094	7 6603	32·2697	57·7303	37·7303	128 2697	3·2697	10·6909	0·8832
1, 7	53·0859	9·6742	62·7601	27·2399	88·2399	77·7601	5·2399	27 4566	2·1097
1, 8	28·4765	3·5225	31·9990	58·0010	49·0010	116 9990	−0 9990	0·9980	0·0773
1, 9	41·8359	10 5697	52·4056	37·5944	66·5944	99 4056	3 5944	12·9197	0·9142
1, 10	38·3203	9·2930	47·6133	42·3867	61·3867	104·6133	0·3867	0 1495	0·0105
1, 11	17 9297	4·0406	21 9703	68 0297	29·0297	136·9703	−2 9703	8 8227	0·8996
1, 12	24 9609	5·6861	30·6470	59 3530	40 3530	125 6470	−0·6470	0 4186	0·3442
1, 13	14 7656	1·9891	16·7547	73·2453	25 2453	140·7547	0·2453	0·0602	0 0072
Total	355 4296	82 5702	437 9998	642·0002	573·0002	1,418·9998	0·0002	..	7·5156

Note.—Computations taken to 4 decimal places, as shown here.

1. First note down $\Sigma a - a_1$. Then calculate a_1/n. Multiply this successively by $a_2, a_3 \ldots a_m$, and finally by $(\Sigma a - a_1)$. The sum of the previous products should check with this. When item 2 is considered, begin by subtracting a_2 from $\Sigma a - a_1$, and note this down.

2. Similarly note down $\Sigma p'\sqrt{n} - p'_1\sqrt{n}$. Then enter $p'_1\sqrt{n}$ on the machine, calculate products and check as for column 1. For item 2, use $\Sigma p'\sqrt{n} - p'_1\sqrt{n} - p'_2\sqrt{n}$, etc.
3. The total of this column is equal to the sum of the two previous totals.
4. The totals of columns 3 and 4, added together, $= (m - 1)a_1$; when item 2 is considered, $= (m - 2)a_2$, etc.
5. The totals of columns 3 and 5, added together, $= \Sigma a - a_1$; when item 2 is considered, $= \Sigma a - a_1 - a_2$, etc., as used in checking column 1.
6. The totals of columns 3, 4, 5 and 6, added together, $= n(m - 1)$; when item 2 is considered, $= n(m - 2)$, etc.
7. The total of this column is equal to the row total for item 1 (cf. Table 3) minus the total of column 3. Use other row totals for other items correspondingly.

An alternative method for computing χ^2, given by Lawley[8], can be adapted for testing the significance of the differences $o_{ij} - e_{ij}$ and should give approximately the same results, with possibly less labour.

The fit is unsatisfactory. $\Sigma\chi^2 (= 96 \cdot 68)$ is disturbingly large for 66 degrees of freedom. Separate sums of χ^2_{ij} for $I = 4, 5, 6$ and 13 are also disturbingly large for the 11 degrees of freedom they may each be allowed; and two component values, where $i = 5$, $j = 6$, and where $i = 7$, $j = 13$, are outside the normal range of χ^2 with one degree of freedom.

In conclusion, the assumption that all the items belong to a single general class and that there is no need to differentiate any subclasses fails to account for the exceptionally close association between items 5 and 6, or for the association between items 7 and 13, it also provides an inadequate account of the associations between item 4 and other items, and gives a generally bad fit. It should be rejected.

5. *The Extraction of a Modified General Factor and Two Group Factors*

Thurstone's method could be used to continue the analysis. The matrix $[o_{ij} - e'_{ij}]$ would be written in full by inserting the symmetrical half in Table 5. Rows and corresponding columns in it would be reflected in sign until the number of positive signs was maximized. From the resulting matrix a second general factor would be extracted. Further factors could be obtained by further repetitions of this procedure. Each repetition would ensure that the differences between the observed and the expected frequencies $= 0$ when summed in a fresh way. Consequently the degrees of freedom contributed by one row and corresponding column of the matrix of observations will be sacrificed each time, and after x factors have been extracted $\Sigma\chi^2$ will be left with $\frac{1}{2}(m - x)(m - x - 1)$ df.–cf. Burt (*op. cit.*).

As the terms in the matrix used in extracting the second factor are deviations from the averages computed when the first factor is extracted, the loadings obtained are average deviations. Those obtained from reflected rows are given negative signs, so generally about half the loadings are positive, and half negative. Each further column of loadings possesses similar characteristics.

Thurstone and his followers have provided methods of "rotating axes," i.e. of finding trigonometric transformations of the loadings in these columns. The positive and negative signs and the size of the loadings in different columns (provided there are two or more) can be altered in an infinite number of ways without changing the expected values in the matrix which is their end product. These methods demonstrate that any expected matrix can be derived from an infinite number of different assumptions. They are used, however, to find preferred solutions in which the number of zero or near-zero loadings is maximized and the number of negative loadings is minimized. Burt gives reasons for preferring not to rotate axes, and attributes the columns of half positive, half negative loadings to "bi-polar" factors. After rotation, conditions of the kind $\Sigma o_i - \Sigma e'_i = 0$ are not necessarily satisfied by the loadings in any single column.

If factor analysis is used for classificatory purposes, this method is an inconvenient one. The extraction of one factor is relevant for considering whether the variables or attributes fall into a single class; two factors for considering whether they fall into two approximately balanced classes; x for 2^{x-1} classes, where x can only be a positive integer. There are thus a large number of hypothetical systems of classification to which Thurstone's method is inapplicable without trigonometric transformations. Other methods, e.g. Burt's group-factor method or Holzinger's

modification of Spearman's, can be used to fill this gap, but may leave the same condition unsatisfied.

In the present instance, the analysis in section 3 shows that the items cannot be treated, without qualification, as belonging to a single general class. Each seems to have a general tendency to occur concomitantly with every other, but some items appear specially frequently in conjunction with other particular items. Such items may be supposed to fall into one or more sub-classes within the general class. For any such item, Σo_i may be partitioned into, say, $\Sigma o_{ig} + \Sigma o_{is}$, where s indicates any other item in the same sub-class as i, and g any of the remaining items. Using p'_i to indicate the probability that item I will occur concomitantly with any other g item, and p''_i to indicate its additional probability of occurring with any other s item, we can compute an average value of $p'\sqrt{}_i n$ which will satisfy the condition $\Sigma o_{ig} - np'_i(\Sigma p' - \Sigma p'_s) = 0$, where $\Sigma p'$ as before indicates the sum of all values of p'_i, and $\Sigma p'_s$ indicates the sum for all items in the same sub-class as I (including I). We can also compute an average value of $p''_i\sqrt{n}$ such that $\Sigma o_{is} - np'_i(\Sigma p'_s - p'_i) - np''_i(\Sigma p''_s - p''_i) = 0$. The procedure used in the previous section can be adapted to these computations. It could be adapted to deal with a great variety of more complicated assumptions, if necessary.

Items 4, 5, 6, 7 and 13 are the ones which, on the evidence of Table 6, appear to need special sub-classification. Items 5 and 6 are specially closely associated with one another, but not with the remaining three. They may therefore be assigned a subclass by themselves. Item 13 is relatively closely associated with items 4 and 7, so these three may be assigned a second sub-class.

On these assumptions the values of o_{ij} shown in Table 8

TABLE 8

Pair of Items.	o_{ij}
4, 7	63
4, 13	21
5, 6	54
7, 13	37
Total . . .	175

must be omitted from the matrix $[o_{ij}]$ when the values of $p'_i\sqrt{n}$ are computed. Table 9 shows the worksheet used and the final values of $p'_i\sqrt{n}$ obtained. Working methods are similar to those for Table 4, but the following notes may be useful. Numbers refer to columns.

TABLE 9.—*Worksheet for Computing* $[p'_i\sqrt{n}]$ *Omitting Certain Values of* o_{ij}

Item.	1. Σo_{ig}	2. Σe_{ig}	3. Obs. covariances.	4. First est. comm. + cov.	5. First est. loading.	6. Second est. comm. + cov.	Final est. loading.
1	438	355 4297	82 5703	4·3007	2 1074	4·4411	2 1262
2	303	220 8516	82·1484	4 2545	2 0961	4·3936	2 1147
3	559	391·0156	167·9844	20 1782	4 5647	20·8365	4 6227
4	362	249·9492	112 0508	33 2649	3 5253	30·7639	3 4460
5	451	317·4609	133·5391	35·2619	4 0950	30·7162	3 9601
6	369	258 3984	110 6016	29 7929	3 4059	25·5473	3 2799
7	634	490·1602	143·8398	43·0506	4·5338	39·5647	4·4236
8	391	322·7344	68 2656	2·8850	1 7261	2·9794	1·7405
9	644	456·4766	187·5234	25·9764	5·1794	26·8262	5 2512
10	590	422·3750	167·6250	20 0804	4 5536	20·7353	4·6114
11	287	209·1797	77 8203	3·7962	1·9800	3 9204	1·9972
12	393	285·6641	107 3359	7·5176	2·7863	7·7635	2·8137
13	155	136·3359	18·6641	8·8515	·6675	5·8250	·5740
	5,576	4,116 0313	1,459·9687	239·2108	41·2211	224·3141	40·9612

1. Σo_{ig} is obtained from Table 3, omitting the values listed in Table 8.

2. When I is a specially subclassified item, its correction term is $a_i(\Sigma a - \Sigma a_s)/n$, where Σa_s is the sum of the frequencies of occurrence of all items in the subclass including I.

After $(\Sigma a - \Sigma a_s)/n$ has been computed, the correction terms for all items in the subclass can be obtained by multiplying it successively by each appropriate value of a_i.

3. When I is a specially subclassified item, the estimate needed for this column is that of $np'_i\Sigma p'_s$. $\Sigma p'_s\sqrt{n}$ is estimated by taking values from the last column of Table 4, and multiplied successively by the appropriate values of $p'\sqrt{n}$ given there. The entries in columns 3 and 4, added together, thus yield estimates of $np'_i\Sigma p'$ and $n(\Sigma p')^2$ as before, the only difference being that the values of o_{ij} listed in Table 8 have been left out of account.

Values of $p''_i\sqrt{n}$ have now to be found for items 4, 5, 6, 7 and 13, from equations of the form

$$np''_ip''_j = o_{ij} - a_ia_j/n - np'_ip'_j.$$

For items 5 and 6 there is only one such equation,

$$np''_5p''_6 = 14\cdot4864,$$

so p''_5 and p''_6 are indeterminate. In the final table of factor loadings (Table 10) $p''_5\sqrt{n}$ has been put, for convenience, equal to $p''_6\sqrt{n} = 3\ 8061$.

TABLE 10.—*Factor Loadings Obtained on the Assumption of a Modified General Factor and Two Group Factors*

Item.	Values of $p'_i\sqrt{n}$ for the general factor (g).	Values of $p''_i\sqrt{n}$ for the first group factor (S_1).	Values of $p''_i\sqrt{n}$ for the second group factor (S_2).
1	2 1262	..	..
2	2 1147	..	..
3	4·6227	..	..
4	3·4460	..	1 2419
5	3·9601	3·8061	..
6	3 2799	3·8061	..
7	4·4236	..	1 8829
8	1·7405	..	..
9	5·2512	..	..
10	4·6114	..	..
11	1 9972	..	..
12	2·8137	..	..
13	·5740	..	5·1449

Note.—Computations taken to 6 decimal places, reproduced here to 4.
$n = 256.$ $\sqrt{n} = 16.$

For items 4, 7 and 13 there are three equations,

$$np''_4p''_7 = 2\cdot3383$$
$$np''_4p''_{13} = 6\cdot3892$$
$$\text{and}\quad np''_7p''_{13} = 9\cdot6874$$

from which the final loadings can be obtained directly, using the algebraic method employed by Spearman, viz.:

$$p''_4\sqrt{n} = \left(\frac{2\cdot3383 \times 6\cdot3892}{9\cdot6874}\right)^{\frac{1}{2}} = 1\ 2419$$

etc. These too are entered in Table 10.

Table 10 therefore gives the complete set of factor loadings derived from the hypothesis adopted in this section. The two sets of values of $p''_i\sqrt{n}$ are of equal rank, since one does not have to be determined before the other, so the same notation is used for both. But they are listed as separate columns, to show that they are mutually exclusive. Using $[g]$, $[s_1]$ and $[s_2]$ to indicate the three columns of loadings, the expected frequencies of concomitance can be calculated as

$$[e''_{ij}] = [g]\,[g]' + [s_1]\,[s_1]' + [s_2]\,[s_2]'$$

under the condition $i \neq j$. The differences between the observed frequencies and these expectations are listed in Table 11. Values of χ^2_{ij}, calculated as before, are shown in Table 12. Table 13

TABLE 11.—*Differences Between the Observed Frequencies (Shown in Table 3) and the Frequencies Expected on the Assumption of a Modified General Factor and Two Group Factors*

Item.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.
1 .	0·52	3·02	−2·40	−2·65	−2·58	5·51	−1·18	3·00	−0·12	−3·18	−0·94	1·01
2 .		−1 87	1·47	4·48	−2·70	0·79	−0·77	1·79	−0·74	1·02	−3·93	0·07
3 .			1·99	4·10	−0·51	−1·43	2·31	4·24	−3·90	−1·15	−5·74	−1·06
4 .				−6·51	−4·36	0·00	2·64	−3·89	8·32	0·78	1·95	0·00
5 .					0·00	−1·24	−0·10	1·23	2·12	0·96	1·01	−3·38
6 .						−0·80	4·14	2·24	2·07	3·50	0·36	−1·37
7 .							−1·48	−2·42	−2·69	−0·92	4·67	0·00
8 .								3 21	0 49	−3·61	−0·36	−5·29
9 .									−2 88	−4·20	−3·78	1·46
10 .										−1·92	0·21	−0·53
11 .											3·24	5·49
12 .												3·74

TABLE 12.—*Value of* χ^2 *Used for Testing the Significance of the Differences Given in Table* 11

Item	1.	2.	3.	4.	5.	6.	7.	8.	9.	10	11.	12.	13.	Total of row.
1 .	—	03	·65	·45	·53	·55	2 32	11	·64	·00	1·03	·07	·13 .	6 50
2 .		—	·34	·22	1·97	·76	·08	·06	·34	05	·13	1·61	·00 .	5·56
3 .			—	·32	1·30	·02	·18	·40	1 41	1·13	14	2·72	·13 .	7·75
4 .				—	3·44	1·65	·00	·57	1·30	5·60	·06	·33	00 .	12 95
5 .					—	·00	·15	·00	·12	·35	09	·08	1·39 .	2 19
6 .						—	·07	1 48	·46	·37	1·33	·01	25 .	3·97
7 .							—	·18	47	58	·11	2·19	·00 .	3·52
8 .								—	·76	·02	1·38	·01	3 58 .	5·75
9 .									—	·62	1·92	1 24	·25 .	4·02
10 .										—	38	·00	·03 .	41
11 .											—	1·14	4·99 .	6·13
12 .												-	1·87 .	1·87
13 .													— .	—
Total of column .	—	·03	·99	·99	7 24	2·98	2 80	2·79	5 50	8 73	6 57	9·40	12·61 .	60 63
Total of row .	6 50	5 56	7 75	12 95	2 19	3 97	3·52	5 75	4 02	41	6 13	1 87	— .	60·63
Total of row plus column .	6 50	5 59	8 74	13 94	9 43	6 95	6 31	8 54	9·53	9·14	12 69	11·27	12 61 .	121 25

TABLE 13.—*Degrees of Freedom*

Number of item.	(*a*) Originally available.	(*b*) Exhausted by hypothesis.	(*c*) Finally available.
4 .	12 .	2 .	10
5 .	11 .	2 .	9
6 .	10 .	2 .	8
7 .	9 .	2 .	7
13 .	8 .	2 .	6
1 .	7 .	1 .	6
2 .	6 .	1 .	5
3 .	5 .	1 .	4
8 .	4 .	1 .	3
9 .	3 .	1 .	2
10 .	2 .	1 .	1
11 .	1 .	1 .	0
12 .	0 .	1 .	0
Total .	78 .	n.a. .	61

n.a. = not applicable.

shows the working method used for counting the degrees of freedom for $\Sigma\chi^2$. In the list of items, those from which more degrees of freedom have been exhausted take precedence over others. Column (*a*) shows the number of degrees of freedom yielded by each row of the matrix of frequencies when the items are arranged in the order shown. Column (*b*) shows the number of sub-totals of Σo_i used in computing the factor loadings for each item. Column (*c*) shows the number of degrees of freedom left, i.e. the differences between corresponding entries in columns (*a*) and (*b*) excluding negative differences.

The amount of $\Sigma\chi^2$ obtained from Table 12 accords with the amount expected by chance, so the hypothesis fits the observations as a whole well enough. The sums of χ^2_{ij} for each item are also amounts within the range of chance, allowing 10 degrees of freedom each to those for items 4, 5, 6, 7 and 13, and 11 each for the remainder. The highest single value of χ^2_{ij}, that for items 4 and 10, is no greater than might be expected to occur by chance in a table containing 74 values, each of which, considered independently of the others, may be allowed one degree of freedom.

It is therefore concluded that the hypothesis considered in this section provides a satisfactory fit to the observations. All the items, it seems, can be treated as belonging to a single class; but two sub-classes need to be differentiated, the first containing items 5 and 6 only, the second, items 4, 7 and 13.

6. *Interpretation and Validation of the Results*

The psychological discussion of these results is intended for publication elsewhere, but some comment may be desired here to provide a happy ending.

The psychological resemblance between items 5 and 6 is evident. In the descriptive account furnished with the records, I find the almost apologetic comment: "Analysing our case histories, it did seem worth while to draw a distinction between (them) because one quarter of our neurotic cases appear to have overcome the adolescent shyness when they reached adult life." No specially close psychological resemblance was suggested between items 4, 7 and 13. But if men prone to neurosis are divided into those who are liable to phases of depression or other periodic reductions in their resistance, and those of a permanently inadequate type, we may consider items 4, 7 and 13 pertinent in differentiating the former from the latter. Thus the classification provided by the statistical analysis is psychologically reasonable.

Following the method used in the two previous sections, we may consider what is the cumulative value of the information as a means of differentiating neurotics from normals, firstly when an equal weight is assigned to all the items, and secondly, when items in the same sub-class are weighted equally but each sub-class is allowed a separate weight. In the first case, we may analyse the total variance of the number of items per person; we find then that 35 per cent. is accounted for by the difference between the neurotic group and the control. In the second, we may separate the total number of items per person into sub-totals for each of the three sub-classes: A, items 5 and 6; B, items 4, 7 and 13; and C, all others. Treating each sub-total as a separate variable, we may calculate the discriminant functions for the same purpose. The appropriate weights are then found to be in the proportion,

Items in sub-class	A,	−	0585
,, ,,	B,	1	4850
,, ,,	C,	1	0000

and 45 per cent. of the total variance of the weighted sum per person is attributable to the difference between the two samples. Thus the classification reached in Section 4 is a useful one.

7. *Notes*

i. *The analysis of derived coefficients* (*tetrachoric correlations, etc.*).—When factor analysis is applied to data collected in the form of matrices of 2×2 tables, the observations for which the preferred hypothesis should account are the observed frequencies of concomitance. Any method of factor analysis may be applied, as here, directly to these observed frequencies, or to functions of them, such as four-point correlation coefficients or tetrachorics. But if it is applied

to any derived function, two extra stages of work are likely to be involved; firstly, deriving the functions for analysis, and secondly, reconstructing the matrix of expected frequencies for comparison with the observed frequencies. Apart from the risk of error introduced by unnecessary computations, the expected frequencies are likely to differ from the observed more widely if these two stages are introduced. For instance, if tetrachoric correlations had been used here, a matrix of them, say $[t_{ij}]$, would need to be calculated from $[o_{ij}]$. Let $[l_1]$ be the column matrix of the first factor loadings obtained, as in section 3, from $[t_{ij}]$, corresponding with the entries in the final column of Table 4. Then the sum of every row and every column in the matrix of differences $[t_{ij}]-[l_1]\,[l_1]'$ will be zero under the condition $i \neq j$. But the values of e'_{ij} reconstructed from $[l_1]\,[l_1]'$ will not necessarily be such that the matrix of differences $[o_{ij}]-[e'_{ij}]$ will satisfy the same conditions. But these are the differences which should be considered in testing goodness of fit. So the hypothesis may be more badly fitted to the observations than it need be. To these general objections to using any derived function whatever must be added any objection to the use of any particular derived function.

The product moment correlation between two continuous, approximately normally distributed variables can generally be estimated within the limits of its error variance by tetrachoric correlation, provided that the dichotomies are taken near the means and the number of cases is not small (say, not under 100). But it is when the number is as large as several thousand, and when facilities for tabulation and computation are limited and the need for information is urgent, that tetrachoric correlations prove most useful. In this instance the number of cases may not be so small as to preclude the use of tetrachorics, but there are no good reasons for using them. The reasons against are strong. The gravest, it seems to me, is that the definition of many of the items is dependent on the point at which the dichotomy occurs. Thus the enquiries about "heredity" (item 1) took only parents and sibs into account. A graduated scale for measuring variations in hereditary predisposition to neurosis would need to be based on more exhaustive enquiries, and would thus lead to a re-definition of " heredity." The use of tetrachorics here thus involves us in speculations on what inferences might have been drawn if the same data had been obtained by different methods of observation, when we have good reasons to suppose that different methods would have yielded different data.

ii. *Analogies between factor analysis and analysis of variance.*—The similarity between a factor loading and an average has, I hope, been shown in the above account. Other similarities between factor analysis and analysis of variance are mentioned here in the hope that they may suggest other approaches to the problem of testing goodness of fit and significance of factor loadings. They are even more apparent when continuous variables are considered in place of dichotomous attributes.

Let us therefore suppose that n individuals have been measured in m variables. Let I and J be any two variables. Let V_i indicate the total variance of the measurements in I, V_j that in J and W_{ij} their total covariance. Factor analysis may be applied to the matrix $[W_{ij}]$ or to matrices of derived coefficients. Usually it is applied to $\left[\frac{W_{ij}}{\sqrt{V_i V_j}}\right] = [r_{ij}]$, i.e. the matrix of mean covariances when each variable has been weighted by the reciprocal of its standard deviation.

In $[W_{ij}]$ the terms in the leading diagonal (where $i = j$) are omitted. All methods of factor analysis can be described as methods for interpolating a set of terms in this leading diagonal which conform with certain requirements and are described as communalities. The total communality of I is the sum of the squares of its factor loadings (the factor loadings being independent of one another). It can be discussed as a proportion of the total variance of I. As the number of factor loadings attributed to I increases, this proportion tends to increase. It might be possible to consider whether the increase due to the extraction of an additional factor is significant.

Again, let—

ΣW = the sum of all the terms in $[W_{ij}]$,

ΣV = the sum of V_i over all values of I,

V_t = the total variance of the n totals obtained by summing all the m measurements on each individual;

then $\Sigma W + \Sigma V = V_t$, and the sum of all the terms in all the matrices

$$[l_1]\ [l_1]' + [l_2]\ [l_2]' + \quad . \quad . \quad .$$
(not subject to the condition $i \neq j$)

can be discussed as a proportion of V_t. The proportion tends to increase as the number of factors increases, and the significance of such increases might be considered.

If the analysis is applied to $[r_{ij}]$, let Σr = the sum of all its terms. Then $\Sigma r + m$ = the mean square variance of the totals obtained by summing all the m measurements on each individual after each variable has been weighted by the reciprocal of its standard deviation. Therefore the factor analysis can still be considered as an analysis of the variance of a definable composite variable.

However, certain difficulties are encountered when we explore this approach further. Returning to the analysis of $[W_{ij}]$, let

Σl_1 = the sum of all the terms in the matrix $[l_1]\ [l_1]'$ not subject to the condition $i \neq j$.
Σc_1 = the sum of its communalities, i.e. of the terms in its leading diagonal, where $i = j$, and let
Σl_2, Σc_2, etc., be defined similarly.

Then the ratio $\Sigma l_1 / V_t$ varies with the method used for computing the first factor loadings. When Thurstone's method is used, as applied in section 3 and described at the beginning of section 4, before rotation $\Sigma l_1 = \Sigma c_1 = \Sigma W$, but $\Sigma l_2 = \Sigma c_2 = 0$, etc. The proportions $\Sigma l_2 / V_t$, etc., therefore do not seem comparable with $\Sigma l_1 / V_t$. This seems likely to be the case, also, if after one general factor the remaining factors extracted are group factors.

Again, let us suppose that two groups of people, X and Y, have been measured in the same m variables, and that two matrices of covariances, $[X_{ij}]$ and $[Y_{ij}]$, have been computed. We may wish to consider whether the first factor loadings obtained from $[X_{ij}]$ differ from those obtained from $[Y_{ij}]$. Following the suggested approach, we might put—

$$[X_{ij}] + [Y_{ij}] = [W_{ij}]$$

(so that V_t becomes the total variance of the composite variables within groups) and make a factor analysis of all three matrices, thus obtaining three sets of loadings, say $[l_x]$, $[l_y]$ and $[l_1]$. We might expect that if $[l_x]$ differs significantly from $[l_y]$, $\Sigma l_x + \Sigma l_y$ will form a noticeably larger proportion of V_t than Σl_1. But in an unpublished experiment of this kind I found that Σl_1 exceeded $\Sigma l_x + \Sigma l_y$.

I conclude that there are pitfalls in this approach to the problem, but I am unable to say whether it should therefore be abandoned. Burt's discussion of this subject should be noted.

iii. *Conditions affecting the definition of factors.*—In the above experiment the matrix analysed was $[o_{ij}] - [e_{ij}]$ where $e_{ij} = a_i a_j / n$, a single correction term which does not allow for the fact that the n cases are drawn from two groups. For instance, $e_{1.2} = 90 \times 54/256$ (data from Table 2). The matrix used is therefore that of the total covariances of the items. Why was the analysis not applied to the matrix of covariances within groups? Why was e_{ij} not taken as the sum of two correction terms, one for each group? Why, for instance, was $e_{1.2}$ not taken as $83 \times 54/201 + 7 \times 0/55$? Let us consider how the adopted treatment affects the definition of the general factor, and how its definition would be affected by this alternative treatment.

Each of the items was selected for inclusion in the matrix because it was more frequently observed among the neurotic officers than among the normal. Consequently, when the frequencies of concomitance were considered, a general tendency towards positive association could be expected to occur. This can be attributed to differences in the men's degree of propensity to neurosis.

If the matrix analysed had been that of the covariances within groups, the same general tendency might still be expected to appear, because the degree of propensity must vary to some extent among different members of the same group, and the items can be expected to be sensitive to such variations. But the general tendency would be much less marked; the general factor loadings would be smaller and the grounds for defining the general factor would be less secure.

When we consider to what an extent the results of this analysis might have been modified by varying either the items selected for inclusion or the composition of the group under observation,

we cannot help doubting, I feel, whether any factor obtained from any matrix of covariances or dependent functions of covariances can be defined without reference to both. But this leads us to doubt whether the general factor discovered under one set of conditions can be identified with that discovered under another. That is to doubt a proposition which has been used as a premiss in many psychological arguments.

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FACTORIAL EXPERIMENTS DERIVABLE FROM COMBINATORIAL ARRANGEMENTS OF ARRAYS

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1. *Introduction.*

IN a paper (Rao, 1946) the author defined certain configurations of arrays called *hypercubes of strength "d"* and applied them in the construction of confounded designs for factorial experiments. These hypercubes can be constructed with the help of configurations of points and planes in finite Projective and Euclidean geometries. It has been shown that—

(i) a system of confounded designs involving the maximum number of factors and preserving main effects and interactions up to the order $(d - 1)$ can be constructed in the case of a symmetrical factorial experiment when a hypercube of strength "d" exists; and

(ii) hypercubes of strength 2 supply confounded designs for asymmetrical factorial experiments defined by Nair and Rao (1941, 1942*a*, 1942*b*).

In this paper the general configuration of *arrays of strength "d"* which supply basic combinatorial arrangements leading to designs for factorial experiments involving simple analysis of results have been defined and some methods of construction discussed. Some of the problems considered are the constructions of-

(*a*) *multifactorial designs* similar to those of Plackett and Burman (1946), but leading to the estimation of main effects and interactions up to the order k when interactions of order equal to and greater than $d(> k)$ are absent,

(*b*) block designs for symmetrical factorial experiments involving only a subset of treatment combinations and preserving main effects and interactions up to a given order when higher order interactions are absent, and

(*c*) a new series of asymmetrical factorial designs derivable from arrays of strength 2.

This method leads to possible arrangements of multifactorial designs of the type introduced by Plackett and Burman (1946) when the number of levels need not necessarily be a prime or a prime power. For instance, in the case of factors each at 6 levels a multifactorial design exists with 36 *assemblies* involving the maximum of 3 factors, and in the case of factors each at 12 levels a multifactorial design with 144 assemblies can include at least 5 factors.

The existence of block designs leads to arrangements of fractional replication in the case of symmetrical factorial experiments. They give rise to multifactorial designs arranged in blocks so that a source of variation affecting groups of treatments can be eliminated. The general theory of fractional replication is treated in a separate communication where the nature of arrangements introduced here is studied from the point of view of algebraic groups.

2. *Arrays of Strength* d

Let there be n factors $A_1, A_2, \ldots . A_n$, each of which can assume s values, those corresponding to the i-th factor being represented by $i_1, i_2, \ldots i_s$. An ordered set

$$1_a, 2_b \ldots n_k$$

may be called a combination or an assembly. This assembly can, without any ambiguity, be

represented by $(a\ b\ .\ .\ .\ k)$. There are altogether s^n assemblies, of which a subset of N assemblies may be called an array and represented by (N, n, s). This array is said to be of strength d if all s^d assemblies corresponding to any d factors chosen out of n occur an equal number of times. An array of strength d is represented by (N, n, s, d). This array, when N is of the form s^m, has been termed as a hypercube of strength d in (Rao, 1946).

In a later section it has been shown that the following relationships hold among the parameters defining an array of strength d:

$$N = \lambda\, s^d \text{ where } \lambda \text{ is an integer,}$$

$$N - 1 \geqslant {}^nC_1 (s - 1) + {}^nC_2 (s - 1)^2 \ldots {}^nC_{\frac{1}{2}d} (s - 1)^{\frac{1}{2}d}, \text{ when } d \text{ is even,}$$

and

$$N - 1 \geqslant {}^nC_1 (s - 1) + \ldots + {}^nC_{\frac{1}{2}(d-1)} (s - 1)^{\frac{1}{2}(d-1)} + {}^{n-1}C_{\frac{1}{2}(d-1)} (s - 1)^{\frac{1}{2}(d+1)}, \text{ when}$$

d is odd.

Some important problems are the constructions of these arrays with the optimum values of n for a given d and N.

When $d = 2$ and $N = s^2$ the array co-exists with $(n - 2)$ mutually orthogonal latin squares, the levels of the last two factors corresponding to the rows and columns and those of the i-th factor to the letters of the i-th orthogonal square.

When $d = 2$ and $N = s^3$ we get arrangements corresponding to orthogonal hypercubes (Fisher (1946), Kishen (1942)). The representation of these hypercubes as a set of assemblies is convenient as no geometrical configurations such as cells of a cube, etc., are used.

3. *Construction of Arrays*

A general method of constructing these arrays of strength d when $N = s^m$ where s is a prime or a prime power has been discussed in (Rao, 1946). Some special methods of construction are discussed below and practically useful arrangements are listed.

Arrays of the Form $(s^2, n, s, 2)$.

These arrays are derivable from $(n - 2)$ mutually orthogonal squares of order s, so that the maximum value of n is two more than the number of mutually orthogonal squares of order s.

Superimpose all the $(n - 2)$ orthogonal squares so that each cell of this composite square contains $(n - 2)$ ordered values. The elements of the i-th square are made to correspond to the levels of the i-th factor. Border this square with elements $1, 2, \ldots s$ for the rows and $1, 2 \ldots s$ for the columns. These elements can be made to correspond to the levels of the $(n - 1)$-th and n-th factors. Each cell together with the bordered elements gives an ordered set of n values and the sets arising out of s^2 cells give the array $(s^2, n, s, 2)$. It follows that when $s = 6$, the maximum value of $n = 3$ as there exists no Graeco-Latin square, and when $s = 12$, n is not less than 5 as it is known that there exist at least 3 orthogonal squares of size 12.

Given an array $(s^2, n, s, 2)$ we can get arrays for factors less than n by omitting the levels of some factors. The method is illustrated below when $s = 4$. The 3 orthogonal squares with bordered elements are—

	1	2	3	4
1	111	222	333	444
2	234	143	412	321
3	342	431	124	213
4	423	314	241	132

The arrangement (16, 5, 4, 2) is

1 1 1 1 1	2 3 4 1 2	3 4 2 1 3	4 2 3 1 4
2 2 2 2 1	1 4 3 2 2	4 3 1 2 3	3 1 4 2 4
3 3 3 3 1	4 1 2 3 2	1 2 4 3 3	2 4 1 3 4
4 4 4 4 1	3 2 1 4 2	2 1 3 4 3	1 3 2 4 4

By omitting the levels of any one and two factors we get arrangements (16, 4, 4, 2) and (16, 3, 4, 2) respectively.

Arrays of the Form (2^r, n, 2, d).

Let us represent the two levels of a factor by + and − and write down all the 2^r combinations of r factors $F_1, F_2, \ldots F_r$ each at 2 levels. These represent r columns of the array. A column obtained by multiplying the signs occurring in a row from the i-th, j-th, . . . columns is represented by $F_i F_j \ldots$ The $2^r - 1$ columns generated by

$$F_i\,(i = 1, 2, \ldots r);\; F_i F_j\,(i, j = 1, 2, \ldots r, i \neq j);\; \ldots;\; F_1 F_2 \ldots F_r$$

satisfy the properties of an array of strength 2. This is the maximum possible number of factors. Hence when $d = 2$ the highest value of n for which the array can be constructed is $2^r - 1$.

To get arrays of strength 3, we have to include besides $F_1, F_2, \ldots F_r$ combinations involving only odd number of selections. This gives the number of factors as 2^{r-1} which is the maximum possible as shown by the latter inequality above. Thus when $d = 3$, the maximum n is 2^{r-1}.

To get arrays of strength 4, we have to include besides $F_1, F_2, \ldots F_r$, the columns listed in Table 1 below in special cases.

TABLE 1.—*Arrays of Strength* 4

N	Columns to be retained besides $F_1, F_2, \ldots F_r$
2^4	$F_1 F_2 F_3 F_4$
2^5	$F_1 F_2 F_3 F_4 F_5$
2^6	$F_1 F_2 F_3 F_4$, $F_2 F_3 F_5 F_6$
2^7	$F_1 F_2 F_3 F_4$, $F_1 F_2 F_5 F_6$, $F_2 F_3 F_6 F_7$, $F_1 F_2 F_3 F_4 F_5 F_6 F_7$

As examples we may construct arrays of the form (2^3, n, 2, d). Thus, for example, in Table 2, following, is given the array (2^3, 7, 2, 2).

Arrays of the Form (s^r, n, s, d).

When $s = 3$ or 5 the levels of a factor can be represented by the residue classes mod s, and when $s = 4$, the levels of a factor are represented by 4 elements, 0, 1, α and α^2 satisfying the relationships $1 = \alpha + \alpha^2$, $1 + \alpha = \alpha^2$ $1 + \alpha^2 = \alpha$. As before we take r factors $F_1, F_2, \ldots F_r$ and write down s^r combinations using these values. A column obtained as a linear combination of the elements in the rows from the first r columns is represented by $\lambda_1 F_1 + \ldots + \lambda_r F_r$, where $\lambda_1, \ldots \lambda_r$ are the compounding coefficients. The column obtained from $\lambda_1 F_1 + \ldots + \lambda_r F_r$ is the same as that obtained from $\sigma\lambda_1 F_1 + \ldots + \sigma\lambda_r F_r$ except for a permutation of the elements. There are $(s^r - 1)/(s - 1)$ linear functions which give rise to $(s^r - 1)/(s - 1)$ factors

TABLE 2.—*The Array* (2^3, 7, 2, 2)

F_1	F_2	F_3	$F_1 F_2$	$F_1 F_3$	$F_2 F_3$	$F_1 F_2 F_3$
+	+	+	+	+	+	+
+	+	−	+	−	−	−
+	−	+	−	+	−	−
+	−	−	−	−	+	+
−	+	+	−	−	+	−
−	+	−	−	+	−	+
−	−	+	+	−	−	+
−	−	−	+	+	+	−

The array (2^3, 4, 2, 3) is obtained by omitting the columns $F_1 F_2$, $F_1 F_3$ and $F_2 F_3$.

TABLE 3.—*Hypercubes of Strength* 3

N	Columns to be retained besides $F_1, F_2, \ldots F_r$
3^3	$F_1 + F_2 + F_3$
3^4	$F_1 + F_2 + F_3,\ F_2 + F_3 + F_4,\ F_1 + F_3 + F_4,\ F_1 + F_2 + F_4$
4^3	$F_1 + F_2 + F_3,\ F_1 + \alpha F_2 + \alpha^2 F_3,\ F_1 + \alpha^2 F_2 + \alpha F_3$
5^3	$F_1 + F_2 + F_3,\ F_1 + 2F_2 + 3F_3,\ F_1 + 3F_2 + 4F_3$

The only useful hypercube of strength 4 is when $N = 3^4$ and $s = 3$, in which case the column to be retained besides F_1, F_2, F_3, F_4 is $F_1 + F_2 + F_3 + F_4$. As an example the array (3^3, 4, 3, 3) is given below in Table 4.

TABLE 4.—*The Array* (3^3, 4, 3, 3)

F_1	F_2	F_3	ΣF	F_1	F_2	F_3	ΣF	F_1	F_2	F_3	ΣF
0	0	0	0	1	0	0	1	2	0	0	2
0	0	1	1	1	0	1	2	2	0	1	0
0	0	2	2	1	0	2	0	2	0	2	1
0	1	0	1	1	1	0	2	2	1	0	0
0	1	1	2	1	1	1	0	2	1	1	1
0	1	2	0	1	1	2	1	2	1	2	2
0	2	0	2	1	2	0	0	2	2	0	1
0	2	1	0	1	2	1	1	2	2	1	2
0	2	2	1	1	2	2	2	2	2	2	0

for an array of strength 2. To get arrays of strength 3 we have to retain the columns shown in Table 3 besides $F_1, F_2, \ldots F_r$ in these special cases.

4. *Confounded Designs for Symmetrical Factorial Experiments*

If all the blocks in a confounded design for a symmetrical factorial experiment are arrays of strength d, then all main effects and interactions up to the order $(d - 1)$ are preserved. These designs can be derived from the arrays constructed in the previous section in a simple manner, and they include the maximum possible number of factors. Designs with lesser number of factors can be derived from them by omitting the levels corresponding to some factors.

Starting with the array (s^m, n, s, d) as constructed in the previous section, we can construct another array of strength d by the following procedure: If $(a' b' \ldots)$ is an assembly not occurring in this array, then the array, constructed out of the sets $(a + a', b + b' \ldots)$, where $(a b \ldots)$ are sets of the given array, is of strength d. By starting with an assembly which has not occurred in the first two we can derive a third array and so on. There are s^{n-m} different arrays that can be constructed in this way and they cover all the s^n combinations. The law of addition when $s = 2$, in which case the levels have been represented by + and −, corresponds to multiplication, i.e. is—

$$(+) + (+) = (+); \ (-) + (-) = (+); \ (+) + (-) = (-).$$

This gives us a confounded design for s^n symmetrical factorial experiment in blocks of s^m plots preserving main effects and interactions up to the order $(d - 1)$. A list of useful designs is given in Table 5. Designs are not written out in full, as they can be easily derived from the method discussed above. The method of analysis is fully discussed in Rao (1946).

TABLE 5.—*Confounded Designs for Symmetrical Factorial Experiments*

Levels of a factor.	Block size.	d.	Maximum number of factors possible.
s (Prime or prime power).	s^m	2	$(s^m - 1)/(s - 1)$
2	2^m	3	2^{m-1}
	2^4	4	5
	2^5	4	6
	2^6	4	8
3	3^3	3	4
	3^4	3	8
	3^4	4	5
4	4^3	3	6

The designs for $d = 2$ are given in Fisher (1943 and 1945).

The possibilities of constructing confounded designs when the block size need not necessarily be a power of the levels of a factor will be discussed in a subsequent communication. They depend on the existence of these arrays for values of N other than powers of the levels of a factor.

5. *Designs for Asymmetrical Factorial Experiments*

The combinatorial problem, methods of analysis and a list of useful designs are given in (Nair and Rao, 1946). The combinatorial problem leading to what has been called a two-dimensional factorial design is as follows: If there are two factors A and B at levels c and m, then $c \times m$ combinations can be represented by $a_i b_j$ $(i = 1, 2 \ldots c, j = 1, 2 \ldots m)$. An

arrangement in b sets of k combinations each is called a two-dimensional factorial design $c \times m$ if in the totality of the sets,

(i) each combination is used an equal number of times, and
(ii) the combination $a_i b_j$ occurs with $a_h b_g$ in λ_{11} sets if $i \neq h$, $j \neq g$; λ_{10} sets if $i \neq h, j = g$; and λ_{01} sets if $i = h, j \neq g$ and $\lambda_{00} = 0$ sets if $i = h, j = g$.

If such an arrangement exists, we can construct designs for $c \times m$ in blocks of size k and $c \times m^2$ in blocks of size km (Yates, 1935). We need replace the m^2 combinations of two factors by m sets of m combinations each such that they involve $(m - 1)$ comparisons belonging to the interaction of these two factors.

If the array $(N, n, s, 2)$ exists with $N = \lambda s^2$, then identifying the n factors with the levels of a factor A and the s values with the levels of a factor B we get the two-dimensional design $n \times s$ with N blocks of size n and $\lambda_{11} = \lambda_{10} = \lambda$, $\lambda_{01} = 0$.

Since the properties of an array are retained by a permutation of the values of any factor or factors, we can get an arrangement of $(N, n, s, 2)$ such that some of the factors have the values $1, 2, \ldots s$ respectively for some s assemblies. Let the number of such factors be g. Then omitting the rest of the factors and these s assemblies, we get a derived configuration involving g factors and $(N - s)$ assemblies. Taking the g factors as levels of A and s values as levels of B and an assembly as a block we get a two-dimensional factorial design $g \times s$ with $(N - s)$ blocks of size g and $\lambda_{11} = \lambda$, $\lambda_{10} = \lambda - 1$, $\lambda_{01} = 0$. The method is illustrated below. Plackett and Burman (1946) have given the array of Table 6.

TABLE 6.—*The Array* (12, 11, 2, 2)

		Levels of the first factor.										
		7	1	2	8	3	4	5	9	10	11	6
Blocks	1	+	+	−	+	+	+	−	−	−	+	−
	2	+	−	+	+	+	−	−	−	+	−	+
	3	−	+	+	+	−	−	−	+	−	+	+
	4	+	+	+	−	−	−	+	−	+	+	−
	5	+	+	−	−	−	+	−	+	+	−	+
	6	+	−	−	−	+	−	+	+	−	+	+
	7	−	−	−	+	−	+	+	−	+	+	+
	8	−	−	+	−	+	+	−	+	+	+	−
	9	−	+	−	+	+	−	+	+	+	−	−
	10	+	−	+	+	−	+	+	+	−	−	−
	11	−	+	+	−	+	+	+	−	−	−	+
	12	−	−	−	−	−	−	−	−	−	−	−

The values inside the table correspond to the levels of the second factor at 2 levels. This supplies the design for $c \times 2$ or $c \times 2^2$ when $c \leqslant 11$ utilizing only 12 blocks with $\lambda_{11} = \lambda_{10} = 3$, $\lambda_{01} = 0$. By omitting blocks (11) and (12) and the factors 7, 8, 9, 10 and 11 we get a derived configuration giving the designs for 6×2 or 6×2^2 with $\lambda_{11} = 3$, $\lambda_{10} = 2$, $\lambda_{01} = 0$ and only 10 blocks. Since designs for $c \times 2$ or $c \times 2^2$ can be obtained from (8, 7, 2, 2) we get the possible types of designs shown in Table 7 for $c \times 2$ or $c \times 2^m$ in general.

TABLE 7.—*Types of Designs for* c × 2^m

Value of c.	Number of blocks.	λ_{11}	λ_{10}	λ_{01}
$c \leqslant 11$	12	3	3	0
$c \leqslant 6$	10	3	2	0
$c \leqslant 7$	8	2	2	0
$c \leqslant 4$	6	2	1	0

Some other types of designs are listed in (Rao, 1946).

6. *General Symmetrical Factorial Experiments*

a) *Contrasts Defining Main Effects and Interactions*

The s^n assemblies arising out of n factors can be represented as $(a\ b\ .\ .\ .\ k)$ where each letter can assume values 0, 1, . . . $(s-1)$. The assembly (0 0 . . . 0) which is a combination of all factors at the first level is called the nominal assembly. Denoting by

$$i^a = m_{0a}\, i_0 + \ldots + m_{s-1\,a}\, i_{s-1}$$

such that $\sum\limits_j m_{ja} = 0$ when $a \neq 0$

$\sum\limits_j m_{ja}\, m_{jb} = 0$ when $a \neq b$

and $m_{ja} = 1$ for all j when $a = 0$

$i_0, i_1, \ldots i_{s-1}$ representing the s levels of the i-th factor, we can define symbolic products of the form $1^a\, 2^b \ldots n^k$ which may be represented by $[a\ b\ .\ .\ .\ k]$ to distinguish it from the assembly $(a\ b\ .\ .\ .\ k)$.

Since $[a\ b\ .\ .\ .\ k]$ is a linear function of the assemblies we may consider orthogonality and independence of these functions in the usual manner. There are s^n functions of this type which are all orthogonal and hence independent. The s^n assemblies can then be, alternatively, expressed as linear functions of these functions.

A function $[a\ b\ .\ .\ .\ k]$ is called a contrast if all the values are not simultaneously zero. The contrast belongs to the interaction of factors for which the corresponding values are not zero. The main effects may be considered to be one factor or zero order interactions. There are $(s-1)^t$ orthogonal functions defining the interaction of t specified factors. Any linear function which defines a component of the interaction of these factors must necessarily be built out of these functions. Hence we get the necessary and sufficient condition that a linear function of the assemblies belongs to or defines a component of a certain interaction is that it is orthogonal to every function of the type $[a\ b\ .\ .\ .\ k]$ except those defining that interaction.

When some of the interactions are not present, i.e. when the parametric functions defining them are identically zero, then any linear function of the contrasts defining them can be added to a contrast belonging to any other interaction without altering its value. Hence we derive the necessary and sufficient conditions that a linear function of assemblies measures a component of an interaction when some of the interactions are absent as

(i) it is orthogonal to [0 0 . . . 0],

(ii) it is not orthogonal to at least one function of the type $[a\ b\ .\ .\ .\ k]$ defining the specified interaction,

(iii) it is orthogonal to functions $[a\ b\ .\ .\ .\ k]$ defining interactions other than those that are absent and the specified one.

It may be possible for linear functions to exist satisfying (i), (ii) and (iii) above, but depending only on a subset of assemblies, so that contrasts defined by these functions can be measured from a subset of the assemblies. Also the necessary and sufficient condition that a specified contrast $[a\,b\,.\,.\,.\,k]$ is measurable from a subset is that there exists a linear function involving only the assemblies in the subset such that it is not orthogonal to $[a\,b\,.\,.\,.\,k]$ but to every other function of this type with the possible exception of those defining the interactions which are absent.

(b) *A Class of Subsets Defined by Arrays of Strength* d

By using the properties of arrays of strength d we can define subsets from which main effects and interactions up to order $(k-1)$ are measurable when interactions of order equal to and greater than $(d-1)\,(d>k)$ are absent. Incidentally the method of constructing contrasts defining specified interactions is also made available.

Let the subset of s^n assemblies be an array $(N, n, s, 3)$ of strength 3. The function obtained from $[a\,b\,.\,.\,.\,k]$ by retaining only the assemblies contained in the subset may be represented by $\{a\,b\,.\,.\,.\,k\}$. Consider $\{a\,0\,.\,.\,.\,0\}$ where $a \neq 0$. This is evidently not orthogonal to $[a\,0\,.\,.\,.\,0]$. Since all combinations of every 3 factors are equally repeated it follows that $\{a\,0\,.\,.\,.\,0\}$ is orthogonal to $\{a'\,b\,c\,0\,.\,.\,.\,0\}$ and hence to $[a'\,b\,c\,0\,.\,.\,.\,0]$ when at least one of a', b, c is zero, except when $a' = a$, $b = c = 0$. As this holds true for every pair of factors taken in conjunction with the first it follows that the contrast $[a\,0\,.\,.\,.\,0]$ is measured by $\{a\,0\,.\,.\,.\,0\}$ when all interactions involving 3 or more factors are absent. Similarly every contrast defining a main effect is measurable. Since $\{a\,0\,.\,.\,.\,0\}$ is orthogonal to $\{0\,b\,0\,.\,.\,.\,0\}$ it follows that any two contrasts defining main effects of two factors are orthogonal. In general, if interactions involving d and more factors are absent, then a sufficient condition for a subset to admit measurability of main effects is that it is an array of strength d.

Similarly, if the subset is an array of strength 4, then $\{a\,b\,0\,.\,.\,.\,0\}$ where $a \neq 0$, $b \neq 0$, measures the contrast $[a\,b\,0\,.\,.\,.\,0]$ when interactions of factors 3 and more are absent. A sufficient condition for the subset to admit measurability of first order interactions when interactions involving d and more factors are absent is that it is an array of strength $(d+1)$.

In general, when interactions of order equal to and greater than $d-1$ are absent, then an array of strength $(d+k-1)$ or n, whichever is smaller, admits the measurability of interactions up to order $(k-1)$ and the expressions defining these contrasts are orthogonal.

(c) *Designs for Multifactorial Experiments*

Plackett and Burman (1946) have given optimum designs for multifactorial experiments from which main effects can be estimated when all interactions of order equal to and greater than one are absent. This means choosing a subset of assemblies satisfying the optimum properties of an array of strength 2. It may be useful to find out multifactorial designs from which all main effects and first order interactions can be estimated except for a possible bias introduced by the presence of interactions involving d and more factors. An array of strength $(d+1)$ as a design for multifactorial experiments satisfies this requirement. Incidentally some more interactions may be measured from this array.

The analysis of such a design can be carried out in the usual manner. The sum of squares for main effects and interactions can be calculated by retaining only the assemblies present in the array and using proper divisors. If we are using N assemblies with n factors each at s levels and the array is of strength greater than 4, then the analysis is as follows:

Analysis of Arrays

	Degrees of freedom.
Main effects	$n(s-1)$
First order interactions	$\frac{n(n-1)}{2}(s-1)^2$
Error	(obtained by subtraction)
Total	$N-1$

If the design is an array of strength $(d + 1)$, then the estimates of main effects and interactions are unaffected by interactions of order greater than one and less than d, but the estimate of error is enhanced by their presence. But this keeps us on the safe side in declaring the significance of observed contrasts. If the design admits the estimability of contrasts other than those specified above, they may be removed from the component of error.

Since by using $(N, n, s, 2t)$ we can measure all interactions up to order $(t - 1)$ assuming higher order interactions to be not present, the number $(N - 1)$ of independent contrasts that can be built out of N assemblies must be greater than the number of independent contrasts that can be estimated; hence we get—

$$N - 1 \geq {}^nC_1 (s - 1) + \ldots + {}^nC_t (s - 1)^t,$$

If we are using the array $(N, n, s, 2t + 1)$, then interactions up to order $(t - 1)$ at least are measurable. If we take any contrast belonging to the interaction of $(t + 1)$ factors and retain only those combinations which are present in this array, then this is orthogonal to contrasts arising out of $(t + 1)$ factors which contain at least one factor in common with the first set. This shows that at least ${}^{n-1}C_t (s - 1)^{t+1}$ more linear functions are measurable. Hence—

$$N - 1 \geq {}^nC_1 (s - 1) + \ldots + {}^nC_t (s - 1)^t$$
$$+ {}^{n-1}C_t (s - 1)^{t+1}$$

When $t = 0$, this inequality reduces to $N - 1 \geq s - 1$, which is true. These are the inequalities mentioned in article 2 connecting N, the number of assemblies and n, the number of factors for given strengths $2t$ and $2t + 1$.

A list of multifactorial designs involving less than 150 assemblies is given in Tables 8–10. The maximum number of factors given by the above inequalities is denoted by *max n*.

TABLE 8.—*Multifactorial Designs (Hypercubes of Strength* 2)

(Main effects measurable when interactions of order $d \geq 1$ are absent.)

N	s	n	*max n*
4λ	2	$4\lambda-1$	$4\lambda-1$
3^2	3	4	4
3^3	3	13	13
3^4	3	40	40
4^2	4	5	5
4^3	4	21	21
5^2	5	6	6
5^3	5	31	31
6^2	6	3	7
7^2	7	8	8
8^2	8	9	9
9^2	9	10	10
10^2	10	3*	11
11^2	11	12	12
12^2	12	5*	13

The designs for $s = 2$ up to $\lambda = 25$ are given in Plackett and Burman (1946).

* Indicates possibly maximum number of factors for which the hypercube exists.

TABLE 9.—*Multifactorial Designs (Hypercubes of Strength* 3)

(Main effects measurable when interactions of order $d > 2$ are absent.)

N	s	n	max n
2^3	2	4	4
2^4	2	8	8
2^5	2	16	16
2^6	2	32	32
2^7	2	64	64
3^3	3	4	5
3^4	3	8	14
4^3	4	6	6
5^3	5	6	7

TABLE 10.—*Multifactorial Designs (Hypercubes of Strength* 4)

(Main effects and first order interactions measurable when interactions of order $d > 2$ are absent.)

N	s	n	max n
2^4	2	5	5
2^5	2	6	7
2^6	2	8	10
2^7	2	11	15
3^4	3	5	6

Designs involving higher numbers of assemblies can be constructed by the methods given for constructing arrays of strength d.

(d) *Multifactorial Experiments with Groups of Assemblies*

In considering multifactorial experiments it has been assumed that there exists no source of variation due to any assignable causes affecting the assemblies. In practice, it may not always be possible to get the effects of assemblies on homogeneous material. In such cases valid comparisons are still possible if the assemblies are assigned at random to different portions of the material. Another method which has the advantage of reducing the error to which the contrasts are subject is to assign groups of assemblies to fairly homogeneous portions of the experimental material and build up contrasts from comparisons arising within the groups. This purpose is served by blocks in field experimentation. In pharmaceutical and other experiments it has been found that observations are affected by the temperature of the day on which the experiment is carried out. If the experiment involves a large number of assemblies with experimentation spread over a number of days, it is desirable to assign groups of assemblies to days in such a manner that desired comparisons are unaffected by temperature differences of days.

If we are using only a subset of assemblies, then the problem reduces to splitting the subset into groups such that desired contrasts are orthogonal to contrasts arising out of groups. The only change in the analysis of variance table is to include the sum of squares due to groups with the appropriate degrees of freedom.

Consider the designs corresponding to (N n, 2, 2) which admit the measurability of main effects only. We may split this into two groups so that the contrast of the groups is the interaction of the first two factors A and B. This interaction is measurable from (N, n, 2, 2) if all combnations of 3 factors involving both A and B occur an equal number of times. As a necessary condition it follows that N must be a multiple of 8. If we want an arrangement in four groups then the two factor interactions AB, BC, CA of the three factors A, B and C may be chosen as contasts among the groups. A sufficient condition that these effects are orthogonal to main effects is that all combinations of 3 factors involving any two of A, B and C occur an equal number of times.

The actual splitting of the array into two groups is done as follows: All those assemblies with identical levels for the first two factors are put in one group and the rest in another. To get a design in four groups all assemblies containing the combinations 111, 000; 101, 010; 001, 110; and 100, 011 for the first three factors are put in the first, second, third and fourth groups respectively. The correspondence of (0, 1) with (+, −) can be made in any desired manner if we are using the array containing the elements + and − used in their construction.

Arrays satisfying the above properties can be constructed when $N = 2^r$. To get designs in two groups we have to exclude the columns with even number of selections of F_1, F_2, . . . F_r article 2) whenever F_1 or F_2 or F_1 and F_2 are involved, if F_1 and F_2 are made to correspond with A and B. To get designs in four groups we have to see that this holds good with any pair chosen out of F_1, F_2 and F_3. A list of designs in two and four groups is given in Table 11.

TABLE 11.—*Factorial Arrangements in Groups*

(Main effects measurable when interactions of order $d > 1$ are absent except AB for 2 groups and AB, BC, CA for 4 groups.)

N	s	Number of factors for 2 groups.	4 groups.
2^3	2	6	4
2^4	2	14	12
2^5	2	30	28
2^6	2	62	60
2^7	2	126	124

In case we are using multifactorial designs of the type (N, n, s, 2) for $s > 2$ we may impose the condition that all combinations of 3 factors involving the first two are equally repeated. In

TABLE 12.—*Factorial Arrangements in* s *Groups*

(Main effects measurable when interactions of order $d > 1$ are absent except AB.)

N	s	Number of factors.
3^3	3	11
3^4	3	38
4^3	4	18
5^3	4	27

this case $(s-1)^2$ contrasts are available of which a subset of $(s-1)$ contrasts can be used as contrasts of the groups. The assemblies in the array are divided into s groups with the help of the combinations of the first two factors. If these s^2 combinations are identified with the cells of a latin square, with the factors corresponding to rows and columns, then the combinations occurring with a latin letter determine the assemblies in a group.

If we are using $(N, n, s, 3)$ then the above conditions are automatically satisfied. Hence they can be split up into groups for the same number of factors given in Table 9, thus supplying designs for factorial experiments from which main effects are measurable without involving first order interactions.

If we are using $(N, n, s, 4)$ and desire to estimate main effects and first order interactions we have to choose for contrasts of the groups a three-factor interaction. As a sufficient condition, we may impose the restriction that all combinations of every 5 factors involving the first three occur equally often, in which case the contrasts of ABC are available for comparisons of the groups. The minimum number of assemblies is s^5.

TABLE 13.—*Factorial Arrangements in 2 Groups*

(Main effects and first order interactions measurable when interactions of order $d \geqslant 2$ are absent except ABC.)

N	s	Number of factors.
2^5	2	6
2^6	2	8
2^7	2	11

There is a choice in these factorial arrangements in groups as to which of the factors are to be identified with A, B and A, B, C whose interactions are used for confounding in the groups in the above cases. Since we are assuming that all interactions of this order with the possible exception of these are absent we may identify with A, B, C those factors which indicate slight interactions. If these interactions are not negligible, it may be, from a practical point of view, necessary to measure them, in which case arrays of higher strength have to be used.

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Exhibition of Mechanical Aids to Statistical Computation

On July 20, 1946, the Research Section held an exhibition of machines and instruments designed to facilitate some of the lengthy and tedious computations necessary in statistical work. The idea of organizing such an exhibition arose out of the "Symposium on Autocorrelation in Time Series" held earlier in the session, when a number of new machines for the calculation of autocorrelations were described, but could not be demonstrated at the meeting itself. The venture was more than justified by the excellent attendance and the great interest shown in the exhibits.

The exhibition was arranged in two sections. At Imperial College, South Kensington, in a room kindly placed at the Society's disposal by Professor H. Levy, the machines described at the Symposium were demonstrated in action. Particular interest was shown in the Relay Computor, designed by Mr. W. Barnes for the use of Dr. L. B. C. Cunningham and Mr. W. R. Hynd at the Ministry of Aircraft Production, and in the Correlogram Calculator made by the National Physical Laboratory to Mr. G. A. R. Foster's design.

Calculating machines of the more orthodox type, including the National Accounting Machine, were also demonstrated at Imperial College by the Scientific Computing Service, who also arranged a comprehensive display of tables indispensable for statistical and other types of computation. Dr. L. J. Comrie introduced the exhibition with a short lecture describing the relative merits of the various machines.

In the afternoon, by courtesy of the War Office, visitors were invited to attend a demonstration of Hollerith equipment which was held at the Q. Stats. Department, Northumberland Avenue, and which was well attended. The essential part of the calculation of serial correlations (recently described in the Supplement, Part II, 1946) was demonstrated on the Reproducer, the well-known method of summary multiplication (or progressive digiting as it is called in the United States) was shown on a Senior Roller Tabulator, and numerous smaller demonstrations were given on the Sorter, Multiplying Punch and other punched card equipment.

The Society is greatly indebted to all who co-operated in making the exhibition an unqualified success. In particular they wish to thank Professor H. Levy and Mr. G. Barnard of Imperial College, Dr. L. J. Comrie and his staff at the Scientific Computing Service, the Q. Stats Department of the War Office, and Mr. Michaelson of the British Tabulating Machine Company.

The meeting was well reported by the *Manchester Guardian* in their "London Letter."

SUPPLEMENT TO THE

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Statistical Investigation of Casualties Suffered by Certain Types of Vessels.

By S. Vajda.

[Read before the Research Section of the Royal Statistical Society, April 2, 1947, Mr. H. L. Seal in the Chair.]

This paper gives a description of some statistical problems and of the technique used to deal with them. Statisticians may be interested in the methods of analysis utilized in the investigation, which was carried out in the Statistical Branch of the Admiralty.

1. *The Material.*

Reports were received by the Statistical Branch of completion of new vessels of the types investigated and of casualties to these vessels. The precise definition of "Casualty" or "Accident" and the decision whether a succession of mishaps should be considered as a single casualty or not remained, of course, outside the duties of this Branch, which had to accept in this respect the information received.

Reports were also received of events which made a vessel cease to form part of the investigation. This is not necessarily due to a casualty resulting in a complete loss, but may be a conversion to some other type, sale to another Government, or generally any reason which makes it certain that future casualties will not be made known to the collecting section in the routine way. Such events are referred to as "Exits."

The technique of investigation was developed so as to make the best use of an installation of Hollerith machines consisting of punches, sorter, reproducer, collator and tabulator. A detailed description of the work carried out on them is given in the Appendix.

The basic notion of this investigation is the "Accident Rate." It is defined as the ratio of the number of accidents and the number of vessels, each vessel multiplied by the number of days during which it was exposed to the risk of accidents. It should be mentioned, however, that no deduction was made for days spent in harbour, dock, etc. It would have been impossible with the data available to take account of these days. On the whole, the effect of this inaccuracy is small. Its tendency is to underestimate the accident rate.

This paper deals only with a period covering the three years from December 1, 1941, to November 30, 1944. The following table will give some idea of the extent of the corresponding material:

TABLE 1.

Month.	Vessels completed.	First Casualties.	All Casualties.	Exits.	Month.	Vessels completed.	First Casualties.	All Casualties.	Exits.
Dec., 1941	2	—	—	—	June, 1943	111	16	16	7
Jan., 1942	3	—	—	—	July ,,	106	12	13	10
Feb. ,,	11	—	—	—	Aug. ,,	108	11	15	6
Mar. ,,	16	—	—	—	Sept. ,,	111	5	7	10
Apr. ,,	26	—	—	—	Oct. ,,	116	21	27	15
May ,,	42	—	—	2	Nov. ,,	100	16	22	13
June ,,	51	—	—	3	Dec. ,,	124	49	61	12
July ,,	52	—	—	8	Jan., 1944	75	57	90	12
Aug. ,,	58	1	1	2	Feb. ,,	78	47	69	5
Sept. ,,	67	2	2	4	Mar.	81	70	111	7
Oct. ,,	65	1	1	3	Apr. ,,	74	36	63	5
Nov. ,,	64	4	4	7	May ,,	64	29	39	2
Dec. ,,	81	15	16	1	June ,,	55	13	23	6
Jan., 1943	77	13	14	8	July ,,	49	4	7	5
Feb. ,,	80	22	24	7	Aug. ,,	48	9	17	4
Mar. ,,	99	30	33	19	Sept. ,,	42	8	15	3
Apr. ,,	107	11	16	8	Oct. ,,	46	7	12	3
May ,,	116	18	22	4	Nov. ,	42	2	4	2
					Totals	2447	529	744	203

There were altogether—

	373 vessels with 1 casualty	373 casualties.
	117 ,, ,, 2 ,,	234 ,,
	24 ,, ,, 3 ,,	72 ,,
	11 ,, ,, 4 ,,	44 ,,
	3 ,, ,, 5 ,,	15 ,,
	1 ,, ,, 6 ,,	6 ,,
Total	529 vessels with altogether	744 casualties.

TABLE 2.

Vessels in Service During the Second Half of the Month.

Month.	Number.	Month.	Number
December, 1941	2	June, 1943	1045
January, 1942	5	July, ,,	1141
February, ,,	16	August, ,	1243
March, ,,	32	September, ,	1344
April, ,,	58	October, ,	1445
May, ,,	98	November, ,,	1532
June, ,,	146	December, ,,	1644
July, ,,	190	January, 1944	1707
August, ,,	246	February, ,	1780
September, ,,	309	March, ,	1854
October, ,,	371	April, ,,	1923
November, ,,	428	May, ,	1985
December, ,,	508	June, ,,	2034
January, 1943	577	July, ,,	2078
February, ,,	650	August, ,,	2122
March, ,,	730	September, ,	2161
April, ,,	829	October, ,,	2204
May, ,,	941	November, ,	2244

1918 vessels remained without a casualty during the period under review.

For the construction of approximate accident rates, we assume that in any calendar month exits and entries all happened in the middle of the month. A table of vessels in service during the second half of that month can then be constructed by subtracting the number under "Exit" from that under "Vessels completed" and accumulating the results line by line. The result is shown in Table 2.

If we take the means of the previous and the current monthly numbers, e.g. 1 for December, 1941, $3\frac{1}{2}$ for January, 1942, etc., and multiply these means by the numbers of days in the current month, approximate numbers of ship-days are arrived at; these are given in Column 2 of Table 3 below. Comparison with the casualties of the same month, as given in Table 1, leads to the accident rates in Columns 3 and 6 below.

TABLE 3.

Month.	Ship-days exposed.	Accident rates × 10,000.	Month.	Ship-days exposed.	Accident rates × 10,000.
Dec., 1941	31·0	—	June, 1943	29,790·0	5·37
Jan., 1942	108·5	—	July ,,	33,883·0	3·84
Feb. ,,	294·0	—	Aug. ,,	36,952·0	4·06
Mar. ,,	744·0	—	Sept. ,,	38,805·0	1·80
Apr. ,,	1,350·0	—	Oct. ,,	43,229·5	6·25
May ,,	2,418·0	—	Nov. ,,	44,655·0	4·93
June ,,	3,660·0	—	Dec. ,,	49,228·0	12·39
July ,,	5,208·0	—	Jan., 1944	51,940·5	17·33
Aug. ,,	6,758·0	1·48	Feb. ,,	50,561·5	13·65
Sept. ,,	8,325·0	2·40	Mar. ,,	56·327·0	19·71
Oct. ,,	10,540·0	·95	Apr. ,,	56,655·0	11·12
Nov. ,,	11,985·0	3·34	May ,,	60,574·0	6·44
Dec. ,,	14,508·0	11·03	June ,,	60,285·0	3·82
Jan., 1943	16,817·5	8·32	July ,,	63,736·0	1·10
Feb. ,,	17,178·0	13·97	Aug. ,,	65,100·0	2·61
Mar. ,,	21,390·0	15·43	Sept. ,,	64,245·0	2·33
Apr. ,,	23,385·0	6·84	Oct. ,,	67,657·5	1·77
May ,,	27,435·0	8·02	Nov. ,,	66,720·0	·60
			Total	1,112,479·0	6·69

The accident rates show very conspicuous peaks in winter and troughs in summer. If they are telescoped into rates for months irrespective of calendar year by adding the exposed ship-days and accidents and computing the ratios of these items, the following table results:

TABLE 4.

Month.	Ship-days.	Accidents.	Rate.	Month.	Ship-days.	Accidents.	Rate.
Dec.	63,767·0	77	12·08	June	93,735·0	39	4·16
Jan.	68,866·5	104	15·10	July	102,827·0	20	1·95
Feb.	68,033·5	93	13·67	Aug.	108,810·0	33	3·03
Mar.	78,461·0	144	18·35	Sept.	111,375·0	24	2·15
Apr.	81,390·0	79	9·71	Oct.	121,427·0	40	3·29
May	90,427·0	61	6·75	Nov.	123,360·0	30	2·43
				Totals	1,112,479·0	744	6·69

It would be interesting to apply some periodicity analysis to these data. Such an analysis would have to take account of the different weights attached to the rates and no satisfactory method appears to be available.

2. *The Problems and the Tables which Form the Basis of the Analysis.*

The problems which were investigated and whose treatment can be considered typical of statistical work carried out with the aid of a punch-card installation were these:

(i) Is the accident liability of all vessels the same? Or do the casualty rates show differences from builder to builder, say?

(ii) Does the accident liability change with the increase of age?

(iii) Does the fact that the vessel has been subject to an accident alter the probability of further accidents?

In order to find an answer to these questions, a series of tables was constructed. Tables I, II and III show, for All Casualties, First Casualties, and Later Casualties respectively the material distributed according to Age Attained and Builders. The successive age groups 1, 2, etc., stand for a period of 50 days each, viz. 0–49, 50–99, etc. The columns give—

(1) The number of days exposed at the corresponding ages.

(2) The number of casualties suffered at these ages.

(3) The ratio (2)/(1), i.e. the accident rates, multiplied by 10,000 for convenience.

The grand totals are:

744 casualties in 1,110,838 days, giving an over-all rate of $6{\cdot}6977 \times 10^{-4}$ for all casualties. When comparing this result with Table 3 it should be remembered that the days given there were approximations only.

529 first casualties against 922,716 days exposed, giving a rate of first casualties of $5{\cdot}7331 \times 10^{-4}$ and finally—

215 later casualties against a total of 188,122 days exposed, which gives a rate of later casualties of $11{\cdot}4288 \times 10^{-4}$.

For reasons connected with theoretical considerations set out in the next Section the following tables were also constructed:

A frequency distribution of the intervals which preceded the accidents (Table IV).

A distribution of casualties according to their order and to the age of the vessel at the time of the casualty (part of Table VII).

Table V: Here the vessels are sorted according to their highest age attained during the period of investigation. For each group the final stratification into vessels with 0, 1, 2 . . . casualties was recorded. The table includes a column giving the total of all casualties and the ratio casualties/vessels for each line. The column "Number of Vessels" is identical with the corresponding column of Table D of the Appendix.

3. *Analysis of the Material.*

The previous chapters give a summary of the material collected. As far as results are concerned the tables which emerge are, perhaps, disappointing, in that they allow conclusions to be reached almost by inspection and consequently do not leave much scope for subtle analysis. However, as this paper is concerned with methods rather than results, a few possible ways of extracting information will be mentioned, in the hope that they may be of some value in future, similar investigations.

a. *Distribution of Intervals.*

As a preliminary to more detailed tests, we ask whether it may be assumed that the accident liability, being the same for all vessels, is independent of age and of previous accidents. The question can be answered by a consideration of the spells of service between completion and first accident and those between successive accidents.

If the probability of an accident during the time interval s/N is qs/N, then the probability that the first accident happens in the Nth of such successive intervals is clearly

$$(1 - qs/N)^{N-1}.qs/N.$$

When N tends to infinity, this probability tends to $q exp(-qs)ds$. This is then the probability density of intervals of length s between accidents.* The figures in Table IV do not agree with this frequency distribution.

In order to find some explanation for this fact we calculate the average intervals for all builders and obtain:

TABLE 5.

Builder.	Total length of intervals.	Number of casualties.	Average intervals.
A	22,759	107	213
B	21,274	104	205
C	33,988	141	241
D	20,933	83	252
E	43,177	197	219
F	13,361	48	278
G	9,568	64	150
Total	165,060	744	222

It is obvious that there are very significant differences between builders. The following table, which classifies casualties according to whether they were first or later accidents, leads to the same conclusion. It must be clear, however, that they refer only to those vessels which did suffer at least one casualty. Thus, taken by themselves, they can only give a very rough indication.

TABLE 6.

Builder.	Average of spell before first casualty.	Average of intervals before later casualties.
A	234	152
B	214	185
C	294	113
D	317	94
E	265	134
F	286	211
G	146	165
All	255	140

b. *Contingency Table Analysis.*

We will now attempt to find out something more about the possible reasons for differences in the accident rates. There are at least three which may be operative—origin from different builders, dependence on age, dependence on previous casualties. We therefore have constructed Tables II and III, which give: E (the days exposed) and C (the casualties) for every combination of the attributes age (a), builder (b) and order of casualty (c) (i.e. whether first or later casualty). The combination of these tables can be considered as a threefold contingency table with entries of unequal weight. In order to avoid some of the consequent complications, we shall use hypotheses somewhat different from those usually tested. Instead of stating hypotheses such as: "There is no (a)-effect," or "no (ab)-interaction," we test hypotheses of the type: "there is no effect apart from, possibly, (a)" or "no effect apart from, possibly, (a), (b) and/or (ab)," etc. The test for these hypotheses is comparatively simple, even for unequal weights, as will be shown in the following paragraphs ([4, 5]).

Let the number of days exposed in the position (a, b, c) be denoted by E_{abc}, the corresponding casualties by C_{abc} and the ratio C_{abc}/E_{abc} by Q_{abc}. Suppose the number of values of a, b and c

* This approach was first suggested by D. R. Cox ([6]).

to be $p+1$, $q+1$ and $r+1$ respectively. We then fix constants n_{ijk} ($i=0, 1, \ldots p$; $j=0, 1 \ldots q$; $k=0, 1 \ldots r$) so that, for all a, b and c, we have

$$Q_{abc} = \sum_{i,j,k} n_{ijk}\, P_i(a)P_j(b)P_k(c) \qquad (1)$$

where the P_i are orthogonal polynomials of order i such that $\sum_a P_i(a)P_j(a) = 0$ for $i \neq j$ and similarly for b and c. ($P_i(a)$ and $P_i(b)$ are of the same order i, but they are not identical if their arguments extend over different ranges.) Moreover, we assume for convenience that an arbitrary constant factor is fixed so that $\sum_a [P_i(a)]^2 = 1$ and similarly for b and c, as before. The solution of the set (1) with respect to n_{ijk} gives

$$n_{ijk} = \sum_a\sum_b\sum_c Q_{abc}\, P_i(a)P_j(b)P_k(c) \qquad (2)$$

We want now to find out what it means if $n_{ioo} = 0$ for all values of i which are different from zero. Because of (2), this means

$$\sum_a P_i(a) \sum_{bc} Q_{abc} = 0. \quad (i = 1, 2, \ldots p) \qquad (3)$$

We have here p homogeneous equations for the $p+1$ unknown $\sum_{bc} Q_{abc} = Q_{aoo}$, say ($a = 1, 2, \ldots p+1$).
In view of $\sum_a P_i(a) = 0$ we see that (3) is equivalent to $Q_{100} = Q_{200} \ldots = Q_{p+1\,00}$.

Thus $n_{ioo} = 0$ for all values of $i \neq 0$ means that there is no (a)-effect.

To go one step further, let $n_{ijo} = 0$ for all values of i and j different from zero. This leads to $p.q$ homogeneous equations for the $(p+1)(q+1)$ unknown $\sum Q_{abc} = Q_{abo}$, say.

We can therefore choose, say, $Q_{110}, Q_{120} \ldots Q_{1\,p+1\,0}, Q_{210}, Q_{310} \ldots Q_{q+1\,10}$, and the others will thereby be fixed. It is immediately seen that the above set of linear equations can now be written

$$Q_{110} - Q_{210} = Q_{120} - Q_{220} = \ldots = Q_{1\,q+1\,0} - Q_{2\,q+1\,0}$$

$$Q_{110} - Q_{p+1\,10} = Q_{120} - Q_{p+1\,20} = \ldots = Q_{1q+1\,0} - Q_{p+1\,q+1\,0}$$

Thus, if $n_{ijo} = 0$ for all values of i and j which are not zero, there is no (ab) interaction.

Generalization to more than three subscripts and to higher order interactions is obvious, but will not be pursued here.

Let us now assume that the variance of all Q_{abc} is the same. Then if we wish to test the significance of any effect we put the appropriate coefficients zero and decide whether the minimum of the sum of squares

$$\sum_{abc} [Q_{abc} - \sum_{ijk} n_{ijk}\, P_i(a)P_j(b)P_k(c)]^2 \qquad (4)$$

differs significantly from zero. The normal equations of this set have the same solutions for the remaining n_{ijk} as were given in (2), whatever terms we omit. It follows that the minimum of (4) is

$$\sum_{abc} Q^2_{abc} - \sum_{(ijk)} [\sum_{abc} Q_{abc}\, P_i(a)\, P_j(b)\, P_k(c)]^2 = \sum_{(i'j'k')} [\sum_{abc} Q_{abc} P_{i'}(a) P_{j'}(b) P_{k'}(c)]^2$$

where the set (i, j, k) refers to those coefficients which were retained and (i', j', k') to those which were omitted. It will be noticed that these expressions are, in fact, those which are characteristic of the various effects and interactions tested in the traditional Analysis of Variance technique. The more terms that are omitted, the larger, of course, is the minimum which is obtained. The number of degrees of freedom is equal to the number of terms in expressions like (4) which have been omitted.

This simple procedure cannot be applied when the variance of Q_{abc} is not a constant, but is proportional to E^{-1}_{abc}. In this case the expression which must be made a minimum is

$$\sum_{abc} E_{abc}[Q_{abc} - n_{ooo} - n_{1oo}\, P_1(a) - \ldots]^2 \qquad (5)$$

The testing of a hypothesis still consists in omitting certain appropriate terms on the r.h.s. of (1), and comparing the resulting minimum of the quadratic expression of the type (5) with an estimate of the variance of the weighted mean of the Q's.

If, then, we test the hypothesis that no effect exists apart from (a), or in other words that not only (b) and (c), but also (bc), (ab) and (ac) are not significant, then we retain only those terms of (5) which have zero subscripts in the second and third place, obtaining

$$\sum_{abc} E_{abc}[Q_{abc} - n_{000} - n_{100}P_1(a) - \ldots - n_{p00}\, P_p(a)]^2$$

which must be made a minimum. Simple calculations show that the normal equations for this condition are solved by

$$\Sigma\, n_{i00}\, P_i(a) = \frac{\sum_b \sum_c C_{abc}}{\sum_b \sum_c E_{abc}}$$

and that the value of the minimum thus obtained is

$$\sum_{abc} \frac{C^2_{abc}}{E_{abc}} - \sum_a \frac{\left(\sum_b \sum_c C_{abc}\right)^2}{\sum_b \sum_c E_{abc}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Similarly, the hypothesis that no effect is significant apart from, possibly, (a), (b). and/or (ab), is to be tested by

$$\sum_{abc} \frac{C^2_{abc}}{E_{abc}} - \sum_{ab} \frac{\left(\sum_c C_{abc}\right)^2}{\sum_c E_{abc}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

In the present case the following sums were obtained:

TABLE 7.

$$\sum_a \sum_b \sum_c \{C^2_{abc}/E_{abc}\} = \cdot 87296$$

$$\sum_a \sum_b \{\sum_c C^2_{abc}/\sum_c E_{abc}\} = \cdot 70425$$

$$\sum_b \sum_c \{\sum_a C^2_{abc}/\sum_a E_{abc}\} = \cdot 62030$$

$$\sum_a \sum_c \{\sum_b C^2_{abc}/\sum_b E_{abc}\} = \cdot 60032$$

$$\sum_a \{\sum_b \sum_c C^2_{abc}/\sum_b \sum_c E_{abc}\} = \cdot 52525$$

$$\sum_b \{\sum_a \sum_c C^2_{abc}/\sum_a \sum_c E_{abc}\} = \cdot 57341$$

$$\sum_c \{\sum_a \sum_b C^2_{abc}/\sum_a \sum_b E_{abc}\} = \cdot 54900$$

$$\sum_a \sum_b \sum_c C^2_{abc}/\sum_a \sum_b \sum_c E_{abc} = \cdot 49830$$

The following list contains some hypotheses and the corresponding sums of squares, obtained from expressions like (6) and (7), with their degrees of freedom:

TABLE 8.

Hypothesis: No effect apart from	d. of f. n.	S.S.	χ^2.	$\sqrt{2\chi^2} - \sqrt{2n-1}$.
(a), (b), (ab)	154	·16871	253	5·0
(b), (c), (bc)	294	·25266	379	3·3
(a), (c), (ac)	264	·27264	409	5·6
(a)	286	·34771	522	8·4
(b)	301	·29955	447	5·4
(c)	306	·32396	486	6·4
No effect at all	307	·37466	562	..

It must now be decided what estimate of the variance of the weighted mean of the Q's should be used. In the customary Analysis of Variance method a certain aggregate of effects (usually some combination of higher order interactions) is assumed to be not significant. Omitting these terms, we obtain a minimum M_1. We then omit further terms, which are characteristic of the effects to be tested, and obtain thus a (higher) minimum M_2; $(M_2 - M_1)/M_1$ is then entered in a table of the variance ratio, such as Table V in ([6]), and its significance gauged.

In the present case we do not know of any effect or aggregate of effects which is certain to be without significance. However, in view of the smallness of $q = \frac{\Sigma\, C_{abc}}{\Sigma\, E_{abc}}$ we can take q itself as an estimate of the population variance of $E_{abc}^{-\frac{1}{2}}Q_{abc}$. The degrees of freedom on which this estimate is based are certainly large enough to use that line of the table of the variance ratio which corresponds to χ^2 and therefore, instead of dividing the sums of squares by their degrees of freedom, we divide by q and enter the χ^2 tables to test the significance of the result. This has been done in the table above and we find that the age (a) has some effect, although a much smaller one than the other attributes. Thus a result which can be guessed by the inspection of the tables is confirmed.

Having thus found that the age has some effect, it is natural to ask whether, on the whole, higher ages lead to more or to fewer casualties. The coefficients of linear regression of accident rates against age were computed for first casualties and give the following picture:

TABLE 9.

Builder	A	B	C	D	E	F	G	All
Coeff. $\times\ 10^6$	$-2{\cdot}27$	$-26{\cdot}09$	$2{\cdot}16$	$23{\cdot}66$	$-15{\cdot}01$	$3{\cdot}58$	$-105{\cdot}28$	$-4{\cdot}98$

These are the regressions on age groups, denoting age 0–49 by 1, 50–99 by 2, etc. The signs show that on the whole the occurrence of accidents decreased with higher ages. No test of significance of these regression coefficients was carried out; it would, in fact, have been illogical to do so, because we know already that the age has some effect.

c. *An Application of the Theory of the Stratified Population.*

Another promising approach can be based on a paper by M. Greenwood and G. U. Yule([1]). After showing the defects of earlier attempts the authors give, in Section III of their paper, formulae for the distribution of a population into groups of members who have suffered 0, 1, 2 . . . accidents respectively, within a given time. It happens that those results, which are relevant to our purpose, are special cases of some work which was concerned with a stationary stratified population which is subject to mortality and to promotion([2], [3]). A few remarks on this subject will perhaps be appropriate here.

Let us first consider the population of vessels before their first accident, if any.

If the rates of accident were the only operative decremental forces, then their operation could be described as follows:

Let the number of vessels without accident at time t be l_t^o and define a rate of accident by

$$\nu_{ot} = -\frac{dl_t^o}{l_t^o dt}$$

then clearly $l_t^o = l_o^o \exp(-\int_0^t \nu_{ot}dt)$, and if ν_{ot} is a constant, say, ν_o, we obtain the value $\exp(-\nu_o t)$ for the proportion of all vessels which have not had an accident.

Now in our present case there are other forces of decrement working, because vessels disappear from l_t^o for other reasons than accidents. Let us denote the number of all vessels observed at age t by l_t and introduce, as force of decrement, operating on them (and representing exits as well as disappearance through reaching the end of the period under investigation) $\mu_t = -\frac{dl_t}{l_t dt}$. This gives $l_t = l_o \exp(-\int_0^t \mu_t dt)$. Now l_t^o is itself subject to μ_t and ν_{ot}. Because $l_o = l_o^o$, and the two forces of decrement are independent, we have

$$l_t^o = l_o \exp\left[-\int_0^t(\mu_t + \nu_{ot})dt\right] = l_t \exp(-\int_0^t \nu_{ot}dt).$$

Once again, if ν_{ot} is constant, $l_t^o = l_t \exp(-\nu_o t)$. The factor l_t in lieu of l_o^o shows the difference between Greenwood and Yule's earlier approach and our present case.

Similar reasoning can be applied to vessels which have already had accidents, and Greenwood and Yule give in formulae (32A) to (32F) the basis for computing the expected frequencies of vessels with 0, 1 . . . accidents after time T, always assuming that no other decremental force exists, and moreover supposing that the forces which lead to casualties of different orders are different. They then depart from the latter assumption and we are here particularly concerned with formula (33), which distinguishes only between a rate ν_o for the first casualty, and another rate, ν_1, for all other casualties. Our own material is in any rate too scanty for further differentiation. Writing $\delta = (\nu_1 - \nu_o)t$, their formula (33) gives the following stratification after time t:

TABLE 10.

Number of casualties suffered.

0	1	2	3	4
$e^{-\nu_{ot}}$	$e^{-\nu_{ot}}\nu_o\, tf(\delta)$	$-e^{-\nu_{ot}}\nu_o\,\nu_1 t^2 f'(\delta)$	$e^{-\nu_{ot}}\nu_o\,\nu_1^2 t^3 \dfrac{f''(\delta)}{2\,!}$	$-e^{\nu_{ot}}\nu_o\,\nu_1^3 t^4 \dfrac{f'''(\delta)}{3\,!}$

N.B. $f(\delta) \equiv \dfrac{1-e^{-\delta}}{\delta}$

We can now take stock and see which observed values can be compared with theoretical values, in order to test the hypothesis that ν_{ot} is constant (but not, of course, μ_t). We have in Table I the observed values of

$$\text{(i)}\quad L_{t_i,t_{i+1}} = \int_{t_i}^{t_{i+1}} l_t dt$$

and in Table II the values of

$$\text{(ii)}\quad L^o_{t_i,t_{i+1}} = \int_{t_i}^{t_{i+1}} l_t^o dt = \int_{t_i}^{t_{i+1}} l_t \exp\left(-\int_o^t \nu_{ot} dt\right)dt.$$

Moreover, we have the casualties up to time t

$$\text{(iii)}\quad C_t = \int_o^t l_t \exp\left(-\int_o^t \nu_{ot} dt\right)\nu_{ot} dt = \int_o^t l_t^o\, \nu_{ot} dt.$$

Either part of (iii) can be used for the test whether ν_{ot} may be replaced by a constant ν_o. We can either consider

$$C_\omega = \nu_o \int_o^\omega l_t \exp(-\nu_{ot})dt - \nu_o \sum_{t_i} L_{t_i,t_{i+1}} \exp\left[-\nu_o\left(\tfrac{t_i+t_{i+1}}{2}\right)\right] \text{ (approx.)}$$

(where ω is the highest age attained) and find ν_o so that this equation is satisfied; then

$$L^o_{t_i,t_{i+1}} = L_{t_i,t_{i+1}} \exp\left[-\nu_o\left(\tfrac{t_i+t_{i+1}}{2}\right)\right] \text{ (approx.)}$$

can be compared with the observed numbers of days exposed to first casualties. Alternatively, and this is simpler, we can take $C_\omega = \nu_o L^o_{o,\omega}$, calculate ν_o from this and compare the observed values of casualties between t_i and t_{i+1} with

$$\nu_o L_{t_i,t_{i+1}} \exp\left[-\nu_o\left(\tfrac{t_i+t_{i+1}}{2}\right)\right].$$

In this case there is, of course, no reason why the total of all expected casualties should equal that of the casualties actually observed. The latter procedure was adopted for first casualties and similarly for later casualties, taking as values of ν_o and ν_1 $5{\cdot}7331 \times 10^{-4}$ and $11{\cdot}4288 \times 10^{-4}$ respectively. Table VI gives the calculated days exposed up to the times of the successive casualties, whereas VII shows the accidents which would have arisen under the hypothesis of constant ν's, together with those which actually did arise. For instance, we have multiplied 120,087, which appears in the first line of Table I, under "Totals," by $\exp(-25 \times 5{\cdot}7331 \times 10^{-4})$

= ·98577, to obtain 118,378 in Table VI. Further multiplication by $5{\cdot}7331 \times 10^{-4}$ gives 67·9 in Table VII. The agreement is bad, which is not surprising, as all the builders were lumped together. Similar calculations were therefore made for builders D and F separately, and Table VIII, which gives calculated and observed first and later casualties, shows a much better fit, although one would still say that it is very far from perfect.

d. *A Generalized Regression Analysis.*

In order to cope with the difficulty of giving due weight to the ages of the vessels under observation yet another approach may be adopted. This consists of the separation of the total population into subgroups according to the highest age attained by the individual vessels. For each subgroup the distribution according to casualties suffered was obtained and the result is given in Table V. Now if the average rate of accident were $q = \sum_x C_x/\Sigma V_x x$, and if Q_x is the ratio of casualties C_x to vessels V_x in the sub-group with vessels whose highest age attained was x, then we should expect Q_x to be approximately $= qx$. Similarly, $\Sigma\, V_x(Q_x - qx)^2$ is a measure of the goodness of fit.

At a first glance this looks like a linear regression analysis, but it will be found on closer investigation that this impression is mistaken. Let us try to fix the number of degrees of freedom. If q were taken from theory without recourse to the data, then the number of degrees of freedom would be 22. On the other hand, if q were found by the method of least squares, 21 would be the appropriate number. But the least square value of q is $\Sigma\, C_x x/\Sigma\, V_x x^2$, which is different from $\Sigma\, C_x/\Sigma\, V_x x$ given above. Thus the testing of the fit by the chi-squared method is only an approximation. It gives 105, which is large enough to reject the hypothesis without scruples, and this is in accordance with our earlier results. However, it is more interesting to investigate the exact sampling distribution of the expression

$$\sum_x V_x \left(Q_x - \frac{\Sigma\, C_x}{\Sigma\, V_x x}\right)^2$$

To obtain a slightly more general result we consider the expression $\sum_i (y_i - ca_i)^2$, where the y_i are sample values from a normal population with variance 1. We then introduce $c = \sum_k b_k y_k$, so that the quadratic expression becomes $\sum_i (y_i - a_i \Sigma b_k y_k)^2$.

Now it is known from Cochran's paper(7) that this expression is distributed as the linear form $\Sigma\, \lambda_i z_i$, where the z_i^2 vary independently as a χ^2 distribution with 1 degree of freedom and the λ_i are the non-zero latent roots of the matrix of the quadratic form. If m of the latent roots are unity and all the others are zero, then we obtain a χ^2 distribution with m degrees of freedom. Now the matrix of the form is in our case equal to MM', where

$$M = \begin{pmatrix} 1 - a_1b_1 & -a_2b_1 & \dots & -a_nb_1 \\ -a_1b_2 & 1 - a_2b_2 & \dots & -a_nb_2 \\ \vdots & \vdots & & \vdots \\ -a_1b_n & -a_2b_n & \dots & 1 - a_nb_n \end{pmatrix}$$

We notice that

$$\begin{pmatrix} 1 & a_2/a_1 & \dots & a_n/a_1 \\ 0 & 1 & \dots & 0 \\ \vdots & \vdots & & \vdots \\ 0 & 0 & \dots & 1 \end{pmatrix} M \begin{pmatrix} 1 & -a_2/a_1 & \dots & -a_n/a_1 \\ 0 & 1 & \dots & 0 \\ \vdots & \vdots & & \vdots \\ 0 & 0 & \dots & 1 \end{pmatrix}$$

$$= \begin{pmatrix} 1 - \Sigma\, a_ib_i & 0 & \dots & 0 \\ -a_1b_2 & 1 & \dots & 0 \\ \vdots & \vdots & & \vdots \\ -a_1b_n & 0 & \dots & 1 \end{pmatrix}$$

so that M is only singular when $\Sigma a_ib_i = 1$. If the b_i are taken from the least square solution, then this is the case, and it is known that a χ^2 distribution is obtained. In the case with which

we are concerned, y_i corresponds to $\sqrt{V_xQ_x}$, c to $\Sigma Q_xV_x/\Sigma V_xx$ and a_i to $x\sqrt{V_x}$, hence b_i corresponds to $\sqrt{V_x}/\Sigma V_xx$. Again $\Sigma a_ib_i = 1$, but the remaining squares do not, in general, have the same variance, and the χ^2 distribution does not apply.

A more detailed investigation of the latent roots of the matrix MM' and generalizations to include the fitting of more than one constant must be left for a later occasion.

A glance at this paper will show that it was not written at a first attempt. During its growth I have had the benefit of discussions with many friends and colleagues, some of whom were technical experts and some who were competent in statistical theory.

References.

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Appendix.

The Work Done on the Hollerith Installation.

1. *Collection of Material and Creation of the Card Files.*

Schedules were completed by the collecting section giving basic data for each vessel included in the investigation. (Specimen 1 attached.)* The schedule headings 1–25 refer to the columns of an 80–column Hollerith card on to which the information was to be punched. The schedule contains also entries which were not intended to be recorded by punching and therefore were not headed by a column number. One of them is the name of the vessel, which was often subsequently altered, making it unsuitable for purposes of identification. An appropriately defined "registered number" was used instead. Column 1 was left free altogether; the entry in column 20 was always "0."

One card was punched for every line of the schedule and thus the History Card (H.C.) file was created. This file was always kept in strict Registered Number order. The cards were later to receive the details of each casualty suffered, as the relevant reports came in.

A specimen of a H.C. is attached. A detailed explanation of the columns is given in Table A.*

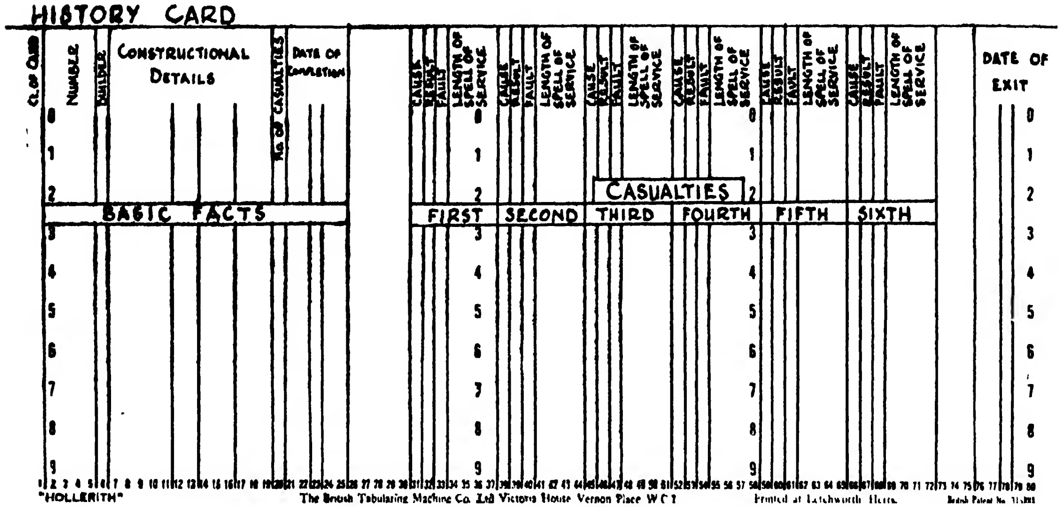
The punching of the data concerning casualties and exits on to the H.C.'s was not done directly from schedules, but by automatic reproduction from Change Cards, which were either Casualty Cards (C.C.) or Exit Cards (E.C.), and which will be described presently. However, it is necessary to point out at this stage that as casualties were recorded on the H.C., their number was punched into column 20. Thus this column, which contains only "0" as long as no casualty was reported, accumulates further entries and gives, by its last punching, the number of casualties suffered by the vessel up to date.

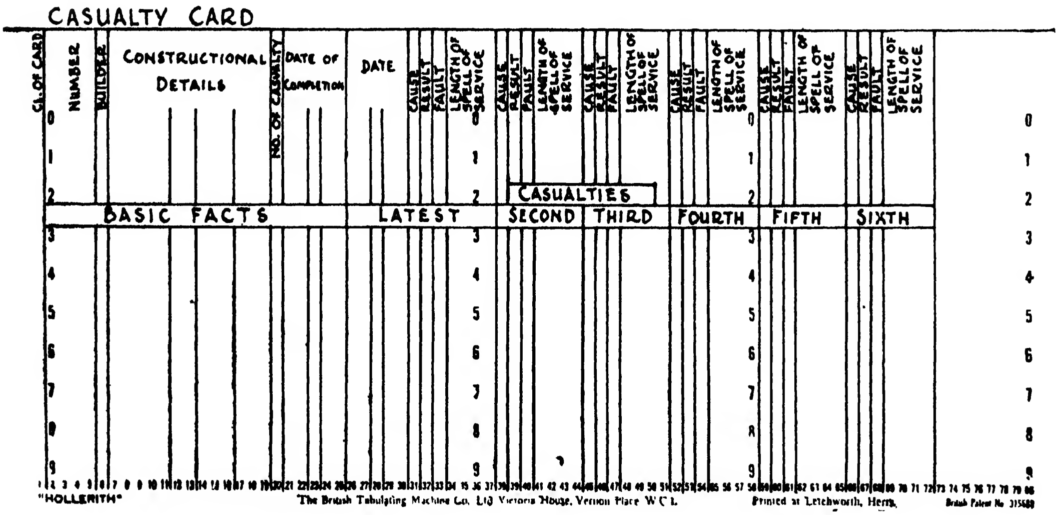
It should perhaps be mentioned that the data were collected retrospectively, but in principle it would have been possible to receive them as vessels were completed.

As the Reports of Casualties came in casualty schedules were prepared. (Specimen 2 attached.) They contain the name of the vessel and cover columns 1–5 (1 remaining blank) for identification, 20, and 26–37 for the description of the casualty. Column 20 and the last four columns (34–37) were, however, left blank by the collecting section. The Duration of Spell of Service is, in the case of the first casualty, the difference in days between the Date of Completion and the Date

* Tables A, B, C, D and E and Specimens 1 and 2, are given at the end of this Appendix.

of the Casualty. In the case of later casualties the spell of service is the difference between the previous casualty and the casualty then considered. Hence, before making any entry in these columns, the number of the casualty just reported had to be ascertained. This was done by the





Hollerith section. The H.C. card with the same Registered Number was located and the last number punched on column 20 as well as the date of the corresponding casualty was noted (but without taking the card out of its file). The next higher number was then entered on the schedule in column 20 against the casualty reported and the spell of service computed and entered in 34-37. A Casualty Card (C.C.) was punched for every line of the Casualty Schedule and thus the

C.C. file was created. It was to serve two purposes: firstly, to build up the H.C. file with casualty data, and secondly, to provide the data for statistical investigations.

A specimen of a C.C. is attached, and full details are given in Table B.

The field consisting of columns 26–37 and headed "Latest Casualty" probably requires clarification. In order to simplify casualty investigations, it was necessary to have all casualties punched in the same columns irrespective of whether 1st, 2nd or 3rd, etc. Thus, columns 31–37 will always be punched, plus a repetition (except for a first casualty) within the correct casualty field. To amplify this explanation: If one History Card is punched with casualty data up to column 51 (i.e. 3rd Casualty), then there will be three Casualty Cards in the Casualty File, each separately recording the 1st, 2nd and 3rd casualties in columns 26–37, as well as within their appropriate fields. The C.C.'s punched from the schedule do not yet contain entries in columns 6–19 and 21–25. These details are to be taken from the corresponding H.C.'s and the latter have therefore to be extracted from the H.C. file by the Collator. The next step consisted in the transfers of 6–19 and 21–25 from the H.C. to the corresponding C.C. in the same columns. This was carried out by the Reproducer, which checks, at the same time, that the two files are in identical order, so that the transfer is not made on to a wrong card. This completes the punching of the C.C.'s.

The following step was to transfer the casualty data from the C.C.'s to the H.C.'s. It was, however, necessary to treat 1st Casuatlies separately from all others; therefore the H.C.'s were sorted on column 20 and those containing only a "0" were separated from the remainder. The corresponding Casualty Cards were those which had a "1" in column 20, and these were also separated. As before, the data were transferred, in this case from C.C. to H.C., by means of the Reproducer. By inlcuding column 20 in this transfer, the H.C. always contains the number of casualties so far reported.

All that remains is to file the H.C.'s and the C.C.'s back into their respective files by means of the Collator.

Finally, for each exit an Exit Card was punched, giving the Registered Number and the Date. The latter was then transferred on to the corresponding H.C. by a process which was analogous to (though shorter than) the one just described. The exit cards were marked by a Y in column 1. i.e. by a hole in the top position.

2. *The Treatment of the Cards for the Construction of Tables*

The H.C. file was not supposed to be disturbed for any length of time, because it has always to be available for reference, and it was therefore decided that copies were to be made if any H.C.'s were to be used for the investigation. In the sequel we will only refer to the reproduced H.C.'s.

The first problem to be dealt with was the determination of the total of days of service during which every vessel was exposed to a first casualty. In this connection all vessels had to be classified according to the occurrence or not of casualties and simultaneously according to whether or not they were in service up to November 30, 1944.

The four resulting groups had to be dealt with differently. Those with casualties had the total of days of service already given in columns 34–37 of their H.C.'s. Those without casualties were, of course, blank in these columns. They were now sorted according to the date of delivery (columns 21–25), and then listed on the Hollerith Tabulator to produce the Registered Number, the day of Delivery and the day of Exit (if any). The days from completion to Exit were then worked out for exits occurring without previous casualty and the days from completion to November 30, 1944, inclusive for cards still in existence at the termination of the observations. The total of the corresponding days was then entered on all cards in columns 34–37.

The step now to be described is the construction of a double entry table for Days in Service 0–49, 50–99, . . . (to be called age groups) and builders A, B . . . G. Each cell of this table should contain (1) the number of those vessels which suffered their first causalty during the period, (2) the number of those vessels which did not suffer any casualty during their period of exposure, and (3) the total of the numbers in columns 34–37 for all cards belonging to the cell.

The cards were sorted and collected into age groups. Within each age group a further sorting

TABLE I.—

Age Group.	BUILDER. A			B			C			D		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
1	18,154	12	6·6	17,138	13	7·6	17,031	5	2·9	23,557	1	0·4
2	16,257	12	7·4	15,618	13	8·3	16,887	17	10·1	23,370	3	1·3
3	14,585	8	5·5	14,376	8	5·6	16,604	10	6·0	23,125	4	1·7
4	13,196	5	3·8	13,267	10	7·5	16,151	15	9·3	22,446	5	2·2
5	11,851	17	14·3	11,943	12	10·0	15,827	10	6·3	20,807	14	6·7
6	10,710	13	12·1	10,858	10	9·2	15,306	10	6·5	18,858	11	5·8
7	9,870	7	7·1	10,010	6	6·0	14,453	17	11·8	17,049	7	4·1
8	8,596	11	12·8	9,018	5	5·5	12,864	11	8·6	14,659	12	8·2
9	7,404	5	6·7	8,335	5	6·0	11,886	11	9·3	12,373	3	2·4
10	6,430	6	9 3	7,460	6	8·0	10,722	14	13·1	10,306	10	9·7
11	5,654	6	10 6	6,092	3	4·9	9,392	8	8·5	8,452	9	10·6
12	4,565	4	8·7	4,845	4	8·2	8,092	3	3·7	6,875	1	1·5
13	3,584	..	0·0	3,707	3	8 1	6,676	6	9·0	5,535	3	5·4
14	2,908	1	3·4	2,935	4	13·6	5,439	2	3 7	4,403	..	0·0
15	2,270	..	0·0	2,161	..	0 0	4,400	..	0·0	3,372	..	0·0
16	1,666	..	0 0	1,512	..	0 0	3,247	..	0·0	2,205	..	0·0
17	1,010	..	0·0	1,063	..	0·0	2,412	2	8·3	1,121	..	0·0
18	428	..	0·0	578	1	17·3	1,706	..	0·0	522	..	0·0
19	30	..	0·0	138	1	72 5	617	..	0·0	230	..	0·0
20	..	..	..	..	..	..	178	..	0·0	89	..	0·0
21	..	..	..	..	..	..	13	..	0·0	11	..	0·0
22	..	..	..	..	..	..	..	..	..	..	..	..
Totals	139,168	107	7·7	141,054	104	7·4	189,903	141	7·4	219,365	83	3·8

TABLE II.—

1	17,950	11	6·1	16,910	13	7·7	16,835	5	3·0	23,531	1	0·4
2	15,404	10	6 5	14,593	11	7·5	16,118	17	10 5	23 236	3	1·3
3	13,420	8	6·0	13,110	5	3·8	15,436	7	4·5	22,896	3	1·3
4	11,743	3	2 6	11,626	9	7·7	14,537	10	6 9	22,122	4	1·8
5	9,959	15	15 1	10,040	7	7·0	13,779	8	5 8	19,988	12	6·0
6	8,410	8	9·5	8,596	7	8·1	12,926	6	4·6	17,749	9	5·1
7	7,560	3	4·0	7,600	5	6·6	11,713	11	9·4	15,537	5	3·2
8	6,219	9	14 5	6,574	2	3·0	9,723	6	6·2	12,982	6	4·6
9	4,804	3	6·2	5,977	2	3 3	8,565	6	7·0	10,548	1	0·9
10	3,886	3	7 7	5,174	2	3·9	7,357	8	10·9	8,307	6	7·2
11	3,255	3	9 2	4,103	2	4 9	6,342	6	9·5	6,336	5	7·9
12	2,678	2	7·5	3,110	1	3·2	5,258	2	3·8	5,011	1	2·0
13	1,956	..	0 0	2,111	1	4·7	4,242	5	11·8	3,952	3	7·6
14	1,562	1	6·4	1,395	1	7 2	3,318	1	3·0	3,208	..	0·0
15	1,240	..	0·0	1,051	..	0·0	2,738	..	0·0	2,524	..	0·0
16	985	..	0·0	731	..	0·0	2,143	..	0·0	1,720	..	0·0
17	642	..	0·0	528	..	0·0	1,585	2	12·6	924	..	0·0
18	341	..	0·0	275	..	0 0	1,062	..	0·0	453	..	0·0
19	30	..	0 0	71	1	140 8	310	..	0·0	180	..	0·0
20	..	..	..	..	..	..	67	..	0·0	87	..	0·0
21	..	..	..	..	..	..	13	..	0·0	11	..	0·0
22	..	..	..	..	..	..	..	..	..	..	..	..
Totals	112,044	79	7·1	113,575	69	6·1	154,067	100	6·5	201,302	59	2·9

TABLE III.—

1	204	1	49·0	228	..	0·0	196	..	0·0	26	..	0·0
2	853	2	23·4	1,025	2	19·5	769	..	0·0	134	..	0·0
3	1,165	..	0·0	1,266	3	23 7	1,168	3	25 7	229	1	43·7
4	1,453	2	13·8	1,641	1	6·1	1,614	5	31·0	324	1	30·9
5	1,892	2	10·6	1,903	5	26·3	2,048	2	9·8	819	2	24·4
6	2,300	5	21·7	2,262	3	13 3	2,380	4	16 8	1,109	2	18·0
7	2,310	4	17·3	2,410	1	4·1	2,740	6	21·9	1,512	2	13·2
8	2,377	2	8·4	2,444	3	12·3	3,141	5	15 9	1,677	6	35·8
9	2,600	2	7·7	2,358	3	12·7	3,321	5	15·0	1,825	2	10·9
10	2,544	3	11·8	2,286	4	17 5	3,365	6	17·8	1,999	4	20·0
11	2,399	3	12·5	1,989	1	5 0	3,050	2	6·5	2,116	4	18·9
12	1,887	2	10·6	1,735	3	17·3	2,834	1	3·5	1,864	..	0·0
13	1,628	..	0·0	1,596	2	12·5	2,434	1	4·1	1,583	..	0·0
14	1,346	..	0·0	1,540	3	19·5	2,121	1	4·7	1,195	..	0·0
15	1,030	..	0·0	1,110	..	0·0	1,662	..	0·0	848	..	0·0
16	681	..	0·0	781	..	0·0	1,104	..	0 0	485	..	0·0
17	368	..	0·0	535	..	0·0	827	..	0·0	197	..	0·0
18	87	..	0·0	303	1	33·0	644	..	0·0	69	..	0·0
19	..	..	..	67	..	0 0	307	..	0·0	50	..	0·0
20	..	..	..	..	..	..	111	..	0·0	2	..	0·0
21	..	..	..	..	..	..	..	..	..	..	..	..
22	..	..	..	..	..	..	..	..	..	..	..	..
Totals	27,124	28	10·3	27,479	35	12·7	35,836	41	11·4	18,063	24	13·3

(1) = Days exposed. (2) = Casualties.

All Casualties.

Age Groups.	E			F			G			Totals.		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
1	16,515	13	7·9	17,892	9	5·0	9,800	24	24·5	120,087	77	6·4
2	16,369	16	9·8	17,309	5	2·9	8,997	6	6 7	114,807	72	6·3
3	16,125	15	9·3	16,515	3	1·8	8,207	4	4·9	109,537	52	4·7
4	15,788	21	13·3	15,504	3	1·9	7,585	4	5 3	103,937	63	6·1
5	15,591	15	9·6	14,435	6	4·2	6,710	6	8·9	97,164	80	8·2
6	15,351	20	13·0	13,224	1	0·8	5,718	4	7·0	90,025	69	7·7
7	14,848	18	12 1	12,171	3	2·5	5,134	6	11·7	83,535	64	7·7
8	13,567	17	12·5	10,418	2	1 9	4,328	5	11·5	73,450	63	8·6
9	12,250	12	9·8	8,869	..	0·0	3,670	2	5·4	64,787	38	5·9
10	10,588	13	12 3	7,457	3	4·0	3,061	..	0·0	56,024	52	9·3
11	9,222	9	9·8	6,186	2	3·2	2,480	2	8·1	47,478	30	8·2
12	8,010	13	16·2	5,195	2	3·8	1,790	..	0·0	39,372	27	6·9
13	6,560	7	10·7	4,064	2	4 9	1,106	..	0·0	31,241	21	6·7
14	5,246	1	1·9	3,318	4	12·0	660	1	15·2	21,909	13	5·2
15	4,146	4	9·6	3,006	1	3 3	360	..	0 0	19,715	5	2·5
16	3,170	1	3·2	2,758	2	7·3	204	..	0·0	14,762	3	2·0
17	2,362	2	8·5	2,050	..	0·0	95	..	0 0	10,113	4	4·0
18	1,695	..	0 0	1,320	..	0·0	49	..	0·0	6,298	1	1·6
19	971	..	0 0	578	..	0·0	..	..	..	2,564	1	3 9
20	315	..	0·0	255	..	0·0	..	..	..	837	..	0·0
21	10	..	0 0	146	..	0·0	..	..	..	180	..	0·0
22	..	..	..	16	..	0·0	..	..	..	16	..	0·0
Totals	188,708	197	10 4	162,686	49	3·0	69,954	64	9 1	1,110,838	744	6·7

First Casualties.

1	16,155	13	8·0	17,665	9	5·1	9,097	21	23·1	118,143	73	6·2
2	15,378	13	8 5	16,705	5	3·0	7,882	5	6·3	109,316	64	5·9
3	14,675	12	8·2	15,745	3	1·9	6,972	2	2·9	102,254	40	3·9
4	13,483	19	14·1	14,692	2	1·4	6,241	4	6·4	94,444	51	5·4
5	12,573	10	8·0	13,358	6	4 5	5,211	5	9·6	84,908	63	7·4
6	11,720	15	12 8	11,993	1	0·8	4,242	4	9·4	75,636	50	6·6
7	10,823	9	8 3	10,903	2	1·8	3,636	4	11·0	67,772	39	5·8
8	9,316	10	10·7	9,327	2	2 1	2,933	4	13·6	57,074	39	6·8
9	7,916	4	5·1	7,888	..	0·0	2,256	2	8·9	47,954	18	3·8
10	6,331	7	11 1	6,494	3	4·6	1,815	..	0·0	39,364	20	7·4
11	5,163	4	7 7	5,293	2	3·8	1,390	..	0 0	31,882	22	6·9
12	4,267	5	11 7	4,365	..	0 0	935	..	0·0	25,624	11	4·3
13	3,293	2	6 1	3,341	2	6·0	578	..	0·0	19,473	13	6·7
14	2,618	..	0 0	2,537	3	11·8	286	..	0·0	14,924	6	4·0
15	2,141	3	14·0	2,251	1	4 4	166	..	0·0	12,111	4	3·3
16	1,696	1	5 9	1,988	2	10·1	127	..	0 0	9,390	3	3·2
17	1,211	1	8 3	1,410	..	0 0	45	..	0·0	6,345	3	4·7
18	940	..	0·0	925	..	0·0	..	..	..	3,996	..	0·0
19	498	..	0 0	340	..	0·0	..	..	..	1,429	1	7·0
20	172	..	0·0	191	..	0 0	..	..	..	517	..	0·0
21	10	..	0 0	110	..	0·0	..	..	..	114	..	0 0
22	..	..	..	16	..	0 0	..	..	..	16	..	0·0
Totals	140,379	128	9·1	147,537	43	2·9	53,812	51	9·5	922,716	529	5·7

Further Casualties.

1	360	..	0·0	227	..	0·0	703	3	42·7	1,044	4	20·6
2	991	3	30·3	604	..	0·0	1,115	1	9·0	5,491	8	11·6
3	1,450	3	20 7	770	..	0 0	1,235	2	16·2	7,283	12	16·5
4	2,305	2	8·7	812	1	12·3	1,344	..	0 0	9,493	12	12·6
5	3,018	5	16·6	1,077	..	0·0	1,499	1	6 7	12,256	17	13·9
6	3,631	5	13·8	1,231	..	0·0	1,476	..	0·0	14,389	19	13·2
7	4,025	9	22·4	1,268	1	7·9	1,498	2	13·3	15,763	25	15·8
8	4,251	7	16·5	1,091	..	0·0	1,395	1	7 2	16,376	24	14·6
9	4,334	8	18·4	981	..	0·0	1,414	..	0·0	16,833	20	11·9
10	4,257	6	14·1	963	..	0 0	1,240	..	0·0	16,660	23	13·8
11	4,059	5	12·3	893	..	0·0	1,090	2	18 3	15,596	17	10·9
12	3,743	8	21·4	830	2	24·1	855	..	0·0	13,748	16	11·6
13	3,276	5	15·3	723	..	0·0	528	..	0·0	11,768	8	6·8
14	2,628	1	3·8	781	1	12·8	374	1	26·7	9,985	7	7·0
15	2,005	1	5 0	755	..	0·0	194	..	0·0	7,604	1	1·3
16	1,474	..	0·0	770	..	0·0	77	..	0·0	5,372	..	0·0
17	1,151	1	8·7	640	..	0·0	50	..	0·0	3,768	1	2·6
18	755	..	0·0	395	..	0·0	49	..	0·0	2,302	1	4·3
19	473	..	0·0	238	..	0·0	..	..	.	1,135	..	0·0
20	143	..	0·0	64	..	0·0	..	..	..	320	..	0·0
21	..	..	..	36	..	0·0	..	..	..	36	..	0·0
22	..	..	..	..	..	..	..	..	..	..	..	..
Totals	48,329	69	14·3	15,149	5	3·3	16,142	13	8·0	188,122	215	11·4

(3) = Rate of casualties.

on column 6 took place. The groups were then separately fed into the Tabulator, and the result for the first group (0–49 days of exposure) is here given as an example:

First Casualties

Days of Exposure, 0–49.

Yard.	Number of casualties.	Number of "No casualties."	Total days.
A	11	45	1750
B	13	37	1510
C	5	4	185
D	1	4	131
E	13	2	355
F	9	9	515
G	21	16	847
	73	117	5293

In this way all the columns of the table were produced on the Tabulator. The figures were then entered by hand into a schedule and the cross totals worked out and checked (Table C).

A slightly different procedure had to be followed for the analysis of the aggregate of all casualties. The cards referring to vessels with casualties show in columns 34–37 the number of days up to the first casualty; this period is, however, not the total period of exposure. The reproduced H.C.'s already used in the analysis of first casualties could not, therefore, be used now, but had to be replaced by others, which show, in columns 34–37, the total of days exposed up to November 30, 1944 (incl.) (if there was no earlier exit), or up to exit if such occurred (modified H.C.'s).

To understand the necessity for the altered method of procedure it must be remembered that the cards dealt with have been essentially reproduced History Cards, and that all casualties to the same vessel are recorded on the same card. But now *all* casualties are to be counted and registered appropriately, and it was therefore necessary to provide new Casualty Cards, which differ from the original C.C.'s in that they should all show in columns 34–37, not the total of days between the last but one and the last casualty, but the whole period from completion of the vessel to the casualty considered. This number is, of course, the total of the numbers appearing in columns 34–37, 41–44, 48–51, 55–58, 62–65, 69–72 on the H.C., the addition being carried out up to and including the field referring to the casualty for which the new card is being constructed.

The preparation of the data for punching into columns 34–37 of the "modified C.C." was carried out by means of the Rolling Total Tabulator. Owing to the limited number of counters (namely six), the totals for vessels with six casualties had to be made by hand. These tabulations were used as schedules for punching.

The construction of a table corresponding to Table C was now made in two steps. The first step gave, for every combination of builder and age group, the total of days exposed, and the second step gave the casualties for the same combinations. This time, if a vessel had suffered two casualties, say, after 95 and 208 days, and had its exit after 852 days, then it would be counted as one casualty in group 2 (50–99 days) and one in group 5 (200–249 days), and would count for 852 days exposed in group 18 (850–899 days' exposure). The final result is given in Table D.

So far we have constructed tables which subdivide the vessels into groups according to their *total length* of observation. This led to Table V. In view of problem (ii), however, we are also interested in events happening *at* certain ages, and for this purpose we have to find out how many vessels were observed at ages 0–49 days, 50–99 days, etc.

Such a table had to be worked out for First Casualties, for All Casualties, and under each of these headings for each builder separately. We illustrate its construction for the total of All Casualties. Let us consider the row totals of Table D and take, as an example, the fifth line. There were altogether 99 vessels, every one of them exposed for a total period of not less than

200 and not more than 249 days. The total of all days exposed of these vessels was 22,564 days. We want to calculate in respect of all vessels the total of all days which were spent at ages from 200 to 249 days. Hence we must first take away from the 22,564 days mentioned above those 99 × 200 days which were spent at earlier ages. To the resulting difference we must add the days spent at age 200–249 days of all vessels which grew older and are recorded in the lower lines. Therefore we add all these vessels (169 + 83 + . . . + 1, i.e. 1888) and multiply their number by 50. This gives 94,400. The number of all days at ages 200–249 is therefore finally 22,564 − 19,800 + 94,400 = 97,164. In this way the whole table was re-shaped. Table E shows the computations for "All Casualties."

Similar computations were made for each of the builders, and also in respect of "First Casualties." The results are incorporated in Tables I and II. Finally, to obtain the similar Table III for "Later Casualties," the numbers in II were subtracted from those in I and the ratios recalculated.

TABLE IV.

Frequency Distribution of Intervals

(1).	(2).	(1).	(2).	(1).	(2).
0–9	24	300–309	15	600–609	5
10–19	12	310–319	9	610–619	1
20–29	31	320–329	6	620–629	3
30–39	35	330–339	11	630–639	3
40–49	23	340–349	7	640–649	2
50–59	29	350–359	13	650–659	4
60–69	32	360–369	8	660–669	0
70–79	21	370–379	9	670–679	1
80–89	20	380–389	5	680–689	1
90–99	19	390–399	12	690–699	0
				700–709	2
100–109	16	400–409	2	710–719	2
110–119	13	410–419	3	720–749	0
120–129	14	420–429	6	750–759	2
130–139	17	430–439	7	760–779	0
140–149	18	440–449	5	780–789	1
150–159	11	450–459	8	790–799	0
160–169	12	460–469	8	800–809	1
170–179	15	470–479	6	810–819	1
180–189	10	480–489	7	820–829	0
190–199	15	490–499	3	830–839	1
200–209	20	500–509	7	840–909	0
210–219	20	510–519	4	910–919	1
220–229	10	520–529	3		
230–239	17	530–539	6	Total	744
240–249	10	540–549	3		
250–259	17	550–559	1		
260–269	14	560–569	1		
270–279	14	570–579	5		
280–289	8	580–589	1		
290–299	12	590–599	3		

(1) Intervals in days.

(2) Frequency with which the intervals, specified in col. (1), preceded an accident.

TABLE V.

Total exposure of vessel. Days.	Vessels having had the following number of casualties within this time. 0.	1.	2.	3.	4.	5.	6.	Total of casualties.	Number of vessels.	10,000 × Ratio casualties ÷ vessels.
0–49	117	1	..	..	..	..	..	1	118	84·75
50–99	67	3	..	..	..	..	..	3	70	428·57
100–149	126	3	1	..	..	..	..	5	130	384·62
150–199	137	5	..	..	..	..	..	5	142	352·11
200–249	88	10	1	..	..	..	..	12	99	1212·12
250–299	154	12	3	..	..	..	..	18	169	1065·09
300–349	69	13	1	..	..	..	..	15	83	1807·23
350–399	208	22	5	..	1	..	..	36	236	1525·42
400–449	195	20	8	1	..	..	..	39	224	1741·07
450–499	83	15	4	..	..	..	..	23	102	2254·90
500–549	140	48	13	2	..	1	..	85	204	4166·67
550–599	136	45	13	2	3	1	..	94	200	4700·00
600–649	64	17	10	2	..	..	..	43	93	4623·66
650–699	75	36	10	3	3	1	..	82	128	6406·25
700–749	59	42	17	7	2	..	1	111	128	8671·88
750–799	37	11	6	1	1	..	..	30	56	5357·14
800–849	63	33	8	5	..	..	..	64	109	5871·56
850–899	64	17	10	..	1	..	..	41	92	4456·52
900–949	20	9	5	1	..	..	..	22	35	6285·71
950–999	10	10	2	..	..	..	..	14	22	6363·64
1000–1049	5	1	..	..	..	..	..	1	6	1666·67
1050–	1	..	..	..	..	..	..	..	1	..
Totals	1918	373	117	24	11	3	1	744	2447	

TABLE VI.

Assumption: Accident rate independent of age. Calculated days exposed to

Ages in days.	Casualties. 1st.	2nd.	3rd.	4th.	5th.	6th.	Later.	Totals.
0–49	118,378	1,685	24	0	..	..	..	120,087
50–99	109,975	4,629	197	6	0	..	..	114,807
100–149	101,962	7,053	497	24	1	..	..	109,537
150–199	94,016	8,978	882	58	3	..	..	103,937
200–249	85,405	10,340	1,301	111	7	0	..	97,164
250–299	76,893	11,222	1,718	177	14	1	..	90,025
300–349	69,335	11,793	2,123	259	23	2	..	83,535
350–399	59,241	11,468	2,370	333	35	3	0	73,450
400–449	50,776	10,989	2,561	406	49	5	1	64,787
450–499	42,668	10,180	2,639	467	62	7	1	56,024
500–549	35,138	9,141	2,606	508	75	9	1	47,478
550–599	28,315	7,960	2,473	527	85	11	1	39,372
600–649	21,833	6,582	2,212	511	89	12	2	31,241
650–699	16,916	5,435	1,962	488	92	14	2	24,909
700–749	13,012	4,430	1,709	455	92	15	2	19,715
750–799	9,467	3,400	1,395	397	86	15	2	14,762
800–849	6,302	2,378	1,034	312	72	13	2	10,113
850–899	3,813	1,507	691	220	54	11	2	6,298
900–949	1,509	622	300	101	26	5	1	2,564
950–999	479	205	104	37	10	2	0	837
1000–1049	99	45	24	9	2	1	0	180
1050–	9	4	2	1	0	0	0	16
Totals	945,541	130,046	28,824	5,407	877	126	17	1,110,838

Table VII.

All Builders.

Ages.	Expected casualties.							Observed casualties.						
Days.	1st.	2nd.	3rd.	4th.	5th.	6th.	Totals.	1st.	2nd.	3rd.	4th.	5th.	6th.	Totals.
0–49	67·9	1·93	0·03	0·00	0 00	0·00	69·9	73	4	..	..	..	..	77
50–99	63·0	5·29	0·23	0·01	0·00	0·00	68·5	64	7	1	..	..	..	72
00-149	58·5	8·06	0·57	0·03	0·00	0·00	67·2	40	10	1	1	..	..	52
50–199	53 9	10·26	1·01	0 07	0·00	0·00	65·2	51	8	4	..	..	..	63
00–249	49·0	11·82	1·49	0·13	0 01	0·00	62·5	63	15	2	..	..	..	80
50-299	44·1	12·83	1·96	0·20	0·02	0·00	59·1	50	13	5	1	..	..	69
00–349	39 7	13·48	2·43	0·30	0·03	0·00	55·9	39	23	1	1	..	..	64
50–399	34·0	13 11	2·71	0·38	0·04	0·00	50 2	39	18	3	3	..	..	63
00–449	29·1	12·56	2 93	0·46	0·06	0·01	45·1	18	11	5	3	1	..	38
50–499	24·5	11·63	3 02	0·53	0·07	0·01	39·8	29	15	5	1	2	..	52
00-549	20·1	10·45	2·98	0·58	0·09	0·01	34·2	22	10	4	2	1	..	39
50–599	16·2	9·10	2 83	0 60	0·10	0·01	28 8	11	9	3	3	..	1	27
00–649	12·5	7·52	2·53	0·58	0·10	0 01	23·2	13	6	2	..	..	..	21
50–699	9·7	6·21	2·24	0·56	0 11	0·02	18·8	6	4	..	..	.	..	10
700–749	7·5	5 06	1·95	0·52	0·11	0·02	15·2	4	1	3	..	..	..	8
750-799	5·4	3·89	1·59	0·45	0·10	0·02	11·5	3	..	..	..	..	..	3
800–849	3·6	2·72	1·18	0·36	0·08	0·01	8 0	3	1	..	..	..	..	4
850-899	2·2	1 72	0·79	0 25	0·06	0·01	5·0	..	1	..	..	..	..	1
900-949	0·9	0 71	0·34	0·12	0·03	0·01	2·1	:	..	..	..	..	..	1
950–999	0·3	0·23	0·12	0·04	0·01	0·00	7	..	..	..	..	..	..	..
00–1049	0·1	0 05	0·03	0·01	0·00	0·00	·2	..	..	..	..	..	..	..
50–	0·0	0·00	0·00	0·00	0 00	0·00	0	..	..	..	..	..	.	..
	542 2	148 63	32·96	6·18	1·02	0·14	731·1	529	156	39	15	4	1	744

Table VIII.

Assumption: Accident Rate Independent of Age.

Ages in days.	Builder D. First. Casualties. Calculated.	Builder D. First. Observed.	Builder D. Later. Calculated.	Builder D. Later. Observed.	Builder F. First. Casualties. Calculated.	Builder F. First. Observed.	Builder F. Later. Calculated.	Builder F. Later. Observed.
0–49	6·8	1	0·2	0	5·2	9	0·0	0
50–99	6·7	3	0·7	0	5·0	5	0·1	0
100–149	6·5	3	1·1	1	4·7	3	0·2	0
150–199	6·2	4	1·5	1	4·3	2	0·3	1
200–249	5·7	12	1·8	2	4·0	6	0·3	0
250–299	5·1	9	1·9	2	3·6	1	0·3	0
300–349	4·5	5	2·1	2	3·2	2	0·4	1
350–399	3·8	6	2·0	6	2·7	2	0·4	0
400–449	3·2	1	1·9	2	2·3	0	0·3	0
450–499	2·6	6	1·8	4	1·9	3	0·3	0
500–549	2·1	5	1·6	4	1·6	2	0·3	0
550–599	1·7	1	1·4	0	1·3	0	0·3	2
600–649	1·3	3	1·2	0	1·0	2	0·2	0
650–699	1·1	0	1·0	0	0·8	3	0·2	1
700–749	0·8	0	0·9	0	0·7	1	0·2	0
750–799	0·5	0	0·6	0	0·6	2	0·2	0
800–849	0·3	0	0·3	0	0·5	0	0·1	0
850–899	0·1	0	0·2	0	0·3	0	0·1	0
900–949	0·1	0	0·1	0	0·1	0	0·0	0
950–999	0·0	0	0·0	0	0·1	0	..	0
1000–1049	..	0	..	..	0·0	0	..	0
1050–	..	..	..	..	..	0	..	..
Totals	59·1	59	22·3	24	43·9	43	4·2	5

SPECIMEN 1.

Schedule for Punching. *Basic Data for each Vessel.*

	Name.	(3).	Reg. no.				Builder.	(1).	(2) Constructional details.														(3).	Date of completion.				
		1	2	3	4	5		6	7	8	9	10	11	12	13	14	15	16	17	18	19		20	21	22	23	24	25
1																							0					
2																							0					
3																							0					
4																							0					
5																							0					

(1) Enter code for builder. (2) Enter code for constructional details. (3) No entries to be made here.

SPECIMEN 2.

Schedule for Punching. *Casualties.*

	Name.	(1).	Registered No.				(5).	Date of casualty.					(2).	(3).	(4).	(5). Duration of spell of service.			
		1	2	3	4	5	20	26	27	28	29	30	31	32	33	34	35	36	37
1																			
2																			
3																			
4																			
5																			

(1) No entry to be made here. (2) Enter code for "Cause." (3) Enter code for "Result." (4) Ender code for "Fault." (5) No entry to be made here by the collecting section.

TABLE A.

Explanation of All Columns and Headings on History Cards

Column.	Heading.	
1	Class of Card	This will always be punched "0" as a means of identification.
2–5	Number	The Registered No.
6	Builder	
7–19	Contructional Details	
20	Number of Casualties	Herein is recorded the number of casualties to this particular vessel. Where no casualties have occurred, there will only be a "0" punched. As further casualties occur, "1," "2," etc., will be added in the same column.
21–25	Date of Completion	Day (2), Month (1), Year (2).
26–30	..	These columns are to remain unpunched on the History Card. (Compare Casualty Card, Table B.)
31–37	1st Casualty	Details of the Cause (1), Result (1), Fault (1) and Length of Spell of Service (4) in days between Date of Completion and First Casualty or between each Casualty. This is punched from the Casualty Card.
38–44	2nd "	
45–51	3rd "	
52–58	4th "	
59–65	5th "	
66–72	6th "	
73–75		Unpunched.
76–80	Date of Exit	Day (2), Month (1), Year (2).

Note.—Cols. 34–37 had slightly altered meanings on History Cards referred to in the next section, viz.:
Reproduced H.C.:
For vessels which suffered a casualty: Meaning as above.
For vessels without casualty: Meaning altered to "highest age attained, in days."
Modified H.C. for *all* vessels, meaning altered to "highest age attained, in days."

Tables B, C and D (see pp. 162 and 163).

Table E.

All Casualties.

Age.	(1). Numbers of vessels.	(2). (a) Sum of numbers.	(3). Days exposed.	(4). (b) Days exposed of vessels in higher category.	(5). (c) Days exposed belonging to lower category.	(6). Days exposed at given age (3) + (4) − (5).
0–49	118	(2447)	3,637	116,450	..	120,087
50–99	70	2329	5,357	112,950	3,500	114,807
100–149	130	2259	16,087	106,450	13,000	109,537
150–199	142	2129	25,887	99,350	21,300	103,937
200–249	99	1987	22,564	94,400	19,800	97,164
250–299	169	1888	46,325	85,950	42,250	90,025
300–349	83	1719	26,635	81,800	24,900	83,535
350–399	236	1636	86,050	70,000	82,600	73,450
400–449	224	1400	95,587	58,800	89,600	64,787
450–499	102	1176	48,224	53,700	45,900	56,024
500–549	204	1074	105,978	43,500	102,000	47,478
550–599	200	870	115,872	33,500	110,000	39,372
600–649	93	670	58,191	28,850	55,800	31,241
650–699	128	577	85,659	22,450	83,200	24,909
700–749	128	449	93,265	16,050	89,600	19,715
750–799	56	321	43,512	13,250	42,000	14,762
800–849	109	265	89,513	7,800	87,200	10,113
850–899	92	156	81,298	3,200	78,200	6,298
900–949	35	64	32,614	1,450	31,500	2,564
950–999	22	29	21,387	350	20,900	837
1000–1049	6	7	6,130	50	6,000	180
1050–1099	1	1	1,066	..	1,050	16
Totals	2447	..	1,110,838	1,050,300	1,050,300	1,110,838

(a) Summation from the bottom upwards.
(b) I.e. number in column (2), one line further down, multiplied by 50 (e.g. 50 × 2259 = 112,950).
(c) I.e. numbers in column (1) multiplied by 50, 100, 150, 200, etc., up to 1100 (e.g. 1000 × 6 = 6000).

TABLE B.—

Column.	Heading.	
1 .	Class of Card . .	Casualty cards will be punched "X" (the position above the "0" hole). This leaves the 1–9 range of punching for special investigation designations where separate card files are created.
2–5 .	Number . . .	As recorded on, and punched from the History Card.
6 .	Builder . . .	
7–19 .	Constructional Details .	
20 .	Number of Casualty .	
21–25 .	Date of completion .	
26–30 .	Date of Casualty . .	The Day (2), Month (1), Year (2) of the latest Casualty.
31–37 .	Latest Casualty . .	The Cause (1), Result (1), Fault (1) and Length of Spell of Service in days (4), for the most recent Casualty. In all cases except the First Casualty the details will appear within the "Latest" Casualty columns and their own particular field.

TABLE C.—

Age Group.	BUILDER. A (1)	A (2)	A (3)	B (1)	B (2)	B (3)	C (1)	C (2)	C (3)	D (1)	D (2)	D (3)
1	1,750	11	56	1,510	13	50	185	5	9	131	1	5
2	2,204	10	30	1,993	11	28	1,368	17	19	436	3	6
3	5,770	8	47	4,060	5	33	1,836	7	11	1,446	3	11
4	5,793	3	32	6,476	9	36	3,337	10	19	6,372	4	34
5	6,709	15	30	5,240	7	23	2,479	8	11	8,138	12	36
6	7,560	8	28	8,196	7	30	6,626	6	24	14,899	9	54
7	3,210	3	10	3,200	5	10	7,113	11	22	8,637	5	27
8	13,269	9	36	7,974	2	22	14,123	6	39	21,982	6	60
9	11,854	3	28	6,427	2	15	12,815	6	30	21,648	1	58
10	4,736	3	10	6,621	2	14	7,107	8	15	13,207	6	28
11	5,655	3	11	12,453	2	24	13,642	6	26	21,186	5	41
12	11,578	2	20	13,860	1	24	12,758	2	22	16,161	1	28
13	3,756	.	6	8,761	1	14	8,712	5	14	7,502	3	12
14	6,062	1	9	7,345	1	11	12,018	1	18	10,058		15
15	3,610		5	5,851	..	8	9,488	..	13	13,874	..	19
16	3,885	..	5	2,331	..	3	6,993	.	9	7,770		10
17	5,742	..	7	4,128	..	5	9,035	2	11	14,774		18
18	7,941	..	9	4,375	..	5	15,912	..	18	6,203		7
19	930	.	1	2,771	1	3	6,510	.	7	930		1
20	..	.	..	.	..	..	967	..	1	1,937		2
21	..	.	..	..	..	..	1,013	.	1	1,011	..	1
22	.	..	..	..	.	..	..	..	..	..	..	..
Totals	112,044	79	380	113,575	69	358	154,067	100	342	201,302	59	473

(1) Number of days which the vessels enumerated in col. (3) were exposed to risk before

TABLE D.—

Age Group.	A (1)	A (2)	A (3)	B (1)	B (2)	B (3)	C (1)	C (2)	C (3)	D (1)	D (2)	D (3)
1	1,404	12	45	1,088	13	37	131	5	4	107	1	4
2	1,607	12	21	1,368	13	18	187	17	2	220	3	3
3	4,735	8	39	3,426	8	28	1,004	10	8	1,175	4	9
4	5,446	5	30	4,917	10	27	1,751	15	10	5,596	5	30
5	3,601	17	16	4,043	12	18	677	10	3	5,957	14	26
6	7,060	13	26	6,858	10	25	5,256	10	19	12,908	11	47
7	3,220	7	10	2,560	6	8	3,853	17	12	7,049	7	22
8	10,946	11	30	8,368	5	23	12,264	11	34	20,459	12	56
9	12,304	5	29	6,835	5	16	11,986	11	28	24,673	3	58
10	4,730	6	10	7,560	6	16	6,622	14	14	10,406	10	22
11	11,004	6	21	16,592	3	32	17,112	8	33	22,302	9	43
12	15,615	4	27	16,745	4	29	19,742	3	34	20,225	1	35
13	5,634	..	9	9,407	3	15	9,376	6	15	9,385	3	15
14	10,058	1	15	9,435	4	14	17,339	2	26	16,753	..	25
15	10,920	..	15	13,811	..	19	21,900	..	30	19,722	.	27
16	6,216	..	8	4,662	..	6	8,547	..	11	11,655	..	15
17	13,160	..	16	9,063	..	11	15,612	2	19	18,871	..	23
18	10,578	..	12	10,578	1	12	21,306	..	24	7,072	..	8
19	930	..	1	3,738	1	4	9,317	..	10	930	..	1
20	..	..	..	..	..	..	4,878	..	5	2,889	..	3
21	..	..	..	..	..	.	1,013	..	1	1,011	..	1
22	..	..	..	..	..	..	..	..	..	..	..	..
Totals	139,168	107	380	141,054	104	358	189,903	141	342	219,365	83	473

(1) Number of days exposed of vessels enumerated in col. (3).

Casualty Card.

Column.	Heading.	
38–44 . or	2nd Casualty	Details of Cause (1), Result (1), Fault (1) and Length of Spell of Service (4) for each Casualty.
45–51 . or	3rd ,,	
52–58 . or	4th ,,	
59–65 . or	5th ,,	
66–72 .	6th ,,	
73–80 .		Unpunched.

Note.—On the "Modified Casualty Cards" columns 34–37 signified age at casualty, in days.

'irst Casualties.

Age Group.	E (1)	E (2)	E (3)	F (1)	F (2)	F (3)	G (1)	G (2)	G (3)	Totals. (1)	Totals. (2)	Totals. (3)
1	355	13	15	515	9	18	817	21	37	5,293	73	190
2	1,178	13	16	1.255	5	17	1,132	5	15	9,506	64	131
3	2,225	12	17	3,045	3	24	2,472	2	20	20,854	40	166
4	4,333	19	25	4,592	2	25	3,141	4	17	34,044	51	188
5	2,673	10	12	4,758	6	21	4,061	5	18	34,058	63	151
6	5,420	15	20	8,193	1	30	4,892	4	18	55,786	50	204
7	5,473	9	17	4,853	2	15	2,236	4	7	34.722	39	108
8	12,866	10	35	13,577	2	37	6,633	4	18	90,421	39	247
9	11,966	4	35	14,488	..	34	5,506	2	13	90,704	18	213
10	10,381	7	22	8,494	3	18	2,365	..	5	52,911	29	112
11	12,513	4	24	13,493	2	26	5 190	..	10	84,132	22	162
12	12,217	5	21	12,165	..	21	6,335	..	11	85,074	11	147
13	7,493	2	12	9,991	2	16	1,878	..	3	48,123	13	77
14	7,318	..	11	7.287	3	11	3,086	..	6	54,074	6	81
15	7,941	3	11	4,351	1	6	716	..	1	45,861	4	63
16	6,196	1	8	3,088	2	4	777	..	1	31,040	3	40
17	6,511	1	8	12,260	..	15	1,615	..	2	54,095	3	66
18	8,840	..	10	13,275	..	15	..	..	..	56,546	.	64
19	5,598	..	6	2,790	..	3	..	..	..	19,529	1	21
20	4,872	..	5	1,941	..	2	..	..	..	9,717	..	10
21	1,010	..	1	2,060	..	2	..	..	..	5,094	..	5
22	.	..	..	1,066	..	1	.	..	..	1,066	..	1
Totals	140,379	128	331	147,537	43	361	53,812	51	202	922,716	529	2,447

uffering their first casualty (2) Casualties. (3) Vessels, listed according to age at first casualty.

1ll Casualties.

1	65	13	2	292	9	9	550	24	17	3,637	77	118
2	219	16	3	909	5	12	847	6	11	5,357	72	70
3	875	15	7	2,665	3	21	2,207	4	18	16,087	52	130
4	1,038	21	6	4,354	3	24	2,785	4	15	25,887	63	142
5	941	15	4	3,435	6	15	3,910	6	17	22,564	80	99
6	1,701	20	6	7,924	1	29	4,618	4	17	46,325	69	169
7	3,198	18	10	4,521	3	11	2,234	6	7	26,635	64	83
8	11,717	17	32	14,968	2	41	7,328	5	20	86,050	63	236
9	17,200	12	40	16.169	..	38	6,420	2	15	95,587	38	224
10	8,038	13	17	7,557	3	16	3.311	..	7	48,224	52	102
11	16,072	9	31	15,036	2	29	7,830	2	15	105,978	39	204
12	18,560	13	32	14,545	2	25	10,440	..	18	115,872	27	200
13	11,869	7	19	8,764	2	14	3,756	..	6	58,191	21	93
14	18,746	1	28	7,318	4	11	6,010	1	9	85,659	13	128
15	18,946	4	26	4,356	1	6	3,610	..	5	93,265	5	128
16	7,770	1	10	3,108	2	4	1,554	..	2	43,512	3	56
17	15,612	2	19	15,550	..	19	1,645	..	2	89,513	4	109
18	13,245	..	15	17,620	..	20	899	..	1	81,298	1	92
19	11,171	..	12	6,528	..	7	..	..	..	32,614	1	35
20	10,715	..	11	2,905	..	3	..	..	..	21,387	..	22
21	1,010	..	1	3,096	..	3	..	..	..	6,130	..	6
22	..	..	..	1,066	..	1	..	..	..	1 066	..	1
Totals	188,708	197	331	162,686	48	361	69,054	64	202	1,110,838	741	2,447

2) Casualties. (3) Vessels, listed according to highest age attained.

Discussion on Dr. Vajda's Paper

Mr. Maddex: I feel I am not a very suitable person to propose this vote of thanks or to take part in the discussion of Dr. Vajda's paper because, as a practising actuary, I confess that I have more interest in results than in methods, whereas it is quite clear Dr. Vajda is more interested in methods than in results. Nevertheless I am glad to have the opportunity of making what little contribution I can from my limited reading of the paper.

The actuary's approach to problems of this kind is severely practical. He seeks to express an experience in terms of a series of rates, breaking the rates down into their simplest components. Hence his primary concern, perhaps, is to get as good an approximation as he can to what, in the actuary's jargon, is called the "exposed to risk" and to ensure the validity of his results as a basis for forecasting.

Dr. Vajda has gone a long way along the path of the actuary. One takes considerable pleasure in watching his careful build-up of the exposed to risk, and in the extraordinarily full and valuable tables which he has produced with the aid of his Hollerith tabulation system. Whether, with the numbers involved—which are really rather small—the Hollerith system is economical as well as efficient, I do not know. I should have thought that a hand-written and hand-sort basis of tabulation might have been more flexible, but the results given in the appendices to the paper speak for themselves.

Problems of this sort are not strange to the actuary. He has developed a well-established technique, more particularly in relation to the analysis of sickness and mortality and accident experiences, but generally he has to do with relatively large samples and is not confronted with the statistical problems which are the special reason for Dr. Vajda's paper. At times, however, he has to make up his mind on similar questions in a small experience; I would instance the examination of the mortality, and still more the sickness, experience of a small Friendly Society. There it may be extraordinarily difficult to say whether or not a particular sickness experience represents a significant deviation from some standard, even if it be only the standard of the Society's own previous experience.

You have in such cases also a problem which Dr. Vajda has avoided by not considering the duration of his casualties. The sickness rate can most simply be considered as a compound of two components: an attack rate (corresponding to the author's accident rate) and a duration of claim. The theoretical problem which that presents seems somewhat intractable, and while from a practical point of view the analysis of sickness in those terms is an elementary step, I have yet to see any development of statistical theory which is at all helpful in considering it. I should like to think that Dr. Vajda's paper is a first step in that direction. However, when it comes to a critical examination of his mathematical analysis I confess that I am not—to use his own rather disarming phrase—competent in statistical theory; nor am I versed in the literature on which he draws. Indeed, when I get to that stage, like Goethe in another connection, I gaze in wonder; I do not seek to understand.

You will appreciate, therefore, that I do not share the disappointment which Dr. Vajda expresses at the fact that his tables allow conclusions to be reached almost by inspection. Disappointing as that may be to the mathematician, I should have thought it a tribute to the statistician, because, as I see statistical investigation on what may be called the non-mathematical side, its object is so to subdivide, analyse and generally pull to pieces the figures which the data present as to bring out these results; and it seems to me that that is evidence as much of statistical competence in one direction as the more subtle mathematical analysis after which Dr. Vajda strives.

I am curious to know a little more about the material which the author has used. One recognizes the limitations which he is under, but I should like to know whether the accidents with which he is concerned are solely breakdowns due to internal causes, or whether, whilst excluding war casualties, they include what might be properly called marine hazards. This is a relevant point, because it affects not only one's approach to the incidence of the casualties, but also the possible interpretation of the various analyses made. I take it that a casualty which results in permanent withdrawal is included both among the accidents and among the exits; this is to be presumed, but it was not quite clear from the paragraph at the beginning of the paper.

I would like to know a little more also about the nature and effect of the exits. In this experience it is probably not material, because they are less than 10 per cent. of the entries, but one would like to be sure that they were in no sense selective, that is to say that the withdrawal of these individuals was not in any way due to their own accident experience, whether on the favourable or the unfavourable side. The point is a commonplace to actuaries, because the influence of selective withdrawal in affecting the experience of the residual population is an element with which actuaries are only too familiar, and which they have to watch in interpreting their results.

Another point on which one would like to hear the author's justification is his method of

taking casualties as isolated occurrences in a continuous exposure running over the whole of the three years, making no deduction for the time during which a casualty is in dock, or, indeed, for the time during which vessels in the normal course are in harbour and therefore not exposed to the casualty risk. Both types of temporary withdrawal from the experience may be important in themselves, and while one accepts the author's statement on the point, I gather he is referring only to accidents and not to normal time spent in harbour. The effect of making no deduction may be small, but one would like to be assured that the repercussions have been fully considered.

It is difficult to comment without knowing more about the types of vessels, but let us suppose that merchant vessels were under consideration. Very roughly—and these are illustrative figures only—supposing the average round-voyage time is about four months, about a month of that time might be spent in port, moving cargo, turning round, bunkering, and so forth, and another ten days in repairs; you thus dispose of about a third of the round-voyage time as time during which the vessel is not exposed to risk. That is an average, but the proportion would vary greatly between vessels employed in different oceans. The North Atlantic, with a shorter round-trip and a higher marine risk, might possibly show half-time not exposed to risk. If, therefore, the vessels in the experience were engaged in different oceans, that would be a factor which might be just as important as any of the other factors considered in influencing the casualty rate.

I think this consideration is important, because the later stages of the investigation are not an analysis of isolated non-reversible incidents (like deaths for instance), but of life histories in which the individual may retire temporarily, endure a period of invalidity or other non-exposure, and return again to the active list several times before he dies or disappears (assuming he does not last out the static three years' period). It should be remembered also that the author does not follow through his experience as is done in what an actuary calls a " select investigation "; he is unable, that is, to follow it from the point of entry at completion of the vessel, year by year or month by month over the same number of months for every vessel, but has to stop at the end of his three years—a serious defect from the point of view of the specific objects of the analysis. It is difficult to believe that the large proportion of time which is spent out of active service is not a factor of importance when you come to investigate the casualty rate on a durational basis—more particularly when you look at second and later casualties, for which the members are relatively small and the casualty rates heavy. It seems to me that it is not only a question of under-estimating the casualty rate but, more important, of distorting it.

That leads to a somewhat similar point, in essence. Table 5, which is a preliminary tabulation according to builder, is very interesting but rather tantalizing, because it is not quite clear to me without further explanation precisely what those figures represent, giving durations from completion to first accident or from one accident to the next. The differences are certainly significant, but significant of what? The author has not stated, although he may have satisfied his own mind, that he has been able to eliminate or compensate for the effects of differential incidence of entry into the service, remembering the considerable seasonal variation in the casualty rate; the differences due to service in different oceans; different proportions of time which would be spent in port, on different kinds of trip, and so forth. I do feel that, from the point of view of what might be called the arithmetical statistical investigation, those are important points, and I should like to know precisely what has been done about them, and how the author assures himself and us that they did not enter into his experience in such a way as to vitiate the attempt to compare in isolation the experience with reference to builders, age and successive accidents, on the assumption that there are no other variables vitiating the comparison.

The difficulty of giving weight for age in these analyses seems to have weighed heavily on the author, and I wonder if he employed any method of standardizing his results by weighting accident rates for individual builders with the exposed to risk by age for the whole experience, and *vice versa*. You do not, of course, get conclusive results that way, but I should have thought it was a useful first step; I expect the author has done it, but if he did, one would have liked to see the results before going on from that straightforward approach to his mathematical analysis.

I should like to say what a pleasure it is to read a paper in which all the data are exposed so fully for the benefit of the reader, and in which, by the way, there is not a single table in which the numbers add up to a hundred.

Mr. E. G. Chambers: As one who has been associated with the study of accident proneness for a good many years I listened to this paper with considerable interest, and feel it should provide some very useful suggestions for people who are working in this type of field and with similar types of data.

I am not qualified to criticize the more mathematical aspects of the paper, but there are two

small points I should like to mention. First, as one closely associated with the original use of the term "accident proneness," used to denote the personal psychological qualities entering into the human susceptibility to accidents, I must enter a gentle protest against the application of the term to ships.* I say a gentle protest advisedly, because I am only too conscious of the fact that some psychologists have borrowed and mis-applied statistical techniques, and it is perhaps only fair that statisticians should borrow and possibly mis-apply psychological terminology. Sailors, I believe, claim that each vessel has its own individuality. Does this individuality extend to cover what we call "accident proneness" in human beings?

I have here a possibly very naïve suggestion: that is that the question might be examined to see whether the distribution of all casualties which Dr. Vajda gives on the first page of his paper shows a good fit to a Poisson distribution, or if it shows a better fit to the Greenwood-Yule negative binomial distribution. I have not had the actual figures long enough to try this out for myself, but, in the absence of proof that the latter theoretical distribution does give the better fit, I suggest the use of the term "accident liability" with regard to ships instead of "accident proneness."

The other point has already been touched on by Mr. Maddex—that is, that Dr. Vajda has shown a marked relationship between casualty rates and month of the year, as may be seen in Table 4. I suggest that this relationship must have a very strong effect on the time after launching before the first casualty. For example, a vessel launched in November has a much greater chance of an early casualty than one launched in, say, June. This same factor may have an effect also on the time of the first casualty of ships built by different builders. If some builders tend to launch their ships in the spring and others in the autumn (I do not know whether that is so), then the unequal distribution shown in Table 6 may be in part accounted for by the fact of the unequal distribution of casualties during the seasons of the year. In any case that factor rather seems to me to have been neglected in the analysis.

I have very much pleasure in seconding the vote of thanks.

The vote of thanks was then put to the meeting and carried unanimously.

Sir William Elderton said he was in the same difficulty, to a very large extent, as the author, because having been mixed up with shipping a good deal during the War he had to be a little careful what he said, even at that stage, so that although he had guessed a good deal about what was behind the paper, he would have to be careful not to disclose all that he had guessed.

He thought it safe to say that it looked as if the definition of a casualty must follow fairly nearly the sort of accident that might occur and be covered by insurance, plus, perhaps, a certain number of things that might not be covered. The fact that the accident rate was so much higher in winter than in summer agreed well with the general experience of all merchant shipping—that you do get a higher casualty rate and, of course, a higher loss rate, in winter than in summer. If a vessel had any weakness and had a bad shaking across the Atlantic in the sort of weather experienced lately, especially if she had had to slow up on various occasions because submarines were about, or quicken for other reasons, or suddenly turn into a different direction, then that defect would be more readily shown up in those circumstances than in smoother waters.

If you looked at these various building groups you found that they did not all run for the same period. The ships of builder "G" apparently were running for about two hundred days short of those of builder "F," and on the whole had a high rate of accident, especially in their early life. If the vessels produced by builder "F" were largely put, by chance or by design, on the route, say, from New York to India and back, and the vessels produced by builder "G" were, perhaps because some of them came on at a later stage, put on the North Atlantic route, in those circumstances even if they were all equally well built the effect on the casualties would have been on the lines of the experience because one lot would have been shaken about a good deal more than the other.

Again a casualty was counted when a vessel had a small defect, or when a vessel had a big defect; the big one would be out of action perhaps for several months, and the other one would very likely be put right while the ship was turning round and would run a chance of having a second accident very soon afterwards because she would be in service again. If, for instance, the 64 cases of the builder "G" were all casualties for long periods, they might yet have gone back again into the experience as merely a casualty and not permanently out. This might distort the rates of casualty. He thought therefore that the time when these cases happened, and the routes on which ships were employed might be examined to see what could be found from them. One little check might have been done, and probably had, namely to work out the monthly accident rate in the same way as Table 4 for each builder, and to see if different results were shown for the various builders.

* Mr. Chambers is referring to an earlier printing of Dr. Vajda's paper, in which the term "accident proneness" was used. See also the concluding section of Dr. Vajda's reply.

He had heard quite outside the Ministry with which he was at one time connected that there were some English owners who might like to buy American ships if they knew the yard in which they had been built; this implied a feeling that there was considerable variation in standard of building. But it was possible to jump to wrong conclusions. Towards the end of the 1914–18 war there had been a certain amount of quick building in American yards, and a number of people in this country had been horrified because a lot of those ships, the first time they had got across, had had to have repairs, and it was voiced abroad, to a certain extent in the ordinary Press, that this must have meant the ships were badly built; and yet a large number of the remaining ships of that group, having been used for a bit and then laid up, were used strenuously during the recent war and had done very well.

Mr. Buckland felt it was of great value to have access to these additional examples of the application of statistical techniques to problems of operational research, because he believed that operational research would have to become a part of any large organization in this rather difficult post-war world. As Dr. Vajda had implied, while the particular material under review might be of passing interest, the methods were of a more durable kind.

In his own field, at the London Passenger Transport Board, there seemed to be at least a *prima facie* case for the application of the methods of the paper, arising out of the similarity between groups of vessels and groups of vehicles; road vehicles particularly, as rail vehicles were not so much in the same class. An example had been touched upon briefly by the Government Actuary: if the definition of accidents for road vehicles was extended to mechanical failures resulting either in a delay, or taking the vehicle out of service, then it seemed to him that in the paper they had quite a number of techniques useful for the analysis of that kind of occurrence. It was a problem which was fairly high on his agenda at the moment, so he was grateful for the additional information.

The data in the paper were from the ordinary processes of operating, as distinct from the type of information which came from a series of planned experiments. He felt that was a distinction which had to be borne in mind since it did largely govern a great part of the analysis. For example, much of the analysis would fall under the heading of "making sense out of figures," which Dr. Yates had recently stated was an activity which still took up a great deal of the statistician's time. Quite early in the section devoted to the analysis of material, Dr. Vajda had stated that the observed facts did not agree with his theoretical model, so he had had to cast around for some likely explanation. This was another extremely important aspect. There were, in fact, some interesting papers in the current issue of *Biometrika* on those lines.

He would raise two points, one possibly of major interest, one minor. The first referred to accident-proneness and age. It would appear that age was to be taken as some fairly continuous function of time; what, then, was the effect of a period spent in dock? He raised this problem only because for a road vehicle, such as the Board were in the habit of operating, a period in dock did materially change its effective age. In an imaginary case, for example, a vessel—or a 'bus, in his own sphere—might become the equivalent of a new completion. Anyone who had journeyed around in the "ST" type vehicle that they still had to use on the road would realize how the maintenance and periods in dock had prolonged its effective life, if not its comfortable life.

Mr. R. E. Beard said he had found the paper of very great interest. It linked up rather closely with some work he had been doing during the war, and he wished to refer to that problem. He had been concerned with accidents to naval aircraft, and one of the problems that had to be considered was the action, if any, that should be taken if a particular pilot had more than a certain number of accidents in a given period. There were very few statistics to begin with, but a certain amount of material was built up, and the pilots were then classified by the number of accidents they had suffered in the period of a year. Unfortunately, the period of exposure was not known, and, for various reasons, the pilots with no accidents could not be accurately determined; however, details of those who had one, two, three, four, etc., accidents respectively were available. First of all the data were studied to see if they showed any obvious features. A Poisson distribution was first tried, but did not fit the data; then a generalized Poisson was tried and that did fit the data. The point then was, was there any mathematical model that could be used which would give rise to the observed distribution? There were two of consequence, and the fact that there were at least two showed the difficulties in building up mathematical models to fit the statistics. The first model was that there was a variation among the population of pilots as regards their liability to accidents, this being, of course, closely allied to the studies of Greenwood and Yule; the second assumption was that the accident proneness increased in arithmetical progression with each accident. These two basic assumptions gave rise to the same distribution for the numbers of pilots with one, two, etc., accidents.

Obviously the remedial measures in the two cases would be different. If it was demonstrated

that the more accidents experienced the more likely it was that another would be suffered, then obvious steps should be taken after a prescribed number of accidents had been suffered. On the other hand, if the reason was due to variation among the pilots, alternative tests must be devised to discover those pilots with a high proneness before they had incurred any accidents. The data, however, did not allow a differentiation between the two models, and the present paper did tackle the problem in the same way that was tried, namely by analysis of the intervals between the accidents.

Turning now to the present paper, the first reaction was to see, with regard to Table 1, whether the generalized Poisson did fit these figures. They had already heard of the difficulties and dangers inherent in the data, so the figures were quoted as a piece of arithmetic! The results were as follows:—

Number of accidents.	*Actual number of vessels.*	*Expected number of vessels.*
0	1918	1912·5
1	373	387·1
2	117	104·0
3	24	30·1
4	11	9·1
5	3	2·7
6	1	1·5

The fit was very satisfactory, and led to the suggestion that some progress had been made. Actually there was probably very little, because the period during which the different vessels were exposed differed so considerably. Some were exposed the whole of three years, and others for a lesser period, so that it was necessary to adopt a more exact investigation as developed by the author.

His next point referred to Table 5. For the whole data the average interval between accidents was 222 days, the different builders showing a range from 150 to 278 days However, reference to Table IV, which gave the frequency distribution of all the intervals, showed that it was rather a flat distribution. If Table 5 were regarded as showing samples from that distribution, a sample standard deviation could be worked out and the significance to be attached to the average intervals determined.

Owing to the spread of the data in Table IV, the standard deviation was approximately 175 days. Bearing this in mind, and having regard to the number of casualties in each group, the statement "It is obvious there are very significant differences" did require some clarification. In fact it was doubtful if there was more than one significant difference, and if worked out accurately it was possible that there would be no significant differences. There were, of course, other considerations, from which some differences might be expected to arise, but perhaps Dr. Vajda might comment on why the significance was obvious without making some sort of test.

One other small point was that Dr. Vajda had made the rather happy statement, "It would be interesting to apply some periodicity analysis to these data," but it was surely almost impossible to get anything of any use out of the data for three years. There would be, at the most, three cycles, and it would seem that little confidence could be attached to the results derived therefrom.

In conclusion he was afraid he might have sounded rather critical on minor points of the paper, but he did appreciate that it was a very valuable piece of work. Had it been before him three years before it would undoubtedly have saved him a considerable amount of analysis. He was grateful to Dr. Vajda for his paper.

Professor GREENWOOD said he had been consultant to the Admiralty in medical statistics, but did not feel that gave him authority to comment upon the practical aspects of the paper; he would like, for mainly sentimental reasons, to refer to one of the theoretical aspects.

Dr. Vajda had spoken very kindly of the paper which Mr. Yule and he had contributed to the JOURNAL twenty-six years ago, and he hoped Dr. Vajda would also read a paper which was contributed to the JOURNAL seven years later by his colleague, the late Dr. Ethel Newbold. The point was that in the earlier paper Mr. Yule and he had dealt with three hypotheses, and they had dealt with them necessarily rather superficially: the first hypothesis was that the multiple accident distribution was a chance distribution; the second that first accidents were distributed by chance, but the probability that a person who had had r accidents ($r \geqslant 1$) would have $r + 1$ accidents was not the same as the probability that a person with $r - 1$ would have r accidents; the third, that proneness, or susceptibility, varied from person to person. To the comparatively small section of the public which would be likely to read any paper having his name on it, the third possibility was very much the most exciting, and as the negative binomial gave a nice fit to many data and was easy to use, the hypothesis attracted a lot of attention. But it did seem important

that the algebraical basis should be strengthened, and that was the main point of Dr. Newbold's work.

At that time he was an enthusiastic disciple of the methods of Professor Chuprov, and Miss Newbold was also a disciple, which was the reason why the algebraical treatment was in Chuprov's notation. No doubt younger and better mathematicians could reach the results rather more expeditiously, because Chuprov's elementary methods took a little time.

The first point he desired to make was that the application of the negative binomial to the elucidation of accident proneness could only lead to valid conclusions if the data were homogeneous; one must not mix together records of different experiences, for instance of persons engaged on different processes of manufacture which entailed different risks.

His next point, perhaps only another way of stating the first point, was that if the column totals of such a Table as V on p. 158 were better fitted by a negative binomial than by a Poisson—and, as Dr. Beard had said, they were better fitted—it did *not* follow that accident proneness, in the sense of those who had used the Greenwood-Yule method in industrial research, had anything to do with the result. It was well known that the sum of a number of variables each obeying the Poisson "law" was itself a Poisson variable, but it was *not* true that if we took a number of Poisson frequency distributions and summed the 0's, 1's, 2's, etc., the resultant totals of 0's, 1's, 2's, etc., would make a Poisson distribution; in fact it was easy to see that the variance of the summed data must exceed the weighted mean of the Poisson parameters of the set by the weighted mean of the squared differences between each Poisson parameter and the weighted mean of all of them. This fact did, however, suggest a possible test. If his arithmetic were correct, the mean of Table V was 0·304 and the variance 0·457. Suppose that each horizontal array were a Poisson, what *ought* the variance to be? One can only take the means of the arrays as approximations to the parameters; doing this he made the "expected" variance 0·362—a good deal larger than 0·304, but a good deal smaller than 0·457, and, he thought, "significantly" smaller. This again was one of the results which could be reached by common sense, still, the method might sometimes be useful. He congratulated Dr. Vajda on completing so elaborate an investigation; he himself, like the author, was interested in the theory and not ashamed of that interest.

Dr. HERON wished to join those who had already spoken in thanking the author of the paper for a very interesting piece of work. He welcomed the author's explanation that the material was really homogeneous, but he would like him to go a little further than that, because it was rather important to show that it really was homogeneous; he thought he might have used some of the constructional data on the Hollerith card to indicate if the material was homogeneous, such as tonnage or speed. It was not necessary to give the actual figures, because index numbers could quite well be used and would be innocuous.

Secondly, he wished to associate himself with what had been said about the treatment of the duration of risk, because this seemed a very important factor in the analysis of accident frequencies. In the first Table they would see that one vessel had, in the three years, six casualties, and three had five casualties. In the three years those four vessels must have spent some eighteen months in dock, under repair, and yet it was assumed in the paper that they were on service during the whole three years; he thought that was a serious defect. The data must, of course, be tucked away in some water-tight compartment of the Admiralty, and he had no doubt they could be obtained, and if so they should be used.

There were two other points which might be considered; those were the effects of deferred repairs and the records of the masters of the ships concerned. It was common practice for a vessel which had sustained a casualty to be patched up at the nearest available port and then to be sent to the most convenient yard for more complete repairs. Further, in wartime, and also when freights were exceptionally high, there was a marked tendency to execute only such repairs as were necessary to make the vessel seaworthy, and to leave complete re-fit to a more convenient time. While running after temporary repairs the vessel was definitely a worse risk that it was before the casualty occurred, and this could hardly fail to affect the result of an investigation such as this.

Finally, just as it was well known that some drivers of motor-cars and some workers handling dangerous machinery were what is sometimes termed "unlucky," that is they were more likely to have accidents, so some masters or some commanding officers in Naval vessels might be unlucky in the same way, and their ships might be more likelt to suffer casualties. He thought, therefore, the records of the masters of the ships should be investigated at the same time.

Dr. SUTTON said that the paper was epitomized in paragraph 3, where it was said that the tables were rather disappointing because they allowed of conclusions almost by inspection and

left little scope for analysis, but that the paper being concerned more with methods than results, a few possible ways of extracting information would be mentioned for their possible value in future similar investigations. Nevertheless, some comments must be made on the enquiry as presented, firstly in relation to Dr. Vajda's suggestion that it might bear upon future similar investigations and, secondly, in relation to the Admiralty's possible use of the conclusions. For such purposes the information seemed to be inappropriate to the three problems stated, and it might be useful to suggest in what ways.

First, however, there must be some question as to the types of vessel involved, and in particular whether they were wholly Naval vessels. They might, of course, have included Ministry-owned vessels, but the large numbers completed at least suggested many small ones such as Naval types. If they were wholly Naval vessels then some of the following comments were not appropriate, though they might still be so in relation to other possible enquiries. The more elaborate the technique of enquiry the more must the information be appropriate to it.

The main deficiency was the definition of a casualty, which was clearly stated to be outside the duties of the Statistical Branch. The information required here was not only whether a succession of mishaps was a single casualty, but what type of mishap was called a casualty, what limits in amount were taken for decision and whether unrepaired casualties were included. Many vessels went unrepaired for long periods, sometimes unreported.

The types of casualty came under several distinct headings.

Firstly there were war casualties of the kind covered by war risk insurance policies, which he understood had been excluded. It would be interesting, however, to know on what basis war risk was defined, since this was a very subtle decision in maritime law and the dividing line could affect results materially.

Secondly there were collisions. Convoy collisions between commercial vessels were not war risks, though most people would take them to be. On the other hand, many kinds of collision could by technical decision be construed as war risks, save that Marine Insurance policies had reinstated them as Marine. During war years, such as those under review, the risk of collision was obviously made greater by very many other causes than convoys, including the dimming of both ship and shore lights, the congestion of anchorages and so on.

Thirdly were groundings, which again were made more frequent in such conditions, and particularly during the use of unusual, damaged and neglected harbours and berths.

Fourthly there were fires, which would be increased by various causes, including the relaxation of regulations and precautions, though not in Naval vessels. Sabotage was peculiarly suggestive with fires, though particularly difficult to prove to the satisfaction of war risk insurers.

Fifthly there was heavy weather, the risk of which could be materially affected by deeper loadings and by the necessity for sailing in all sorts of conditions, where normally vessels would either ease up or not proceed at all or be differently loaded.

Sixthly was damage by ice, the risk of which in ordinary times was very carefully watched.

Seventhly there were breakdowns, whether of steering or machinery, the latter of which could be either of the main engines or of auxiliaries. In this, of course, the type of main engine would have considerable bearing.

In relation to all of these the particular service of the vessels should be considered—that was, whether they were predominantly in such areas as the North Atlantic or Arctic, the Mediterranean or in specially protected waters. Consideration would have to be given also to whether their service was otherwise particularly intense, involving fatigue of crew, the taking of risks and proceeding frequently at excessive speeds. Many vessels also were not allowed to repair properly during this period, and so were often not as well fitted to cope with conditions.

Considerations of different kinds are with regard to the personnel of the vessels, both Engine and Deck, and how far negligence or variation of any standard of efficiency contributed to accidents. Of a different kind was the question of the ordered design and equipment of vessels, and even of management, if different managements should be involved.

Anyone having practical experience would readily realize that in the great majority of such casualties there could be no direct question of builders and age of vessel at all. The problem of first and subsequent accidents might have something to reveal, though in practice first casualties seemed to have no effect on later ones. The different types of casualty might be broadly common within the groups of vessels according to builders and not greatly disturb a pronounced trend, but no one would make such a suggestion boldly. The peaks in winter were readily acceptable for nearly all kinds of casualty.

The more important point, however, was that the effects of bad build and of age were ordinarily experienced indirectly rather than directly in casualties. The direct casualty, as for instance breaking in two, was the exception. The most usual types were an increased liability to heavy weather damage and to machinery breakdown, though with the latter the engine builders might

differ from the hull builders. But bad build in particular, and age eventually, showed up in three very material ways: first by constant, small, troublesome occurrences that did not rank as casualties, but were cumulatively nearly as bad; second, by the increased cost of repairing actual casualties; and thirdly by the much greater expense of survey upkeep.

The amount of damage as also the delay involved in the definition of casualties might be important, since although it was clear that large amounts would not at all closely represent significance in the sense of the enquiry, it was equally obvious that if all damage in excess of some very small amount was included as a casualty the significance of the results would be greatly affected. Here, of course, the number of casualties suggested that some moderately high standard had been taken.

The period used for analysis was also a relatively short one.

The usefulness of the mathematical technique in itself was quite a different matter, but it would certainly be of interest to know how far Dr. Vajda has already felt any of the foregoing qualifications to be desirable. He was sure the Society would agree that it was our duty to make clear both the practical use and the limitations of any methods we considered, even though it was well recognized that mathematical methods were often developed in advance of their practical application.

True knowledge transcends personal ideas, and he would, of course, be as much interested in Dr. Vajda's correction of the foregoing as in his confirmation, but if he accepted any material part of it as relevant to satisfactory conclusions, what significance did he attach to the pronounced variations between builders on the alternative possibilities that the irrelevant effects did and did not broadly average out over the comparatively large number of vessels under review? And what further significance might there be with inclusion of the important effects not shown by direct casualties? Would the Hollerith installation be competent to deal with such fuller variations, or in other words, to what degree of detail did he consider the installation appropriate? What additional complications in mathematical technique would there be to provide the valuable suggestion and the all-important check upon *prima facie* practical conclusions so often possible by mathematics?

As a more particular comment, why was it that casualties dropped so rapidly from June, 1944, onwards, in broad line with the decreasing numbers of vessels completed, but not with the continued increase in total vessels? It suggested a change of casualty conditions quite opposite to what should be an increasing effect of age and building defects.

We had, of course, to realize the restrictions on the use of data under which the author had worked, and must thank him for a bold attempt at promoting interest in a new field.

Dr. SOLOMON said his first reaction on reading the paper had been one of unqualified admiration, and his second, which he had hastily tried to overcome, was one of curiosity concerning the material involved. He had tried to suppress that reaction because this was essentially a paper on statistical technique, and he wanted to differ from most other speakers by discussing that aspect alone.

The first point he wished to make referred to a general remark in Section 3 (*a*), which gave an analysis based on the work of Mr. D. R. Cox. He felt moved to say, with reference to a group who were rather under-rated by the general statistical community, that the method was precisely equivalent to the conventional actuarial treatment, and had been extended by many actuarial students using a different nomenclature.

His next point was one of detail. In the section on the Analysis of Variance the difficulty appeared to be to obtain a valid estimate of the random variation against which the several hypotheses could be tested. The difficulty in finding such an estimate was not uncommon in contingency table analysis, and he envied the happy agricultural statisticians who never had any such difficulty, but always found some interaction or other which was physically acceptable as an estimate of random variation. Now in this case the variance of the estimate of an accident rate was, nearly enough, proportional to q itself. The hypotheses tested were of the type "there is no effect other than so-and-so." That implied that allowance was being made for the possible existence of two or more effects—that is of two or more population values of q, the accident rate. Therefore the postulate of the Paper of a single variance depending only on the exposures appeared at first sight to be unacceptable. The difficulty could of course be overcome by the standard methods of using not the accident rates themselves but their square roots, or the inverse sines of their square roots.

One possibility he might mention, although he was not at all sure of its applicability. He had had occasion recently to analyse some actuarial data, and in terms of this paper had tried to find an estimate of the random variation by a regression analysis with respect to age of first accidents of ships from a single builder. That completed, tests were carried out to ascertain whether the

estimates were sensibly uniform for all builders. With regard to the accident rates of ships, however, there were difficulties, as other speakers had shown, in finding any simple formula which expressed the dependence on age. The total life of a ship was, perhaps, twenty-five years, and the ships dealt with in the paper had been examined over a period of only three years. It might be, as others had said, that the effect was not an ageing of the ships, but was associated in some way with the skill in handling them of their masters, as they became progressively more familiar with their vessels.

In conclusion he wished to repeat that although he had learned very little about the accident rates of any class of vessel he had learned a great deal, for which he was grateful, about statistical technique.

The following comments were received in writing:

Mr. Babington Smith: Like one of the speakers in the discussion I mistrust the comment below Table 5, "It is obvious that there are very significant differences between builders."

Since the meeting I have separated the variance "between" and "within" builders in Table I and find a ratio $e^{2z} = 3{\cdot}1$ for 6 and 737 degrees of freedom. In samples from a normal universe P for such a value lies between 1 and 0·1 per cent. It is by no means clear to me that, with distributions such as are found in this paper, the usual tests of significance can be applied and interpreted at their face value. For Table II I find e^{2z} = about 6 for 6 and 522 degrees of freedom, but does such a value do more than confirm the view that we are dealing with abnormal distributions?

My second observation concerns points raised in the discussion. I noted that some speakers drew attention to reasons why one ship should be more liable to casualty than another, such as the efficiency of the master and crew or the more hazardous nature of some seas. Only one speaker, as I recall it, put such a point in a form directly relevant to Dr. Vajda's findings, by suggesting that the ships from different yards might tend to come into service at different times of year and so become exposed to risk at times of greater or lesser hazard.

Unless a cause of variation in risk is in some way associated with the builders (*i.e.*, unless ships from certain yards attract a better type of master or were allotted disproportionately often to dangerous runs) its direct effect on mean casualty rate per builder may be expected to be negligible. On the other hand, since undoubtedly such causes produce differences in casualties between one *ship* and another, they contribute to the general variance, and if their effect could be estimated, the differences between one *builder* and another might well stand out more clearly.

I should be glad to know if Dr. Vajda agrees with this view—but I should still be grateful if he would indicate more clearly why he considers it obvious that the differences between builders are significant.

Mr. Quenouille: I should like to make one or two comments on the problem raised by Dr. Vajda at the end of the first section of his paper. Dr. Vajda regretted the lack of a method for testing periodicity when different weights are attached to the observations. It seems to me that, in this case, the number of observations is so small that we should probably have to content ourselves with testing a multiple-classification table with unequal numbers in the different classes as described by Dr. Yates. However, since the problem appears to be frequently encountered when it is necessary to test percentages and other weighted observations for oscillatory movements, I believe that some remarks might be helpful.

Suppose that the observations $x_1 \quad . \quad . \quad . \quad x_n$ have weights $w_1 \quad . \quad . \quad . \quad w_n$ and that the variance of $w_i x_i$ is $w_i\sigma^2$. Suppose also that the weights arise in such a manner that the estimate $\Sigma w_i x_i/\Sigma w_i$ of the mean is unbiased. It is not difficult to see that these suppositions will not hold if the weights are not representative. For example, if we were observing the accident rate on enemy vessels, our probability of observations would be greatest in summer when the accident rate would be lowest, so that the weighted mean would be biased downward. The detection of such a bias assumes some *a priori* knowledge of what constitutes a representative set of weights, *e.g.*, equal ship-days exposed in each month, and this knowledge must form the basis for an unbiased estimate of the mean.

It should be noted that in addition to correcting the observations x_i for the mean, we may also wish to take out a regression on w_i. By this means we can investigate whether any oscillatory movement in the accident rate arises solely from a similar oscillatory movement in the number of vessels operating.

Suppose that we wish to test for periodicity, and that

$$X_i = w_i(x_i - \bar{x}),\ A = \frac{2}{\Sigma w_i} \Sigma X_i \cos \frac{2\pi i}{\lambda} \text{ and } B = \frac{2}{\Sigma w_i} \Sigma X_i \sin \frac{2\pi i}{\lambda}.$$

Then, for large n, if x_1 . . . x_n are random elements from a normal population,

$$\text{var } X_i \sim w_i\sigma^2$$
$$\text{cov}\,(X_i\, X_j) \sim 0$$

and

$$\text{var } A \sim \frac{2\sigma^2}{\Sigma w_i} + \frac{2\sigma^2}{(\Sigma w_i)^2}\,\Sigma w_i \cos \frac{4\pi i}{\lambda}$$
$$\text{var } B \sim \frac{2\sigma^2}{\Sigma w_i} - \frac{2\sigma^2}{(\Sigma w_i)^2}\,\Sigma w_i \cos \frac{4\pi i}{\lambda}$$
$$\text{cov}\,(A,\, B) \sim \frac{2\sigma^2}{(\Sigma w_i)^2}\,\Sigma w_i \sin \frac{4\pi i}{\lambda}$$

Thus, provided $\Sigma w_i \sin \frac{4\pi i}{\lambda}\Big/\Sigma w_i$ and $\Sigma w_i \cos \frac{4\pi i}{\lambda}\Big/\Sigma w_i$ are small, *i.e.*, provided w_i does not have a term of period $\lambda/2$, we can apply the usual tests of significance to the intensity, $A^2 + B^2$, to determine whether the weighted observations have a period λ, provided that we use Σw_i instead of n.

We can, in the same way, use correlogram analysis with estimated correlation coefficients, $r_j = \Sigma X_i X_{i+j}/\Sigma X_i^2$ to determine any oscillatory movement in the data. Dr. Bartlett's formulae and analysis can be extended to this case. Thus, if $W_j = \Sigma \sqrt{w_i\, w_{i+j}}$ then

$$\text{cov}\,(r_s,\, r_{s+t}) \sim \frac{(n - s - t)\sigma^4}{W_s\, W_{s+t}} \sum_{i=-\infty}^{\infty} W_i\, W_{i-t}\, \rho_i\, \rho_{i-t}$$

and, if the weights are randomly distributed, we get

$$\text{cov}\,(r_s,\, r_{s+t}) \sim \frac{\sigma^4}{\Sigma w_i} \sum_{i=-\infty}^{\infty} \rho_i\, \rho_{i+t}$$

so that Dr. Bartlett's analysis can be applied directly.

As a simpler alternative to the methods described above, we could use a standardized variable $Y_i = \sqrt{w_i}\,(x_i - \bar{x})$ for analysis. This variable has constant variance, σ^2, so that the usual tests of significance can be employed, although these will be less efficient than the above methods.

When we are dealing with percentages, p, the use of the transformation $x = \sin^{-1}\sqrt{p}$, tabulated by Fisher and Yates, is to be recommended, so that the variance of x is independent of p. Under this transformation, w_i becomes the number of observations upon which the percentage is based, while $\sigma^2 = \frac{1}{4}$. This determination of σ^2 allows the Schuster and Walker tests for the periodogram to be used. Again, when we are dealing with index numbers, I, the transformation $x = \log I$ is often very useful.

Finally, it should be noted that while the above tests are extensions of existing tests, we shall, in general, require a larger number of observations if practice is to correspond to theory.

Mr. D. R. Cox: Although I was unable to be present at the meeting, I was privileged to receive a copy of Dr. Vajda's paper. I was surprised to find that the equation in Section 3*a* was attributed to me, since it has been used in many investigations of randomly occurring events—there was certainly no intention of claiming it as original in the R.A.E. Report referred to.

Dr. Vajda has applied the equation directly to data for which the detailed distribution of intervals is known. In many cases these figures will not be available, and it may be of interest to indicate how the interval method can be extended to deal (by standard continuous distributions) with problems that are usually tackled by the use of the often less convenient discrete Poisson distribution. Details are to be found in the R.A.E. Report which Dr. Vajda has mentioned.

Suppose that n events occur in a period T and that it is permissible to assume that events occur randomly in time at the unknown rate m. If the period T was measured up to the n^{th} event, then the result in section 3*a* of Dr. Vajda's paper can be used to prove the known result that, for fixed n, $2mT$ is distributed as χ^2 with $2n$ degrees of freedom.

In most cases it will only be known that the n^{th} event occurred before T and the $(n + 1)^{th}$ event occurred (or would have occurred) after T. It is, however, often permissible to assume that the choice of the end of the interval T is equivalent to the choice of a point at random between the n^{th} and the $(n + 1)^{th}$ events; in other words, that the ratio

$$\frac{\text{Time from } n^{th} \text{ event to } T}{\text{Time between } n^{th} \text{ and } (n + 1)^{th} \text{ events}}$$

has a rectangular distribution over (0, 1). Fisher* has, in another connection, suggested a similar device.

Let T_1 be the time up to the n^{th} event and let $T = T_1 + T_2$.

Let $X = 2mT$, $X_i = 2mT_i$ $(i = 1, 2)$.

The distribution of X_1 (for fixed n) is the χ^2 distribution with $2n$ degrees of freedom, and it is easy to show that the frequency function of X_2 is, under the above assumptions,

$$\tfrac{1}{2}\, ei\left(\frac{X_2}{2}\right) = \tfrac{1}{2}\int_{X_2/2}^{\infty} \frac{e^{-x}}{x}\, dx,$$

and that X_2 is independent of X_1.

The distribution of $X = X_1 + X_2$ can now be found. For large n it will be very close to the χ^2 distribution with $(2n + 1)$ degrees of freedom, and in fact the calculation of cumulants, and computations on the exact frequency function, show that this approximation is very good, even for n as small as one.

Thus under the above assumptions, $X = 2mT$ is distributed (for n fixed) as χ^2 with $(2n + 1)$ degrees of freedom.

To apply this result, suppose that n_i events have occurred in periods T_i $(i = 1, \ldots, k)$, that the above assumptions can be made, and that it is required to test the hypothesis

$$m_1 = \ldots\ldots = m_k.$$

Then $T_i/(2n_i + 1)$ has (for fixed n_i) the distribution of an estimate of variance based on $(2n_i + 1)$ degrees of freedom, and so Bartlett's test may be applied.

If $k = 2$,

$$F = \frac{(2n_2 + 1)\, T_1}{(2n_1 + 1)\, T_2}$$

can be tested in the variance ratio distribution with $(2n_1 + 1,\ 2n_2 + 1)$ degrees of freedom.

The second of these tests is probably more convenient arithmetically than the usual test for the equality of Poisson means, while both the tests given here may be considered as accurate, even for very small samples $(n_i \geq 1)$.

The control of errors of the first kind in the test in which the n_i are regarded as fixed and the T_i as variable is not the same as the control in the Poisson test, in which the T_i are fixed and the n_i variable. However, in many applications the two controls will be equally acceptable.

Dr. Vajda has used the method of intervals in his analysis of ship accident rates; work at the Wool Industries Research Association has suggested another problem for which the method might be used—that of "ends down" in spinning. Each operative is responsible for one or more frames each containing a hundred or so bobbins, and every time an end breaks ("end down"), it has to be repaired. Here, even if the hypothesis of randomly occurring events has to be discarded, the distribution of intervals between successive "ends down" and the joint distribution of adjacent intervals remain of direct interest, since these distributions can be related to the optimum number of frames per operative.

In reply, Dr. Vajda thanked the audience for their very kind reception of his paper. He would reply in detail in writing; in fact he must do so because he had learned so much from their contributions during the last hour that he could not possibly be expected to piece it together in such a short time as was available.

He would say one word about the question of the vessels being homogeneous; in fact they formed practically one single class and were all of the same tonnage, for what that information was worth, and he thought there could be no possible doubt that for that statistical investigation all the vessels could be considered homogeneous as far as their obvious aspects were concerned.

The second point, raised by Mr. Maddex, which he had appreciated from the beginning and had mentioned it in the paper, was the question of deductions to be made for days spent in dock, etc. This was a difficult point, and he could have avoided it to a certain extent simply by saying that he had not had the information. Their American friends had made a similar investigation. They had had to have the log books of all the vessels concerned, and when one of the Lieutenant-Commanders of the United States Navy was over in this country and had said how many Wrens —or the American equivalent—he had sent for how many weeks to a certain place in the United

* *J.R.S.S.* **98**, 39.

States in order to enable them to find out what happened, and how many days these ships had spent in dock and harbour, he had been quite sure that he was justified in forgetting about this altogether. They would not have had the man- or woman-power to do it. He was quite clear about the fact now that he had, on the other hand, caused a very great difficulty by admitting that he had not done anything about it ; he offered his apologies for this. There was the further difficulty that the casualties were not such that they could not have happened in dock, harbour, etc. Many of them did, in fact, happen while the vessel concerned was in harbour, but he felt he must leave it to them to decide whether that was really a good excuse for not tackling the problem.

He was very grateful for the many suggestions which had been made for the way in which the investigation should be carried on. As far as Mr. Chambers' suggestion regarding seasonal variations was concerned, they had started investigations on those lines, but could not finish them in time. He hoped to say something about that in writing. Some of the suggestions had already been considered by the Admiralty; some had not, but would certainly be considered from now on.

Dr. Vajda subsequently wrote as follows:

I do not think that I can add much concerning the time spent in dock and harbour or the type of casualty and of vessels, except to say that I was glad experts referred forcefully to the difficulties of definition and interpretation inherent in such investigations in general and of shipping in particular. Personally, I feel that the efficiency of masters and crew in the vessels investigated was sufficiently high and sufficiently consistent to justify me in ignoring the possible influence of these factors.

A few speakers mentioned further possible tests which might have been applied. Thus Mr. Maddex wondered whether the comparison of accident rates by applying standardized weights was carried out. We did experiment with this method, and the rates obtained by taking the total for all builders as standard was within .1 of the rates given in Tables 1 and 2, except for Builder G, where 8.3 was obtained for "All Casulaties" and 8.4 for "First Casualties" (in lieu of 9.1 and 9.5 in the tables). In any case, this test gives only an idea of the relative accident rates excluding the effect of age, whereas we were also interested in the age effect itself.

Sir William Elderton suggested a periodicity analysis for individual builders. The time of exposure was very short (as Mr. Beard has also pointed out), and further subdivision would have made the data rather thin. But with further material the comparison mentioned by Sir William must not be overlooked.

The question of seasonal variations gives rise to further tricky problems. I appreciate Mr. Quenouille's hints, but his qualifications concerning an unbiased mean appear to be serious. The decrease in accident rates in June, 1944, and after, noticed by Dr. Sutton, seems to fit quite well into the general trend experienced in the earlier years. The objection raised by the Government Actuary and by Mr. Chambers is, of course, a very relevant one. In order to see whether the age effect is genuine or only a spurious product of the seasonal variations we have introduced a new variable, namely, the month. We have constructed a further contingency table, showing the accident rates according to month and age, and have applied the technique mentioned in Section 3(*b*) of my paper, but the exclusion of the effect of the seasons did not make the age effect disappear. Mr. Babington-Smith's question, whether the usual tests of significance are applicable to distributions which appear to be rather unusual, is worth following up, but I feel that if effects are very significant by the usual tests, then we are justified in thinking that the effects are present, if even to a smaller extent than appears at first sight.

I agree with Mr. Beard that a Poisson fit to Table 1 may not mean so much as it seems to at first glance, because of the uneven distribution of exposure time, and I am grateful to Professor Greenwood for his illuminating analysis, supplemented by figures.

I ought not to have used the word "significant" after Table 5, because I did not mean it in its technical sense. As I have pointed out, this test of intervals cannot be used for far-reaching conclusions in any case. However, Mr. Beard's approach in statistical terms is, of course, sound. Dr. Solomon is quite right in saying that it would have been advantageous to apply a square-root transformation to the accident rates. I have mentioned this point myself in an earlier paper, and I am glad he brought it up again.

A few smaller points remain for comment. Dr. Sutton's question whether I considered a Hollerith installation capable of dealing with more detail would have to be investigated in every case.

Some "Exits" were due to casualties, but I could not find any trace of this fact affecting the remainder. Still, with more extensive data this hint given by the Government Actuary should be followed up.

I have accepted Mr. Chambers's plea for the use of the word "liability" rather than "proneness" and the paper as finally printed does not contain this word.

MULTIVARIATE ANALYSIS

By M. S. BARTLETT

[Read before the RESEARCH SECTION OF THE ROYAL STATISTICAL SOCIETY, Thursday, May 29, 1947, Dr. J. WISHART in the Chair.]

CONTENTS

1. *Preamble.*

THE advances in recent years in the theory of multivariate statistical analysis seemed to me to merit a paper on this topic to the Research Section of our Society, and this suggestion was provisionally approved by the Committee in the early summer of 1946. I mention this date, because now that I come to write this paper other expositions in the meantime have made me more doubtful of its necessity. I am thinking particularly of the chapter on multivariate analysis in Volume II of Kendall's treatise (33) (an interesting note on the main lines of development in the United States, India and England, associated respectively with the names of Hotelling, Mahalanobis and Fisher, will be found at the end of Kendall's chapter), and of papers recently presented in the United States, for example, by Tukey ("Vector methods in analysis of variance," at Princeton (51)) and by Brown ("Discriminant functions," at Boston (16)). However, these expositions allow me to present here an order of development of methods of multivariate analysis that is more closely associated with personal researches, without my feeling bound to attempt an exhaustive general exposition. Perhaps I should add that while I have avoided any complicated analytical discussion of theoretical problems, I have not hesitated on occasion to refer to the mathematical theory, with the aid of matrix and vector algebra or associated geometrical representation. Those who prefer a more elementary treatment should also consult Brown's paper, which I assume will be published; or familiarize themselves simply with the numerical procedures illustrated in the various examples to be found here and in the literature.

2. *Multivariate analysis of variance.*

In biological, psychological or anthropological work it often happens that we are interested in several variates simultaneously; we might wish to consider several characters of a plant, or consider the differences between races indicated by several skull measurements. When considering the statistical analysis of such a group of correlated variates we shall find it convenient to adopt the terminology of the theory of regression, and refer to these variates, which we shall analyse in relation to other variates or classifications, as correlated dependent variates. We shall also find it useful to make use of vector and matrix notation, a sample of n observations each in p variates defining automatically a matrix array S of n columns by p rows. The reader who is unfamiliar with matrix theory is urged to consult any standard work (e.g. *Determinants and Matrices*, by A. C. Aitken). In matrix notation the analysis of variance and covariance of a number of correlated variates is very succinctly expressed (see (3)). For example, if the sample S is split up into two components which are mutually orthogonal,

$$S = X + Y, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

say, we have the corresponding analysis of sums of squares and products (analysis of variance and covariance):

$$SS' = (X + Y)(X + Y)' = XX' + YY', \quad . \quad . \quad . \quad . \quad (2)$$

where X' is the transpose of X and the terms YX' and XY' vanish owing to the orthogonality of the components X and Y. Although in many analyses more than one classification is present, the above analysis is still sufficient, provided S is a "reduced sample" with irrelevant components already eliminated, with a consequent loss of degrees of freedom. To illustrate from the paper, just quoted, we are in Table I below, which refers to an analysis of variance and covariance of the effect of fertilizers on the grain and straw yields of cereals, concerned with the first three rows of figures, the effect of blocks being eliminated. The last three entries in each line of the Table correspond with a single matrix term in equation (2), the treatments line corresponding to XX' and the residual line to YY'.

TABLE I.—(x_1 – *Straw*, x_2 – *Grain*)

	Degrees of freedom.	x_1^2.	$x_1 x_2$.	x_2^2.
Treatments	7	12,496·8	6,786 6	32,985·0
Residual	49	136,972·6	58,549·0	71,496·1
Total	56	149,469·4	51,762·4	104,481·1
Blocks	7	86,045·8	56,073·6	75,841·5
Grand total	63	235,515·2	107,836·0	180,322·6

I do not personally believe in "portmanteau tests" when more elementary ones will do, but in cases where it is useful to make an over-all test of the effect of treatments (or other relevant classification) a test based on the general criterion

$$\Lambda = |YY'| / |SS'|, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where X is the component to be tested, was proposed (this being equivalent to the appropriate likelihood criterion in the special case of testing the differences in means among k multivariate samples, considered by Wilks (53) and Pearson and Wilks (40)).

In the above example we have

$$\Lambda = \begin{vmatrix} 136{,}972 \cdot 6 & 58{,}549 \cdot 0 \\ 58{,}549 \cdot 0 & 71{,}496 \cdot 1 \end{vmatrix} \Big/ \begin{vmatrix} 149{,}469 \cdot 4 & 51{,}762 \cdot 4 \\ 51{,}762 \cdot 4 & 104{,}481 \cdot 1 \end{vmatrix}$$

$$= 0 \cdot 4920.$$

A general approximate test of Λ has been given (4). We calculate

$$\chi^2 = -\{n - \tfrac{1}{2}(p + q + 1)\} \log_e \Lambda$$

where n is the number of degrees of freedom of S, q the number of degrees of freedom of X, and p the number of variates. This gives

$$\chi^2 = -51 \log_e 0 \cdot 4920 = 36 \cdot 2$$

with $pq = 14$ degrees of freedom, or a significance level from the tables of almost exactly $P = 0 \cdot 001$. Actually, in the case p or $q = 2$, the distribution of $\sqrt{\Lambda}$ is known from work by Wilks, and it is possible to make an exact test based on the tables of Fisher's z. This was done in my original discussion (3); the exact significance level is obtained by calculating

$$z = \tfrac{1}{2} \log_e \left\{ \frac{1 - \sqrt{\Lambda}}{\sqrt{\Lambda}} \, \frac{n - q - 1}{q} \right\} = 0 \cdot 536,$$

with degrees of freedom $n_1 = 2q = 14$, $n_2 = 2(n - q - 1) = 96$. We find, interpolating in the $P = 0{\cdot}001$ significance levels of z a value z, $(P = 0{\cdot}001) = 0{\cdot}531$, again giving a significance level for the observed z of almost exactly $P = 0{\cdot}001$. Of course, one would not usually use an approximate test when a fairly convenient exact test is available, but I have purposely done so here to stress the value of the approximate χ^2 test, which is available in general. Its application to other examples is illustrated later.

3. *Canonical reduction of the general regression problem*

It is well known that the splitting up of a sample in analysis of variance can be regarded as the regression analysis of the sample in terms of other variates or pseudo-variates, and, as mentioned in the previous section, it is convenient to discuss the analysis in terms of regression. We therefore change the notation of equations (1) and (2), and write S_2 for S, S_1 for the set of "independent variates" that we are taking the regression on, and $S_{2\cdot 1}$ for the "residual component" of S_2 after the regression on S_1 has been removed. For equation (1) we now have (cf. (4)),

$$S_2 = B_{21} S_1 + S_{2\cdot 1}, \qquad (4)$$

where the matrix of regression coefficients is

$$B_{21} = C_{21} C_{11}^{-1}, \qquad (5)$$

where $C_{21} = S_2 S_1'$, $C_{11} = S_1 S_1'$, $C_{22} = S_2 S_2'$ and $C_{22\cdot 1} = S_{2\cdot 1} S_{2\cdot 1}'$; and for equation (2) we have

$$C_{22} = C_{21} C_{11}^{-1} C_{12} + C_{22\cdot 1} \qquad (6)$$

The analysis of a new dependent variate obtained by a linear transformation of the original set, $\mathbf{a}' S_2$, say, is given by

$$\mathbf{a}' S_2 = \mathbf{a}' B_{21} S_1 + \mathbf{a}' S_{2\cdot 1}, \qquad (7)$$

whence

$$\mathbf{a}' C_{22} \mathbf{a} = \mathbf{a}' C_{21} C_{11}^{-1} C_{12} \mathbf{a} + \mathbf{a}' C_{22\cdot 1} \mathbf{a} \qquad (8)$$

Equation (8) represents an analysis of variance of the variate $\mathbf{a}' S_2$; and the ratio of the first to the second sum of squares on the right-hand side of (8) measures the significance of the dependence of $\mathbf{a}' S_2$ on S_1. To find the particular variate $\mathbf{a}' S_2$ which maximizes this ratio we choose the coefficients $\mathbf{a}$ as a solution of the equation

$$(C_{21} C_{11}^{-1} C_{12} - R^2 C_{22})\mathbf{a} = 0, \qquad (9)$$

where

$$| C_{21} C_{11}^{-1} C_{12} - R^2 C_{22} | = 0 \qquad (10)$$

Multiplying (9) on the left by $\mathbf{a}'$, we have

$$R^2 = \frac{\mathbf{a}' C_{21} C_{11}^{-1} C_{12} \mathbf{a}}{\mathbf{a}' C_{22} \mathbf{a}} \qquad (11)$$

so that R^2 is the fraction of the total sum of squares that we have been maximizing, and must be the *largest* root of (10). It is also the square of the multiple correlation between $\mathbf{a}' S_2$ and S_1. The relation of $\mathbf{a}' S_2$ to S_1, while maximized, does not in general exhaust the real dependence of S_2 on S_1, and if the variate $\mathbf{a}' S_2$ is removed, we may still find a significant relation from the remaining variates (chosen to be uncorrelated with $\mathbf{a}' S_2$). In fact, a repeated maximization for such a reduced sample would be found to lead to another root, the second largest, of the original equation (10); and so on. Hotelling (28), to whom the above type of analysis is due, has shown that the original set S_2 can be transformed into an equivalent mutually uncorrelated set $A S_2$, say, each variate of which is associated with one of the roots R^2 in (10); such a system of variates with their corresponding partners from transformations of S_1 are called *canonical variates*, and

the corresponding roots R^2, or rather their square roots R, *canonical correlations*. I shall for convenience refer to such an analysis as a *canonical analysis.*

To relate these roots with the over-all test Λ defined in equation (3), we may note that equation (10) may be written

$$| 1 - C_{21} C_{11}^{-1} C_{12} C_{22}^{-1} - (1 - R^2) | = 0, \quad . \quad . \quad . \quad (12)$$

whence

$$\prod_{i=1}^{p} (1 - R_i^2) = | 1 - C_{21} C_{11}^{-1} C_{12} C_{22}^{-1} |$$
$$= \frac{| C_{22} - C_{21} C_{11}^{-1} C_{12} |}{| C_{22} |}$$
$$= \Lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

This relation is sometimes useful if we wish to test the significance of the remaining roots $R_{r+1}^2 \ldots R_p^2$ when the first r roots $R_1^2 \ldots R_r^2$, say, which are established to be associated with real relationships between S_2 and S_1, have been removed. We have

$$\Lambda = \Lambda'\Lambda'' = \prod_{i=1}^{r} (1 - R_i^2) \prod_{j=r+1}^{p} (1 - R_j^2),$$

where the χ^2 test approximation for Λ may be extended to Λ'' (see (4)), giving

$$\chi^2 = - \{n - \tfrac{1}{2}(p + q + 1)\} \log_e \Lambda'' \quad . \quad . \quad . \quad . \quad (14)$$

with $(p - r)(q - r)$ degrees of freedom.

As a simple illustration, let us consider again the example in section 2. We calculate the determinant in equation (9) for suitable values of R^2, the matrix $C_{21} C_{11}^{-1} C_{12}$ being given by the first row of Table I and C_{22} by the third row. Here the equation is only a quadratic in R^2, but this method still seems the most convenient computational method to find the roots, which are calculated by interpolation (for a more complicated example see section 5; cf. also Fisher (23), Ex. 46.2). We obtain

$$R_1^2 = 0{\cdot}47698, \; R_2^2 = 0{\cdot}05934,$$

and as a check,

$$\Lambda = (1 - R_1^2)(1 - R_2^2) \quad 0{\cdot}4920,$$

agreeing with the value given in section 2. The χ^2 approximation noted above thus gives the analysis

TABLE II.—*Approximate χ^2 Table for Canonical Analysis.*

Root.	d.f.	χ^2.
R_1^2	8	33·06
R_2^2	6	3·12
Total	14	36·18

It is of interest that the first root removes the significant treatments effects. From equation (8) the linear function corresponding to this larger root is given by the consistent pair of equations

$$58{,}797{\cdot}1\, a_1 + 31{,}477{\cdot}2\, a_2 = 0,$$

$$31{,}477{\cdot}2\, a_1 + 16{,}850{\cdot}4\, a_2 = 0,$$

giving the function

$$x_2 \text{ (grain)} - 0{\cdot}535\, x_1 \text{ (straw)}$$

as the quantity affected by the treatments. Of course whether this variate is considered or simply x_2, the yield of grain, depends on what questions the statistical analysis is to answer.

Even if we are interested in all significant effects, the canonical analysis of S_2 is not always necessary. An alternative factorization of Λ corresponds to the separation of S_2 into two groups S_0 and $S_{2\cdot 0}$, say, based on an internal analysis within S_2. An illustration of such an analysis was given in my discussion on Wishart's paper (56) on the "statistical treatment of animal experiments." It will be recalled that growth curves had been fitted to the weekly weights of pigs, on which the effects of three different food rations were being investigated. The constants g, h, i, corresponding to the linear, parabolic and cubic terms in these growth curves, then represented a multivariate set for all of which the effects of food and other classifications were to be examined. A simple χ^2 table summary of the significance effects in relation to food is quoted below,* this being for the analysis of the log. weights.

TABLE III.—*Approximate χ^2 Analysis*

Variable.	d.f.	χ^2.
g	2	13·28
$h.g$	2	0·51
$i.gh$	2	2·29
ghi	6	16·08

As explanation of these figures, we have three dependent variates g, h and i, so that $p = 3$; and two independent variates (the pseudo-variates corresponding to the two degrees of freedom between food treatments), so that $q = 2$. This means that an exact test for Λ could have been made on the basis of $\sqrt{\Lambda}$ (see section 2) but the χ^2 test was convenient. The component items correspond to single dependent variables g, $h.g$ and $i.gh$, which could be tested by the z test, but again it was preferred to use the χ^2 table as shown. This is slightly forced, for the best approximation for a variate like g would be

$$\chi^2 = -\{n - \tfrac{1}{2}(1 + q + 1)\} \log_e (1 - R^2),$$

where R is the multiple correlation between the variate g and the two food pseudo-variates, whereas the coefficient of $\log_e(1 - R^2)$ actually used was the same throughout as for the "total" item, viz. $n - \frac{1}{2}(3 + q + 1)$, where here $n = 21$ (19 for "error" + 2 for "food"), and $q = 2$. The best coefficients for $h.g$ and $i.gh$ would similarly have been $n - 1 - \frac{1}{2}(1 + q + 1)$ and $n - 2 - \frac{1}{2}(1 + q + 1)$. This is in contrast with the canonical analysis formula (14), where the best coefficient of $\log_e \Lambda''$ remains constant. However, the differences in these coefficients are not very serious, the analysis at best being only approximate. The important feature is the predominance of the first contribution to χ^2 in Table III. If a canonical analysis had been made, the χ^2 table would have had the structure

	d.f.	χ^2.
R_1^2	4	A
R_2^2	2	B
Total	6	16·08

where necessarily $16{\cdot}08 \geq A \geq 13{\cdot}28$, $0 \leq B \leq 2{\cdot}80$. It is evident that the first canonical variate, which would be the only significant one, would further not be significantly different from the natural variate g, since the remaining χ^2 contribution 2·80, even if concentrated in the 2 d.f. of B, or in the remaining 2 d.f. of A, could not reach significance. Thus no further analysis is demanded, and the whole of the differences due to food can be regarded as contained in the changes in the constant g in the growth-curves of the pig log. weights.

* With a correction to the third item pointed out to me by Dr. Wishart. It should also be noticed that the results here are based on the adjusted Figures after elimination of the effect of initial weights, a point I had forgotten. This makes n in the ensuing discussion of Table III equal to 21, as noted by Dr. Wishart in the general discussion following my paper; this was the value actually used to obtain the χ^2 values in Table III.

4. *Theory of discriminant functions*

There is no necessity in the canonical analysis outlined in the last section for p, the number of dependent variates, to be less than q, the number of independent variates (or pseudo-variates); but if p is greater than q, there will be only q roots R^2 not zero, as illustrated in Table III. If $q = 1$, we have a comparatively very simple but important case which is worth studying separately. The theory of this case was implicit in Hotelling's work (25) on the generalization of Student's t, where the simultaneous testing of the means of several correlated variates was considered. The test is equivalent to testing the significance of the angle made between the sample vector S_1 representing the independent variate, when this vector is considered to define a direction in n-dimensional space, and the vector corresponding to that linear function of the correlated set of variates S_2 which gives the minimum angle with the first vector. But this angle is simply $\cos^{-1} R$, where R is the formal multiple correlation between S_1 and S_2, and the linear function is just the linear function that would be obtained if a formal multiple regression analysis had been made of the single variate S_1 on the set of variates S_2 (the roles of "dependence" and "independence" thus being formally reversed). In Hotelling's case the independent variate is the pseudo-variate corresponding to isolating the sample means, i.e. a variate with value 1 for all observations, but we have seen that the theory is applicable to all regression problems. The linear function was considered more explicitly by Fisher and other workers associated with him, and called a *discriminant function*, owing to its optimal property in its relation to the independent variate. For example, if the independent variate S_1 is a pseudo-variate with the values $+1$ for one group and -1 for the other, the discriminant function will be that linear function of the dependent variates most able to discriminate members of the two groups.

Before discussing an example in detail, let us summarize the distributional theory corresponding to the above formal analysis. If we assume that the dependent variates S_2 are jointly normal (thereby ensuring "spherical symmetry" for this group of vectors in the sample space), and the independent variate S_1 arbitrary (but known), then a test of no real relation between S_2 and S_1 is based on the random relation of S_2 to S_1, which is distributionally identical with the relation of a random S_1 to an arbitrary S_2. This is just the test of significance that would be used if the formal multiple regression analysis of S_1 on S_2 is assumed to be a multiple regression analysis for which the standard tests of significance will apply.

This justification of tests used with this formal regression analysis may be extended further. For if we ask whether a significant relation of S_2 with S_1 is entirely due to one (or more) of the component variates of S_2, say S_0, we are led to considering the relation of S_2 and S_1 in the space orthogonal to the vector (or vectors) S_0, to which a similar inversion of the roles of S_2 and S_1 may be applied. By identifying a single eliminated variate with a hypothetical discriminant function, this provides an exact test of difference of the observed with such a hypothetical discriminant function. Further, if we separate off all but one of the variates of S_1, the remaining relation indicates the necessity of including this last variate in the discriminant function; such a test is equivalent to providing the coefficient of this variate in the discriminant functions with its formal standard error (see (6)).

Again, if we had two or more separate sets of data, and calculated a discriminant function from each, then the test of differences of the discriminant functions would follow the usual regression method (for in the geometrical representation the discriminant functions, each with the same number of degrees of freedom, would be in orthogonal spaces, and would be transformed to the common discriminant function also with the same number of degrees of freedom plus the residual random part with the remaining degrees of freedom).

Even if S_2 has a real (uneliminated) association with S_1—for example, in the difference in means of two groups a real difference in means may exist—this duality of the relation of S_2 with S_1 still holds and links the distributional theory with the corresponding distributional theory when S_1 is the dependent variate. In the difference in means problem the population parameter is equivalent to Mahalanobis' generalized distance between the two groups, and the distribution of the corresponding sample estimate, first given by Bose and Roy (12), will be identical with one of Fisher's distributions of the multiple correlation coefficient, viz. the case where the correlation is due to regression on fixed variables.

This duality is, however, less obvious in the case of real association, and in fact must not be

assumed necessarily to hold in the more general case of the previous section where S_1 denotes more than one variate. In this more general case (see (8) and section 6 of this paper) it may be shown that for S_1 fixed duality only holds if not more than one true root is non-zero. This case includes as special cases: (i) The case above where S_1 denotes one variate, and hence there is at most one non-zero canonical root. (ii) The case of no real association (cf. (3), p. 338).

5. *Discussion of an example from anthropometry*

As an example of a discriminant function I propose to discuss the anthropometric data first analysed in relation to discriminant functions by M. M. Barnard (2). My calculations for this example were first intended for a draft chapter in the co-operative work envisaged just before the war and referred to by Kendall in his Preface to Vol. I of his treatise. Kendall has since made use of them in his own chapter on multivariate analysis, but as the treatment below is somewhat fuller than that given by him, it is perhaps still worth giving as illustrative material for this paper.

In Barnard's investigations of the changes taking place with time in four series of Egyptian skulls, four variates were selected from a larger set as providing significant information. The four series were formed of skulls from the Late Predynastic, Sixth to Twelfth, Twelfth to Thirteenth and Ptolemaic dynasties respectively. The four selected variates were*:

x_1 maximum breadth.

x_2 basialveolar length.

x_3 nasal height.

x_4 basibregmatic height.

It is required to combine these four characters into a linear function which shall best discriminate the effect of time changes; that is, to maximize the contribution to the total sum of squares in an analysis of variance due to the regression on time, relative to the variance within series. Let this function be estimated to be

$$\mathbf{a}'\mathbf{x} = a_1x_1 + a_2x_2 + a_3x_3 + a_4x_4.$$

The relevant data for the four variates separately are summarized in Tables IV and V, which give the means for the four series, and the total sums of squares and products within series, respectively.

TABLE IV.—*Means in the Four Series*

Variate.	Series I ($n_1 = 91$).	II ($n_2 = 162$).	III ($n_3 = 70$).	IV ($n_4 = 75$).
$\bar{x}_1$	133·582418	134·265432	134·371429	135·306667
$\bar{x}_2$	98·307692	96·462963	95·857143	95·040000
$\bar{x}_3$	50·835165	51·148148	50·100000	52·093333
$\bar{x}_4$	133·000000	134·882716	133·642857	131·466667

TABLE V.—*Sums of Squares and Products within Series* (394 *d.f.*)

	x_1.	x_2.	x_3.	x_4.
x_1	9661·997470	445·573301	1130·623900	2148·584210
x_2	—	9073·115027	1239·221990	2255·812722
x_3	—	—	3938·320351	1271·054662
x_4	—	—	—	8741·508829

The relative times between the series were taken (from other evidence) in the proportion 2 : 1 : 2, so that in order to obtain the regression on time, the values of the time t may be taken as -5 for all observations in Series I, -1 for Series II, $+1$ for Series III and $+5$ for Series IV. With these values, the mean value of t for the 398 observations is $-0{\cdot}432161$, so that the values $t - \bar{t}$ (corresponding to the components of the single vector S_1 in the notation of our general theory) are for the four series respectively, $-4{\cdot}567839$, $-0{\cdot}567839$, $+1{\cdot}432161$, $-5{\cdot}432161$.

* I am indebted to Dr. Herden (see discussion) for pointing out an earlier misidentification of these variates with the figures in Tables IV and V. This has now been corrected.

There are altogether three degrees of freedom among the four series, but only one of these corresponds to the linear regression on time, and (following Barnard's original investigation) we shall for the moment discard the other two, so that with the elimination of the general mean, three degrees of freedom have been eliminated, leaving 91 + 162 + 70 + 75 − 3 = 395, of which 1 corresponds to the regression on time, and the other 394 to the variation within series given in Table V.

The sums of products of x_1, x_2, x_3 and x_4 with $t - \bar{t}$ are respectively,

$$718 \cdot 76286, \; -1407 \cdot 26075, \; -410 \cdot 10194, \; -733 \cdot 42758;$$

and the sum of squares $\Sigma(t - \bar{t})^2$ is 4307·66832. We have seen that the coefficients a_1, a_2, a_3 and a_4 may be obtained by our carrying out a formal regression analysis of $t - \bar{t}$ on x_1, x_2, x_3 and x_4. To do this, we need the matrix of sums of squares and products for the total number of degrees of freedom (395). It is a slight complication in this example that this matrix has not arisen automatically, and it would in some ways be more convenient to solve the equivalent equation where we replace the matrix of total sums of squares and products by that within series (it is readily shown by matrix algebra that the solutions are equivalent, differing merely by a constant factor). However, the later exact tests based on the regression duality follow more easily if we keep to the regression analysis, so that the matrix of total sums of squares and products (395 d.f.) is given in Table VI, obtained by adding to the terms of Table V the terms with 1 d.f. corresponding to the regression on time. For example, the first term 9781·927828 is obtained by adding to the first term 9661·997470 in Table V the value $(718 \cdot 76286)^2/4307 \cdot 66832$.

TABLE VI.—*Total Sums of Squares and Products* (395 *d.f.*)

	x_1	x_2	x_3	x_4
x_1	9781·927828	210·762489	1199·052135	2026·206952
x_2	—	9532·849476	1105·246827	2405·414318
x_3	—	—	3977·363203	1201·230304
x_4	—	—	—	8866·382928

The reciprocal matrix obtained by solving the equations for the coefficients a_1, a_2, a_3, a_4 with

$$\begin{matrix} 1, & 0, & 0, & 0 \\ 0, & 1, & 0, & 0 \\ 0, & 0, & 1, & 0 \\ 0, & 0, & 0, & 1 \end{matrix}$$

on the right-hand side in place of the sum of products of x_1, x_2, x_3 and x_4 with $t - \bar{t}$, is shown in Table VII.

TABLE VII.—*Reciprocal Matrix of Sums of Squares and Products* ($\times 10^6$)

	x_1	x_2	x_3	x_4
x_1	110·368975	6·938481	−28·145236	−23·361935
x_2	—	115·693529	−24·948984	−30·767069
x_3	—	—	273·988409	−23·666591
x_4	—	—	—	129·990069

Multiplying this matrix by the sum of products of x_1, x_2, x_3 and x_4 with $t - \bar{t}$, we obtain the solution

$$a_1 = +0 \cdot 075156739$$

$$a_2 = -0 \cdot 145490050$$

$$a_3 = +0 \cdot 144600884$$

$$a_4 = -0 \cdot 078538419$$

The significance of the relation of the variates x_1, x_2, x_3 and x_4 with time is most easily tested by continuing formally with the regression analysis of the vector S_1 (i.e. the variate $t - \bar{t}$) on x_1, x_2, x_3 and x_4. The multiple regression component with four degrees of freedom is

$$a_1 \times 718\cdot76286 + a_2 \times -1407\cdot26075 + a_3 \times 410\cdot10194$$
$$+ a_4 \times -733\cdot42758 = 375\cdot6657.$$

The formal analysis of variance of $t - \bar{t}$, providing from the theory indicated in the previous section an exact test of significance, is thus (Table VIII):

TABLE VIII.

	d.f.	Sum of squares.	Mean square.
Regression on x_1, x_2, x_3 and x_4	4	375·6657	—
Remainder	391	3932·0026	10·0563
Total	395	4307·6683	

The exact standard errors of the coefficients, in the sense explained in section 4, are obtained in the usual way. For example, for a_1, we multiply he error variance 10·0563 (391 d.f.) by the appropriate term, $110\cdot368975 \times 10^6$, in the reciprocal matrix, and take the square root. In this way we obtain finally

$$a_1 = +0\cdot0752 \pm 0\cdot0333$$
$$a_2 = -0\cdot1455 \pm 0\cdot0341$$
$$a_3 = +0\cdot1446 \pm 0\cdot0525$$
$$a_4 = -0\cdot0785 \pm 0\cdot0362$$

Since every coefficient exceeds twice its standard error, we may say at once that every variate is useful in our discriminant function. As in direct regression analysis, these standard errors do not, of course, give us the whole story, since the coefficients are not independent; their use is, as we have seen, merely one aspect of the various analysis of variance tests that may be relevant.

The coefficients above differ from those given by Barnard, since she took an unweighted regression of the variates x_1, x_2, x_3 and x_4 with time, instead of a weighted regression. The difference this makes is, however, slight, as is shown by the following comparison when the coefficient of x_1 is adjusted to be unity.

	Above analysis.	Barnard.
a_1	(1·000)	(1·000)
a_2	−1·935	1·938
a_3	1·923	2·005
a_4	−1·044	−1·062

The practical interpretation of the above analysis appears to me to need some care. We have established a linear function for discriminating between the four series, with particular reference to their time-order, based on the internal variability of the series. But in doing so we discarded two degrees of freedom representing further differences among the means of the four series. Let us bring these back and analyse the total variability among means on the basis of the more general theory of sections 2 and 3. We shall not yet make a canonical analysis, which would lose the discriminant function just worked out, but merely examine the significance of the variability among means in the remaining two degrees of freedom. We can do this by means of the Λ criterion.

First let us translate the significance of the discriminant function available from Table VIII. From this Table the exact significance may of course be estimated by means of Fisher's z test. As a partly independent calculation of the same significance, we have the component factor of Λ

arising from the regression on $t \sim t$ given by the ratio of the determinants of the matrices in Tables V and VI, i.e.

$$\Lambda' = \frac{0\cdot24269054 \times 10^6}{0\cdot26587779 \times 10^6} = 0\cdot9127914.$$

This checks with the ratio of residual to total sum of squares in Table VIII, i.e.

$$\frac{3932\cdot0026}{4307\cdot6683} = 0\cdot9127914,$$

indicating the identity of the two tests. Using, however, the χ^2 approximation, we have

$$\chi_1^2 = -\{395 - \tfrac{1}{2}(1 + 4 + 1)\} \log_e 0\cdot9127914$$
$$= 35\cdot81 \text{ (4 d.f.)}$$

Alternatively, let us now calculate Λ for all the differences among the means. To do this we construct the matrix of sums of squares and products about the general mean, as in Table IX,

TABLE IX.—*Sums of Squares and Products about the General Mean* (397 *d.f.*)

	x_1	x_2	x_3	x_4
x_1	9785·178098	214·197666	1217·929248	2019·820216
x_2	—	9559·460890	1131·716372	2381·126040
x_3	—	—	4088·731856	1133·473898
x_4	—	—	—	9382·242720

and thus calculate the ratio of the determinants of the matrices in Tables V and IX, i.e.

$$\Lambda = \frac{0\cdot24269054 \times 10^6}{0\cdot29544775 \times 10^6} = 0\cdot8214344.$$

This gives

$$\chi^2 = -\{397 - \tfrac{1}{2}(3 + 4 + 1)\} \log_e 0\cdot8214344$$
$$= 77\cdot30 \text{ (12 d.f.)}.$$

The difference between χ_1^2 and χ^2 is due to the missing factor Λ'' in

$$\Lambda = \Lambda'\Lambda'',$$

where

$$\Lambda'' = 0\cdot8999148$$

and

$$\chi_2^2 = -\{397 - \tfrac{1}{2}(2 + 4 + 1)\} \log_e 0\cdot8999148$$
$$= 41\cdot50 \text{ (8 d.f.)}$$

Here the additive property of χ^2 has not been forced (as in Table II), so that $\chi^2 \neq \chi_1^2 + \chi_2^2$ exactly, but the difference is trivial. The important point is the highly significant value of χ_2^2, indicating further significant differences in the means when compared with the internal variability within series, in addition to that earlier attributed to time changes between the series.

This indicates that while the linear function we earlier constructed may efficiently discriminate the series with particular reference to their time order, we must be more cautious about ascribing such differences between the series to time, as further differences of comparable magnitude exist between the series unconnected with the time order.

It is of some interest to make an analysis of χ^2 in terms of the canonical roots (cf. the example in section 3), to check that there is no single linear function which isolates the differences in means if we do not correlate them with the time order but consider simultaneously all the mean differences.

From the value of Λ we know that no factor $1 - R^2$ is less than 0·82. Taking the matrices given in Tables V and IX, which in matrix notations are $C_{22\cdot1}$ and C_{22} respectively, we consider equation (10) in the form

$$| C_{22\cdot1} \quad (1 - R^2) C_{22} | = 0.$$

The determinant in this equation was evaluated for $1 - R^2 = 0{\cdot}80, 0{\cdot}81 \ldots 0{\cdot}85$, as shown in Table X.

TABLE X.—(*Determinant* $\times 10^{-4}$)

$1 - R^2$.		Δ	Δ^2.	Δ^3.	Δ^4.
0·80 .	135600262				
		-35092410			
0·81 .	100507852		7041522		
		-28050898		-978237	
0·82 .	72456964		6063295		70910
		-21987603		-907327	
0·83 .	50469361		5155958		70907
		-16831645		-836420	
0·84 .	33637716		4319538		
		-12512107			
0·85 .	21125609				

The fourth difference should be constant, and enough values have been taken to provide a check. The values of the determinant can now be built up for all values of $1 - R^2$. Since there are only three degrees of freedom in S_1, the largest root for $1 - R^2$ is necessarily unity. Apart from this value, we locate the roots at about 0·89, 0·94 and 0·98. By inverse interpolation, we obtain the more precise values:

$$1 - R_1^2 = 0{\cdot}89017356, \; 1 - R_2^2 = 0{\cdot}94038339, \; 1 - R_3^2 = 0{\cdot}98130990.$$

This gives the χ^2 analysis for Λ shown in Table XI, the total checking with the value obtained earlier.

TABLE XI

Root.	d.f.	χ^2.
R_1^2	6 .	45·72
R_2^2	4 .	24·16
R_3^3	2 .	7·42
Total . . .	12 .	77·30

It is evident that no single root is able to isolate the mean differences.

I shall not attempt to investigate the situation further, but it is perhaps relevant to note the scientific caution urged by Chattopadhyay (17) in the interpretation of statistically significant mean differences in anthropology, owing to observers' bias, unrepresentative samples, and so on. In the present case, although the second and fourth series were measured by the same workers, they display from a casual inspection differences in means of the same order of magnitude as with the other series so that the differences will be presumed genuine; this still leaves, however, the representativeness of the series as evidence for secular change doubtful, in view of the further differences in means noted.

6. *The General Sampling Distribution of the Canonical Roots*

An important advance in the sampling theory of the canonical analysis outlined in section 3 was made in 1939 with the derivation of the distribution of the canonical roots (Fisher (21),

Hsu (29) and Roy (43)) in the case of no real association between S_2 and S_1. This remarkable distribution, obtained on the usual assumption of the normality of the dependent variates but showing the now familiar duality in form for interchange of dependent and independent variates, may be written

$$C \prod_{i=1}^{p} \{(R_i^2)^{\frac{1}{2}(q-p-1)} (1-R_i^2)^{\frac{1}{2}(n-q-p-1)} [\prod_{j=i+1}^{p} (R_i^2 - R_j^2)]\, dR_i^2\}, \quad . \quad . \quad (15)$$

where $C = \prod_{i=0}^{p-1} \left\{ \frac{\pi^{\frac{1}{2}} \Gamma[\frac{1}{2}(n-i)]}{\Gamma[\frac{1}{2}(p-i)]\, \Gamma[\frac{1}{2}(q-i)]\, \Gamma[\frac{1}{2}(n-q-i)]} \right\}, (p \leqslant q,\ n \geqslant p+q).$

Being a simultaneous distribution, it does not provide at once tests of significance for the canonical roots, but further investigation of the latter problem has since been made—for example, by Roy (46). A more complete grasp of the efficiency of possible statistics and tests is only possible on the basis of the canonical correlation distribution when real association between S_2 and S_1 exists. A general method of obtaining this distribution for the regression and correlation problems considered in this paper I have given elsewhere (8). This distribution, like that of a single multiple correlation coefficient, has two forms corresponding to (i) the true correlation case, and (ii) the regression case on fixed variates, this including such special cases as non-zero means. For example, in the case of only one non-zero true canonical root, the distribution has in both cases a factor depending on this root involving a generalized hypergeometric function. Equivalently, in operational form, this factor is

$$\mathrm{H}\, (1-\rho_1^2)^{\frac{1}{2}n} F(\tfrac{1}{2}n, \tfrac{1}{2}n;\ \tfrac{1}{2}p, \tfrac{1}{2}q;\ z^{-1}) \prod_{i=1}^{p} (1-\rho_1^2 R_i^2 z)^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

in case (i) and

$$\mathrm{H}\, e^{-\frac{1}{2}\beta_1^2} F(\tfrac{1}{2}n;\ \tfrac{1}{2}p, \tfrac{1}{2}q;\ z^{-1}) \prod_{i=1}^{p} (1-\tfrac{1}{2}\beta_1^2 R_i^2 z)^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

in case (ii), where H denotes the operation of taking the term independent of z, and the F functions are defined by

$$F(a_1, a_2;\ b_1, b_2;\ x) = 1 + \frac{a_1 a_2 x}{b_1 b_2} + \frac{a_1(a_1+1)\, a_2(a_2+1)\, x^2}{b_1(b_1+1)\, b_2(b_2+1)} \cdots$$

$$F(a;\ b_1, b_2;\ x) \quad = 1 + \frac{ax}{b_1 b_2} + \frac{a(a+1)\, x^2}{b_1(b_1+1)\, b_2(b_2+1)} \cdots$$

The remaining factor in the distribution is simply the distribution (15). If more than one true root is non-zero, the distributions become more complicated.

Theoretically these results allow the further investigation of the distributional properties of particular sample roots and statistics to be made. Thus Roy, who first obtained for case (ii) a result equivalent to (17) above, has further investigated the distribution of the largest root from this distribution (Roy's work (45), (47) and (48) should be read with the reservation that it does not apply to the case of more than one non-zero root; see Anderson (1) and Bartlett (8)). However, much work remains to be done before the statistician can conveniently make use of the results sketched in this section for practical purposes.

One particular point that needs further investigation, as I have stressed elsewhere (4), is the validity of the χ^2 approximation for Λ after the removal of one or more *canonical* roots (see also (7) and (31)). It is easily demonstrated that the approximation fails if the true values of the roots removed are zero. For example, if $p = 2$, $q = 3$, we easily obtain the probability integral of R_2^2 from (15), as $(1-R_2^2)^{n-3}$, or $-2(n-3)\log_e(1-R_2^2)$ is a χ^2 with 2 d.f., whereas the χ^2 approximation used earlier would have taken only half this quantity, i.e. $-(n-3)\log_e(1-R_2^2)$ as a χ^2 with the same number of degrees of freedom. An interesting property, however, that seems worth noting pending investigation of the more exact significance values, is that the χ^2 analysis becomes approximately valid even for zero true roots if the sample roots removed are

large. For from (15) the distribution of $R_i^2 (i = s + 1 \ldots p)$ for given $R_j^2 (j = 1 \ldots s)$ tends as $R_j^2 \to 1$ to the form

$$C' \prod_{i=s+1}^{p} \{(R_i^2)^{\frac{1}{2}(q-p-1)} (1 - R_i^2)^{\frac{1}{2}(n-q-p-1+2s)} [\prod_{k=i+1}^{p} (R_i^2 - R_j^2)] \, dR_i^2\} \quad . \tag{18}$$

which is the same form as (15) with $p - s$ for p, $q - s$ for q, and n for n. Hence the χ^2 approximation proposed, viz.

$$\chi^2 = -\{n - \tfrac{1}{2}(p + q + 1)\} \log_e \prod_{i=s}^{p} (1 - R_i^2),$$

with $(p - s)(q - s)$ degrees of freedom becomes appropriate (apart perhaps from its use of $n - s$ for n). In large samples the limiting sufficiency of the largest roots R_j^2 for the non-zero true roots implies further that the distribution (18) will remain approximately valid when the R_j^2 are large whether the true roots are zero or not, but it is curious that provided the *sample* values are large, the question whether the true values are large seems to become comparatively unimportant as far as the test of the remaining roots is concerned.

7. *Further Notes*

In this final section are added some miscellaneous notes.

(i) Attention has been concentrated in this paper on the problems arising in the relation of one group of variates with another. This, in the form of analysis of variance and covariance, is perhaps the most important class of problems that arises, but is not, of course, the only one. A second class that has received considerable attention in the literature is the internal "canonical analysis" of a group of variates associated with the ideas of "factor analysis," and especially used in psychology. This includes Hotelling's method of analysis into "principal components" (26); for the distributional theory of this analysis, see, for example, Wilks (54).

(ii) In the case of several dependent variates the statistical analysis can become rather heavy, and some promise that the data are such as to make the analysis worth while, as in other types of statistical analysis, is needed. The assumptions underlying the analysis need watching, for example, if these methods are applied to economic data (cf. Tintner (50)).

(iii) In the exact theory of discriminant functions outlined in section 4, it was noted that the values of the independent variates S_1 must be known. When this is not true this theory will not apply, as, for example, was pointed out in (6) in connection with applications of discriminant function technique in plant breeding. In the later discussion in that paper on approximate standard errors, there is, however, an error which requires correction. I have asserted on page 172 that the composite entries in the varieties row of the analysis of variance and covariance table, when inflated by true genetic components, may be regarded as made up of two independent random parts, whereas they are actually similar to composite entries in other analysis of variance tables based on equal numbers in the different classes in varying like homogeneous random quantities, but with composite expectations. Thus formula (13) on page 172, giving the error variability due to environment, is obviously incorrect, as it should tend to zero if the number of replications of each variety is increased. The correction and further extension of these formulae is under investigation by D. N. Nanda.

(iv) An interesting point was raised in the discussion at Boston following Brown's paper (16). It was pointed out by Cochran that covariance may be used with discriminant functions to correct for further variates (this follows from the general theory indicated in sections 2, 3 and 4, with the corresponding reduction in degrees of freedom for S_2). Such an adjusted discriminant function will be a linear function of the dependent variates and the concomitant variates, but must not be confused with a discriminant function obtained by treating the concomitant variates as further dependent variates, an analysis which would be less relevant and therefore less efficient. I understand that a paper illustrating this point on an actual example will be published by Cochran and Bliss.

(v) In section 3 reference was made to an analysis by Fisher (23, Ex. 46.2) in connection with methods of computation. Fisher's analysis is also worth studying as an example of a

canonical analysis used to determine an efficient scale for experimental scores first made on an arbitrary scale (see also Tukey (51) and my concluding remarks in (9)).

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Discussion on Dr. Bartlett's Paper

Dr. Geary, in proposing the vote of thanks: In regard to his second sentence, we may confidently assure Dr. Bartlett that he need have no doubts about the practical usefulness of his paper. With the ramifications of multivariate analysis in many directions, theoretical and practical, demonstration of the precise relations between different theories is of the first importance. Dr. Bartlett is eminently qualified for the role of co-ordinator. I like the lecturer's use of selected examples to show how different theories are related, a method which has great attraction for all but the purest of mathematicians. The well-chosen example has in it the seed of its theorization.

As most of us know to our cost, matrix algebra has pitfalls for the unwary, and I suspect that even the most adroit matricists in secret insert subscripts and superscripts like the rest of us! In a previous paper of Dr. Bartlett we were reminded that the use of the matrix convention for notational economy does not imply any change in computational method. At the hands of Bartlett, Hotelling, Wilks and other researchers, use of the notation has by analogy pointed the way infallibly from the single- to the many-variate case. The generalized variance of Wilks in the notation SS′ is perhaps the best example.

I would like to make a few observations on the remarkable distribution of the roots of the determinantal equation, discovered by Fisher and developed by Hsu, Bartlett and others. In one form or another the determinantal equation has made its appearance in many theories, latterly in Tintner's theory of linear relationships between economic series. On the latter point it has recently been shown that the Tintner theory is intimately related to Hotelling's principal component theory (1933) through the intermediary of the latent root equation.

There is one aspect of the theory which may be found interesting, but for which no novelty is claimed. Let the determinantal equation be given in the form

$$F(\varphi) = | a_{ij} - \varphi b_{ij} | = 0$$

where a_{ij} and b_{ij} are sample covariances from independent samples of n_1 and n_2 respectively from the same p-variate normal distribution, with all universal means zero and universal variances and covariances known. Then

$$\begin{aligned} F(\varphi) &\equiv (-)^p \, | b_{ij} | \, (\varphi^p - a_1 \varphi^{p-1} + \ldots \pm a_p) \\ &= (-)^p \, | b_{ij} | \prod_{i=1}^{p} (\varphi - \varphi_i) \end{aligned}$$

where the a_i's are easily ascertained functions of principal minors. In this case the Fisher-Hsu distribution is

$$C(p, n_1, n_2) \, P' \left(\prod_{i=1}^{p} \varphi_i\right)^{\frac{1}{2}(n_1 - p - 1)} \left\{\prod_{i=1}^{p} (1 + \varphi_i)\right\}^{-\frac{1}{2}(n_1 + n_2)} \prod_{i=1}^{p} d\varphi_i$$
$$\text{where } P' = \prod_{i>j} | \varphi_i - \varphi_j |$$

Effecting a change of variable φ_i to a_j, the distribution of the latter is found to be

$$C(p, n_1, n_2) \, a_p^{\frac{1}{2}(n_1 - p - 1)} (1 + a_1 + a_2 + \ldots + a_p)^{-\frac{1}{2}(n_1 + n_2)} \, da_1 \ldots da_p$$

At first sight it would appear that this form should be more workable than the φ_i distribution, since it is far more simple algebraically and its use would avoid the tiresome computation of the roots φ_i for significance testing. It is a complication, however, that the a_j universe of integration analogous to $a^2_1 - 4a_2 > 0$ for $p = 2$ is difficult to deal with.

It is remarkable that no great use has been made in practical work of the latent root distribution, despite its theoretical fascination. As far as I know, applications have been confined to the asymptotic case, to which Bartlett and Hsu have made notable contributions. On this point I would like to ask the lecturer which precisely is the theoretical relation between his theory and Hsu's theory on the asymptotic distribution (which in both theories is a χ^2) of the sum of the insignificant roots.

Dr. Bartlett made passing reference to the problem of multivariate analysis of economic data. The problem here is to discover and to assess the statistical significance or reality of relationship between variates observed at many points of time. There are, broadly speaking, two schools of thought. One school is disposed to regard the observations as subject to random error, but all the variates entering into the equations as known; the view taken is that it is the economist's job to write down the equations in algebraic form and the statistician's to determine the values of the parameters. The other school regards all the observations as free from error but certain of the variates are unknown and are represented in the equation by an error or residual term, one for each equation. Both schools agree that a theory encompassing the two approaches would be desirable, but none has been forthcoming so far. Discriminant analysis might reveal if the former school consists to a larger extent than the latter of researchers in whom a practical experience in the compilation of economic statistics has bred scepticism as to their accuracy! It is no detraction of the valuable and suggestive work of both schools to observe that truth is revealing itself in tantalizingly fitful gleams and that a "larger" theory is urgently required for each. I am quite sure that Dr. Bartlett's paper will be a help in this direction. For instance, study of Section 3 suggests that in the theory from the error-in-variate point of view (in which Tintner recently made ingenious use of the Fisher-Hsu distribution of the sum of the insignificant roots of the determinantal equation to determine the number of significant linear relationships), the disadvantage of having to assume the error variances and covariances known in advance might be avoidable.

Sir Cyril Burt, in seconding the vote of thanks, said:

I should like to voice the appreciation of psychologists and others, besides professional statisticians, to whom Dr. Bartlett's methods and illustrations will be extremely valuable. To psychologists, for whom alone I can claim to speak, two points in his paper will be of special interest. The first is his discussion of discriminant functions and their extended use. In applied psychology more particularly there are numerous problems which call for this technique. May I take one example from the many that presented themselves in the course of the work carried out by psychologists for the Forces during the war? In the Air Force we may have a large number of men classified as bombers and pilots respectively; and we may desire to use one and the same set of tests for selecting both. That means we should need to ascertain the best linear function for discriminating as clearly as possible between the two groups. The weights, however, would seem to be the same, whether we use a point biserial or a normal correlation. Still more frequently we might have, not two groups, but several groups to discriminate; in some cases these might be capable (as Dr. Bartlett's groups were) of arrangement in a series; in others (a far more puzzling type of problem) they might simply form co-ordinate classes. Provided our test-measurements had first been duly standardized to eliminate any unintended weighting, we found (as he has done in comparing his own final figures with Barnard's) that unweighted regressions give almost the same results as weighted. But what I think is most important for the practical psychologist to bear in mind is his incidental warning about the cautions to be observed in applying all such methods, wherever we are not quite sure whether the underlying assumptions are adequately fulfilled.

The second topic he has dealt with more briefly, namely, the internal canonical analysis of a group of variates, which psychologists commonly term, perhaps a little inappropriately, factor-analysis. Since this type of analysis is capable of fruitful applications both inside and outside the field of psychology, many of us will hope that Dr. Bartlett will find it possible to develop this topic a little more fully on another occasion.

I myself believe that there are close and unrecognized analogies between the aims and the methods of factor analysis on the one hand, and those of the analysis of variance on the other; and this appears to be confirmed by the approach adopted by Dr. Bartlett. Thus, it would seem that the method he has used (in Section III of his paper) to demonstrate the canonical reduction of the *general* regression problem could be readily modified to meet the conditions of the *internal*

type of analysis. Here and elsewhere (may I say in passing?) I much admire the succinctness and lucidity of the treatment which Dr. Bartlett has been able to achieve by the use of vector and matrix notation, which I think he was among the first to advocate many years ago.

There is one minor point which will be welcome to the amateur who wishes to make practical use of statistics, and that is the encouraging stress that Dr. Bartlett has laid upon the use of approximate tests of significance, particularly of the chi-squared type. In factor-analysis, for example, the ideal method is undoubtedly that to which Dr. Bartlett has referred—namely, the method of principal components. This method, which is now usually associated with the name of Hotelling, was first suggested by Karl Pearson as early as 1901, and was in fact the earliest form of factor-analysis ever proposed. It is, however, far too laborious to employ in actual practice; and factor-analysis would probably have remained unknown, had it not been for the simpler approximate methods later put forward as convenient substitutes by Spearman and other psychological workers in this country. But with these approximate methods any rigorous test for the significance of factors seems out of the question. Approximate tests have been devised; but they are hardly ever used, because the psychologist is too afraid that he may be criticized by the professional statistician for adopting inaccurate procedures.

I venture, therefore, to think that Dr. Bartlett's paper is not only a valuable contribution to theory, but of special interest to the working practitioner; and on these grounds I should like heartily to second the vote of thanks.

The vote of thanks was put to the meeting and carried unanimously.

The Chairman said he had listened to Dr. Bartlett's paper with great interest because it was a general survey of a number of problems involving multiple variates in which the solution was materially assisted by the use of the approximate χ^2 test first put forward by Dr. Bartlett in 1938 as a method of dealing generally with the Wilks criterion Λ. Possibly it was one of the minor tragedies of war that this test, which can be applied to values of p and q greater than 2, had not yet gained the currency that it deserved. The present paper should serve to give the test, and the methods to which it applied, greater prominence, and he looked forward to the time when this test, which he believed to be a remarkably good approximation to the exact test, would become as well known as Dr. Bartlett's earlier χ^2 test for the homogeneity of a set of variances.

This was a case where the problems confronting the experimenter had been leading before the war to almost simultaneous treatment by the theoretical statistician. Dr. Bartlett had referred to some calculations he had made in 1938 when he (the Chairman) opened a discussion on the statistical treatment of animal experiments. Now in an earlier paper (published in *Biometrika*) he had said that "were the decisions reached by separate examination of the growth rate and change of growth rate figures not so clear-cut, it might be necessary to take these figures (g and h - i was added later) together in a simultaneous analysis of variance and covariance, and reach a single test of significance of the effect of food (or of sex) on both simultaneously, after the manner suggested by Bartlett." The reference here was to Dr. Bartlett's 1934 paper (reference 3). But at that time the test which he had drawn attention to now had already been worked out, although the paper (reference 4) was not published until after the paper containing the above quotation was written.

This paper continued, "Not only so, but the fact that it is desirable to take initial weights into account suggests that Bartlett's method should be applied to the variables derived from g and h (and i) when w_0 is held constant, and a test of significance derived in the same sort of way as in the usual covariance analysis." Dr. Bartlett's calculations, made at the time of the 1938 discussion and reproduced in the present paper, did in fact employ the covariance method to eliminate the initial weight of the animals, but he fancied that Dr. Bartlett had forgotten that fact, because not only was it not referred to in Section 3 of the present paper (where n should be 21, not 22), but he also referred in Section 7 (iv) to some work of Cochran as if it were a new development, whereas the only thing new about it was its application to discriminant functions. This did not, of course, detract from the value of Dr. Bartlett's paper, and the point he would make in conclusion was that a great stimulus was often to be had in the development of new theoretical results by studying the experimental work that was going on all the time.

Mr. C. Radhakrishna Rao said attempts had been made in recent years to generalize the technique of analysis of variance of a single variate to the case of several mutually correlated variables. Except in the case of comparison between two groups for which Hotelling's distribution supplied the complete solution no exact tests had been put forward for practical use. When one went through the complicated mathematical symbols and formulae (perhaps for no fault of the authors) appearing in the journals on this topic, there came a feeling that there was no imme-

diate prospect of these being made available for ready use. Dr. Bartlett's illustrations of tests in his paper were particularly important in that they satisfied, to a large extent, the needs of an applied worker in multivariate problems. But there were a few points on which the speaker sought clarification.

The variables $x_1, \ldots, x_p, y_1, \ldots, y_q$ might stand for $p + q$ mutually correlated characters for which samples of sizes $n_1, n_2, \ldots, n_k$ were available from k populations. An important problem which often arose in biometric studies was to test whether all the differences among the k populations could be explained by the differences in the x's only, or, in other words, whether any additional information was gained by using the y's in association with the x's.

Dr. Bartlett called this an internal analysis of S_2, and proceeded to say that an alternative factorization of Λ based on $p + q$ variates corresponded to the separation of S_2 into two groups S_0 and $S_{2\cdot 0}$ so that $\Lambda = \Lambda'\Lambda''$, where Λ' corresponded to the x's. The statistic Λ'' was used to test for the additional information supplied by the y's.

As an alternative to this one might set up a multiple analysis of variance and covariance table involving the q y's treated as dependent variates and the p x's as independent variates. This supplied, on eliminating the variations due to the x's, the dispersion matrices:

(i) $W(y \mid x)$ based on "within" analysis of populations having $n_1 + \ldots + n_k - k - p$ degrees of freedom, and

(ii) $M(y \mid x)$ based on "between" analysis of populations having $(k - 1)$ degrees of freedom.

The problem was one of comparison of W and M. The theory of canonical roots could be applied so that one could compute the maximum, minimum or any intermediate root λ of the determinantal equation:

$$| W + M - \lambda W | = 0$$

The significance of the individual roots could then be tested for. An overall test was provided by the ratio $| W | / | W + M |$. Using Bartlett's approximation the statistic

$$-[\Sigma n - k - p - \tfrac{1}{2}(k + q)] \log_e \frac{| W |}{| W + M |}$$

could be used as χ^2 with $q(k - 1)$ degrees of freedom.

This test, perhaps, differed from Dr. Bartlett's Λ'' criterion and might lead to a different interpretation. It was easy to recognize that in the above problem some of the x's might be in the nature of concomitant variates. Following this method the problem of Table III was identical in comparing the matrix $M(i, h \mid g, w)$ representing the variances and covariances of i and h due to treatments when g and w, the initial weight of pigs, were eliminated, with the matrix $W(i, h \mid g, w)$ representing the variances and covariances due to error when g and w were eliminated. Again, the theory of canonical roots could be employed to test whether g was sufficient to explain the differences in the growth curves. An unsymmetrical test using $h.g$ and $i.hg$ was not convincing since an alternative resolution $i.g$ and $h.ig$ might reveal a different story. On the other hand, a complete test for i, g, h, after eliminating w was supplied by the comparison of matrices $M(g, h, i \mid w)$ and $W(g, h, i \mid w)$.

The second problem to which he drew Dr. Bartlett's attention was associated with his treatment of Barnard's data. There were four variables, x_1, x_2 x_3 and x_4, and four groups. The object was to test whether the differences in the groups could be explained by linear regression of individual variates on time separating the groups. Dr. Bartlett used an alternative factorization of Λ.

If they denoted the internal dispersion matrix of Table V by W, that due to within plus regression of Table VI by $W + R$ and the total of Table X by T, they found the dispersion matrix of deviation from regression as $T - W - R$ with 2 d.f. The matrix W had 394 d.f. and the problem was again one of comparing these two matrices. An overall test was supplied by the ratio:

$$\frac{| W |}{| T - W - R + W |} = \frac{| W |}{| T - R |}$$

In the above example he found:

$$| T - R | = \begin{vmatrix} 9665\cdot 247740 & 449\cdot 008478 & 1149\cdot 501013 & 2142\cdot 197474 \\ & 9099\cdot 726441 & 1265\cdot 691535 & 2231\cdot 524444 \\ & & 4049\cdot 689004 & 1203\cdot 298256 \\ & & & 9257\cdot 368621 \end{vmatrix}$$

$$= 10^{12} (\cdot 26873816)$$

and $W = 10^{12} (\cdot 24269054)$ so that the ratio was $\cdot 90307436$.

The value of χ^2 in this case was

$-[396 - \frac{1}{2}(4 \text{ plus } 2 \text{ plus } 1)] \log_e \cdot 90307436 = 40.02$ with 8 d.f. whereas $\chi^2 = 41 \cdot 50$ according to Dr. Bartlett's criterion. These two tests appeared to be giving nearly equal results. He sought Dr. Bartlett's views on the comparative advantages of these tests he was inclined to put forth and his Λ'' criterion.

It might be noted that the above test could be applied even if the problems were to test (i) whether a parabolic regression with time was adequate to explain the differences, and (ii) whether a parabolic fit was better than a linear fit. One had to get the appropriate matrix due to deviations from the hypothesis and compare it with the error matrix. The comparison might be done by testing the significance of the canonical roots or their symmetric functions. They thus had a generalization of analysis of variance to multivariate problems which may be termed as *Analysis of Dispersion.*

For further details relating to tests by analysis of dispersion and the exact distribution of the Λ criterion he referred the interested reader to a paper by him due to appear in a forthcoming issue of *Biometrika.*

Dr. HERDAN said that the underlying unity of structure between univariate and multivariate analysis of variance had been excellently exhibited in Dr. Bartlett's paper. It might perhaps not be amiss to say a few words about the difference between these methods and the different purposes they served in practical problems. This would also afford an opportunity of stressing the differences in structure, in spite of the fundamental unity.

Such differences were brought out clearly in certain typical cases of industrial research where the various methods of analysis were used to supplement one another.

To fix our ideas we could think of an investigation into the protective properties of anti-corrosive paints. The various compositions of such paints differ in their chemical and physical properties, and it is of interest to know which composition affords better protection. Each composition is put on steel panels, which are then exposed to atmospheric influences and are inspected at regular intervals for corrosion. Corrosion, however, is a complex concept and it is described by various characteristics such as: the complete paint film intact (per cent. by area), anti-corrosive paint only intact (per cent. by area), bare but unrusted steel (per cent. by area), rusted metal (per cent. by area), depth of pits, loss in weight, blistering, flaking, cracking, etc.

It was conceivable to apply to a problem of this kind analysis of variance and covariance which would give the regression of the change in composition on the various properties descriptive of corrosion. But although this was formally possible, it was doubtful whether it would be of any practical value, since what one was interested in in such experiments was the overall question which composition afforded a better protection against corrosion as described by all the examined characteristics together. It was usual, therefore, to sum the marks given for each characteristic, either unweighted or arbitrarily weighted, and thus to arrive at a figure of merit for each panel.

There were two ways of improving this by statistical methods: as an objective way for arriving at the figure of merit, the theory of discriminant functions, or of canonical correlations, suggested itself. The various examination results were recorded as so many variables which, on the basis of the intercorrelations, could be compounded into one overall variable—the discriminant function. Every panel was then characterized by one number only and the total could be subjected to univariate analysis of variance.

The same was achieved by canonical correlations or by factor analysis where the factors took the place of the discriminant function. Although, primarily, in this case, we aimed at and obtained a description of the variables in terms of the factors, we then arrived by regression or other methods at a linear combination describing the factors in terms of the variable. Having done this, we could then apply univariate analysis to find out which chemical ingredient or physical properties in the various compositions made for better protection. Thus, multivariate analysis was here used as a preliminary to univariate analysis of variance.

Wherein now lay the specific function of factor analysis as another form of multivariate analysis? We should realize that there was a certain arbitrariness in compounding all variables into one linear relation as was done in arriving at the discriminant function. This could be partly cured by constructing two or more discriminant functions, but even so we were arbitrary in grouping the variables. In certain cases the linear combination of variables was desirable, but arbitrariness in grouping must be avoided. The testing of rubber might serve as an illustration. Rubber was, at various stages of manufacture, subjected to various tests like abrasion, elongation, hardness, resilience, indentation, stress, benzene swelling, tensile strength. All these tests were highly correlated with one another, some positively, some negatively, and it seemed desirable to arrive at what might be called the true dimensionality of the problem

by ascertaining whether the number of tests could not be reduced to a comparatively small number of factors. It would, however, be risky in cases like this to combine all the tests into one linear combination or into arbitrary groups. What we needed, therefore, was a method for arriving at the true dimensionality of the tests. This was precisely what could be done by means of factor analysis, since factor analysis was essentially a method by which to ascertain whether an assumed pattern of grouping of variables was justified. It was the essence of factor analysis to confirm or invalidate the provisional allocation of variables to groups of linear combinations. It yielded a structure and a pattern, whereas in discriminant functions, although we had a structure, the pattern was arbitrary. For testing rubber, therefore, it would seem desirable to reduce the number of tests by legitimate methods, like factor analysis, to the smallest number of factors compatible with the complex of correlations, and then to subject each of these factors to analysis of variance with the object of finding out which ingredients or properties made for significant test results.

Regarding factor analysis as one case of multivariate analysis threw an interesting light on the objection frequently raised against factor analysis that factors are only mathematical constructs, and thus have no reality. It was readily conceded that the identification of factors was no longer part of factor analysis, and could only be achieved without factor analytical methods with some degree of uncertainty. But, according to the interpretation just given, that objection seemed beside the point. The compounding of a number of observable variables into one or a few factors was not meant to yield again an observable variable, and could not be expected to do so. All we could expect to get in this way was a hypothetical variable which might or might not correlate with something in nature. The justification of factor analysis was that it facilitated description, to which Dr. Herdan believed attention was first drawn by Professor Cyril Burt in his *Factors of the Mind*. It had been said by H. Poincaré that "geometry is not true but convenient." Why should we expect more from factor analysis?

It appeared that in certain respects multivariate analysis was the reverse of univariate analysis. Whereas in univariate analysis we aimed at splitting up the total variance into partial variances corresponding to the various causes which produced the total effect, in multivariate analysis we combined a number of variables linearly into one new variable. Thus, in univariate analysis of variance we had one effect which we explained as depending on a number of causes, whereas in multivariate analysis of variance we had one cause whose regression upon a number of effects we wanted to establish. This could be clearly seen from the illustrations in Dr. Bartlett's paper: the variables "treatments," "food," and "time" occupied purely formally the position of the variables whose total variance was analysed into two parts, one being the chance residual and the other representing the amount of the total variance accounted for by the regression of "treatments" upon "grain" and "straw," of "food" upon the constants g, h, i, and of "time" upon the changes in the skull dimensions x_1, x_2, x_3, x_4, and thus appeared as the "dependent" variables. Yet there could be no doubt that they were, in fact, the physical causes of the changes in the "independent" variables which represented their physical effects. All this suggested that a suitable term comprising the various forms of multivariate analysis of variance would be "Synthesis of Variance."

There was an error in Dr. Bartlett's paper which might be confusing to those interested in the changes of skull measurement with time. Incidentally the same error appeared in Mr. Kendall's *Advanced Theory of Statistics*, Vol. II. Although the order of the quantities in Table IV and in the final results was the same as in the original paper by Miss Barnard, the order in the description of the tests (p. 182 of Dr. Bartlett's paper) was not. The characteristics in question corresponded to those denoted by x_1, x_4, x_6 and x_7 in the original paper, and in this order: maximum breadth, face length, nasal height, basibregmatic height.

According to the original paper, there was an increase with time in maximum breadth and nasal height and a decrease in the other two characteristics. On the basis, however, of the list given in p. 182 of the present paper there would be an increase of face length and maximum breadth and a decrease of the other two.

Dr. C. A. B. Smith, drawing attention to discriminant functions and the discrimination between two populations, said he would like to ask Dr. Bartlett what assumptions were being made in the application of multivariate analysis to the theory of discriminant functions. It was well known that in ordinary significance tests the departure of the underlying distribution from normality was not important, because there would be a tendency to normality in the distribution of, e.g., the mean of a sample, but in discriminating between two different groups they were discriminating the single individual each time, and the departure from normality would, as a result, make the discriminant function less powerful. In fact, if the variance and covariance were not the same in the two groups, the usual linear function would not be the best function or even the best linear

function. It was even found in the data with which they had to deal at the Galton Laboratory (psychological and biometrical data) that the distributions were seriously non-normal, and in dealing with normal and abnormal subjects, the abnormals frequently had greater variance than the normals. He asked, therefore, if anything was known as to how much the tests would be affected by non-normality.

Mr. MORONEY had met with an industrial problem which could be described in terms of discriminatory analysis. It related to the appraisal of cements used in metal to rubber bonding. The various measurements which could be taken during manufacture gave a guide to subsequent performance, but certainly no guarantee that the cement would stick well. In facing this type of problem he had been led to the idea of predicting performance from the complex of measurements by the use of some discriminatory function. He wondered whether Dr. Bartlett knew of any similar application in the industrial field. He was sure industry offered great scope for this branch of theory.

Dr. BARTLETT, replying to the discussion, said that in regard to Dr. Geary's query about the relation of his χ^2 and the χ^2 approximation obtained by Hsu for the sum of the squares of the canonical roots, the fact was that the two tests were asymptotically equivalent. They depended on the same χ^2 distribution if one made the sample large enough, but the further point he was concerned with in the χ^2 test he had used in the paper was to have a test which was, as far as possible, of known accuracy in finite samples. He had pointed out where it had sufficient accuracy and where it needed further investigation.

In regard to Dr. Rao's remarks on the possible alternative methods of testing, he did not, he was afraid, quite follow the exact basis of the alternatives he proposed. He (Dr. Bartlett) would, therefore, like to study his remarks in writing before attempting to comment. He was interested in Sir Cyril Burt's reference to Karl Pearson, with which he, personally, was not familiar, in suggesting that the reduction to principal components went back as far as 1900.

With reference to Dr. Smith's query about the effect of non-normality and unequal variance, that was a question about which he reserved the right to reply. He did not believe it had been given much thought. and he fancied the result would depend on the purpose of the canonical analysis. In the case of the grain or straw yield or the pig analysis he rather doubted whether the application of the discriminant function in the sense of which Dr. Smith was thinking was necessarily required and, therefore, the known result that analysis of variance which referred to the sample as a whole was fairly insensitive to such departures would apply also to the canonical analysis, but the case where one merely used the discriminant functions on a further single observation was a problem which, he agreed, wanted further consideration.

He was afraid he could not help Mr. Moroney; he could not place any industrial application of the type that Mr. Moroney was asking for. He did not know whether anyone else could help, if so, perhaps he would be good enough to write to the Society or to Mr. Moroney giving the reference.

Dr. BARTLETT replied further in writing as follows:

The chief point outstanding seemed to be to clear up any possible differences between Dr. Rao and himself. Dr. Rao had not made the point at issue very clear, as his method for his first example seemed identical with the method proposed in the paper. This was assuming that Dr. Rao's function M was obtained as usual by subtracting the "within" term W from the "total" term $M + W$, as was necessary from the $k - 1$ degrees of freedom assigned to it; in this case the total degrees of freedom would be $n - p - 1$, and not $n - k - p$, and the best numeral coefficient $n - p - 1 - \frac{1}{2}(k - 1 + q)$. Again, in the pig weight analysis, the unsymmetrical test of g, $h.g$ and $i.gh$ was deliberate, as the quantities g, h, i were naturally considered in that order.

But Dr. Rao had put his finger on a real point that wanted clarifying. It arose in the g, h, i analysis and also with the skull data, but in principle it had nothing to do with multivariate analysis, arising just as well in an ordinary analysis of variance. The point was simply this. Suppose A, B and C were three independent sums of squares, of which the first two were to be tested against the residual term C. The complete null hypothesis was tested by the ratio $C/(A + B + C)$, which had two alternative factorizations into independent quantities,

$$\frac{C}{A+B+C} = \frac{C}{B+C}\,\frac{B+C}{A+B+C} = \frac{C}{A+C}\,\frac{A+C}{A+B+C}$$

If logarithms were taken, either factorization gave an additive analysis of the left-hand side. If B were not inflated by any real effect, the second factor of the first factorization gave the most efficient test of A. But if B did contain a real effect, the first factor of the second factorization, being independent of B would be, in general, a more sensitive test of A, and would be used although no longer additive with the test of B.

He could not say he liked Dr. Rao's phrase "analysis of dispersion," preferring the more self-explanatory "multivariate analysis of variance."

With regard to Dr. Herdan's remarks, he was not sure that these clarified the relation of an internal factor or canonical analysis with the external canonical analysis he had been mostly concerned with in his paper. Dr. Bartlett agreed, of course, that both were cases of multivariate analysis, but felt it most important not to confuse people into thinking that an internal factor analysis of a set of variates could give any information whatsoever on the external relation of this set with a second set. It was still a common fallacy, for example, to assume that the major factor emerging from an analysis of a set of marks in different tests was necessarily the best guide to a student's ultimate performance, whereas of course the question is unanswerable unless "ultimate performance" is defined, measured and correlated with the different test scores.

He was indebted to Dr. Herdan and to Dr. Wishart for drawing attention to slips in the paper. These he had now corrected in the text. While it was true that he had, as Dr. Wishart suggested, forgotten the use of covariance to eliminate initial pig weights, there was a new application involved in the application by Cochran and Bliss to discriminant functions, although he agreed and had in fact mentioned in the paper that no new principle arose.

He did not think on consideration that he had anything useful he could add to his spoken comments on the question of non-normality raised by Dr. Smith, except to draw attention to two recent papers in the *Annals of Eugenics*, **13** (1947), from the Galton Laboratory ("Some notes on discrimination," by L. S. Penrose, p. 228; "Some examples of discrimination," by C. A. B. Smith, p. 272). These referred to the effect of unequal variances; the general problem of the effect of departures from the standard assumptions would, he hoped, receive further investigation.

Dr. Bartlett concluded with thanks to all the speakers for their contributions to the discussion.

METHODS OF DEFERRED SENTENCING IN TESTING THE FRACTION DEFECTIVE OF A CONTINUOUS OUTPUT

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SUMMARY

WHEN the output of a continuous process is subjected to a routine test, in which it is observed whether the articles sampled are satisfactory or defective, a clustering of defectives (i.e. the occurrence of several defectives in rapid succession) may be taken as an indication that quality has deteriorated. In Part I inspection schemes of this kind are considered and compared with sequential sampling methods applied to the product after it has been divided into bulks of a suitable size. In Part II it is shown how the operating characteristic of a deferred sentencing scheme can be calculated.

These schemes are, we believe, the only inspection procedures available at present that relate specifically to a continuous output and apply when the inspection test is destructive. They are non-rectifying.

Mathematically, we are concerned with, among other problems, the frequency of clusters of random points on a line of infinite length.

Most of the work on which this paper is based was carried out by the authors in the Ministry of Supply Advisory Service on Statistical Method (S.R. 17), and their thanks are due to the Director-General of Scientific Research (Defence) for permission to publish it. Part I and the first section of Part II are by F. J. Anscombe, and the remainder of Part II by H. J. Godwin and R. L. Plackett, as indicated by author's initials.

PART I.—DESCRIPTION OF METHODS

TESTING CONTINUOUS PRODUCTION

1. WE consider routine tests on a mass-produced article, under the following conditions.

(i) We are concerned with testing the output of a single production line. The quality of the output is *continuous*, in the sense that production is not in recognizable batches of uniform quality. If a change in quality, temporary or permanent, occurs, it may occur, so far as we know, at any time. If it is the producer's practice to despatch his product in "lots" bearing a lot-number, the size of the lot is determined solely by considerations of convenience in despatching or book-keeping or whatever, and is not determined by anything known to have an important influence on quality of output. For example, each 1000 articles may be boxed up to form a "lot" as they come off the line, simply because 1000 is a convenient number for book-keeping. (Alternatively, the product may be known to be considerably influenced by some factor, such as the shift or the batch of raw material, and so naturally falls into batches of relatively uniform quality, but these batches are too small in size for adequate individual testing. The uniformity of the batches is then of no help to the inspector.)

(ii) We are concerned with the final test of the articles produced, and the test is destructive and costly, and therefore carried out on a small scale. It is a functioning test, i.e. an article tested is either satisfactory or defective; and it is applied to samples from the whole of the output. Its purpose is to check that quality is satisfactory rather than to weed out bad from good lots. As a result of the test lots are either accepted or rejected, where by rejection may be meant outright scrapping or investigation followed perhaps by an attempt to remedy the fault. The test may be carried out by the producer or the consumer, for different reasons (we shall actually consider it from the consumer's point of view).

2. The method by which this inspection problem has often been met is to select a small number of articles, perhaps three or five, from each lot, test them, and if no defectives are encountered accept the lot, and if one or more defectives are encountered reject the lot, or select and test a few more articles, accepting the lot if no further defects are observed, or some such rule. Thus

each lot is sentenced separately, by a single-sampling or double-sampling method. But the number of articles tested is so small that the test is very insensitive. A 10 per cent. defective lot would clearly stand a high chance of being accepted under such a scheme, and in some instances that have been observed a considerably lower fraction defective than 10 per cent. was considered unacceptable by the user. Then such an inspection scheme exercises only a moral control of the quality. And from the producer's point of view such a scheme is discouraging, since usually the lots rejected will be no different in quality from the neighbouring lots accepted.

3. One possible method of achieving an effective direct control of the quality, without increasing the rate of test (i.e. the number of items tested per 1000 made), is to group the product into larger bulks than the manufacturer's "lot", and apply an efficient sampling method, which might be, for example, a Wald sequential scheme (Refs. 1, 2 and 7), to each bulk. Each bulk would be accepted or rejected as a unit, and we should have a guarantee of the average quality of the bulk.

4. An alternative way of tackling the problem is by the method now to be described. It was conceived and first applied by H. B. Spalding, P. Halliday and E. H. Sealy, in 1942, in connexion with some military stores (Refs. 5 and 6). Since then it has been used fairly widely, and subjected to a good many variations. The underlying idea is as follows.

It is supposed that the quality of the output remains constant in stretches, and that when there is a change in quality the change is sudden, or a gradual trend in one direction, but not a rapid oscillation. (Rapid oscillations will not, in fact, be observable with a low rate of testing, but only the general trend.) Any possible precautions will be taken by the manufacturer to ensure that this state of affairs holds, by, for example, using up raw materials in the order of their supply, and avoiding a continuous belt system or anything confusing a direct orderly flow along the production line. (Precautions of this kind are often not possible, however.) If now items are selected for test at a regular rate, and if the quality of the product remains constant, defectives will be found with a constant probability in a random distribution. This will involve occasional accidental clusterings of the defectives, when they will occur in rapid succession. If the quality deteriorates the clusterings will become more frequent and pronounced. The method of sentencing consists of deciding on a criterion for a significant cluster and rejecting the product represented by such a cluster. In general, sentence on a lot or batch of the product is not passed as soon as the test items from the batch have been tested, but is delayed until the immediately following batches have been tested, in case a significant cluster should develop. Such schemes have therefore been given the name of *deferred sentencing.**

The Simplest Deferred Sentencing Scheme

5. The simplest type of deferred sentencing scheme is the following, and this has been the most fully investigated.

> *Sentencing rule.*—The product, as it leaves the line, is divided into small lots, and one item is selected from each for test. D and n being given integers, whenever n defective items are encountered out of D or fewer consecutive lots tested, all the lots consecutively from that giving the first to that giving the nth defective in the cluster are rejected. Lots not rejected by this rule are accepted.

A run of clear lots, i.e. lots giving satisfactory test results, of length D will be accepted at once, and so will following clear lots. If a defective occurs, sentence will be suspended until either a further $D - 1$ lots have been tested or $n - 1$ further defectives have been encountered, whichever occurs the sooner. In the first case, if the $D - 1$ following lots give fewer than $n - 1$ defectives, the first defective and any succeeding clear lots will be accepted. As soon as n defectives occur in not more than D lots, all lots not so far sentenced will be rejected. Thus sentence will sometimes be passed at once and sometimes with a delay not exceeding $D - 1$ lots. Some of the lots to be rejected according to the sentencing rule may already have been rejected through the operation of the rule on a previous cluster of n defectives partly overlapping the one being considered. The actual number of new lots rejected whenever the rule operates may therefore be any number from 1 to D.

* They were first known by the not wholly fortunate name of "rational sentencing". The word "sentencing" itself is that regularly used by Government Inspectorates to denote their decision to accept or reject lots of ammunition submitted to them.

6. We shall choose D and n so that if the quality of the product is satisfactory, there is only a very small risk of any product being rejected owing to an accidental clustering of defectives in the inspection. We shall also require that if the quality deteriorates to a serious extent, as much as possible of the product will be rejected. In fact, given D, n, and the fraction defective p of the product, assumed to be constant, it is possible to calculate what proportion of the product will be rejected. Solutions were found simultaneously by Godwin and myself, and are given below in Part II. Godwin's approach, the enumeration of all possible acceptable or rejectable combinations of defectives, appears to be the more powerful, as it has been applied successfully to more complicated deferred sentencing schemes (examples at the end of Part II).

7. The numerical results of this investigation are summarized in the table below. It has been calculated on the assumption that the fractions defective are small, i.e. for the limit as $p \rightarrow 0$. Percentage points of Dp are given, and their interpretation will be clear from an example. Suppose it is desired that output that is constantly $\frac{1}{2}$ per cent. defective should be almost all, namely 99 per cent., accepted, in the long run. The appropriate value of D for a given value of n is found by dividing the entry for Dp in the 99 per cent. column by the value of p, namely ·005. Thus if we choose $n = 5$, we shall need $D = 194$. Reading along the row for $n = 5$, we see that if the fraction defective of the output is constantly $\frac{1 \cdot 84}{194}$, i.e. 0·95 per cent., then 90 per cent. of it will be accepted in the long run. Similarly, if the fraction defective is 1·8 per cent., 50 per cent. of the output will be accepted, if it is 3·1 per cent. only 10 per cent. will be accepted, if the fraction defective is 4·6 per cent. only 1 per cent. of the output will be accepted.

Percentage Points of Dp

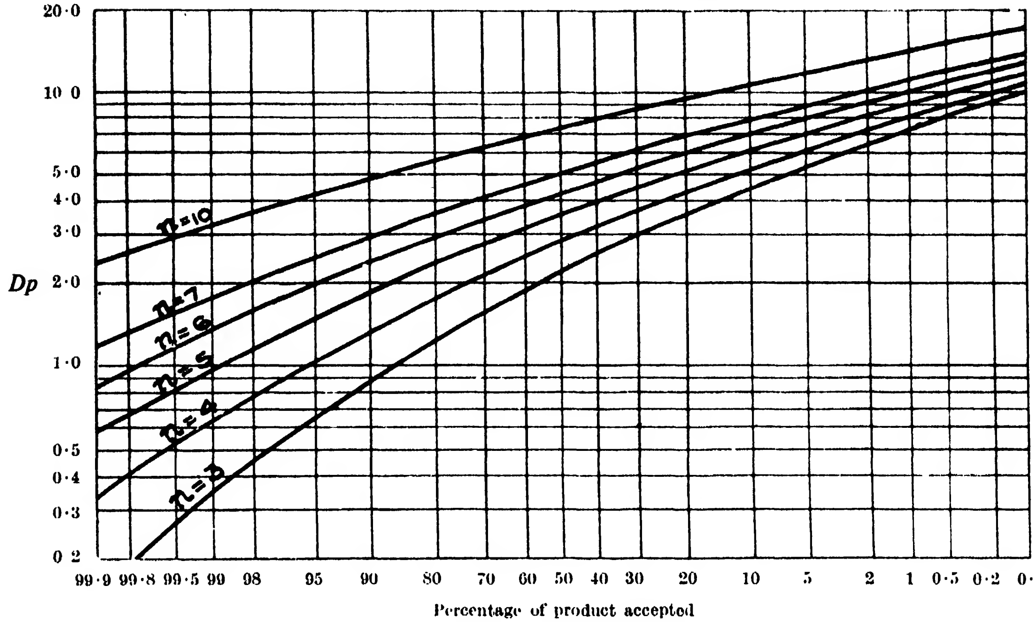
n	Values of Dp for which a given proportion of the output will be accepted				
	99 per cent.	90 per cent.	50 per cent.	10 per cent.	1 per cent.
3	·35	·89	2·20	4·5	7·3
4	·63	1·34	2·84	5·3	8·1
5	·97	1·84	3·54	6·1	9·0
6	1·36	2·39	4·28	7·0	10·0
7	1·78	2·96	5·04	7·9	11·0
10	3·25	4·85	7·43	10·8	14·2

If, for any n, the value of Dp is plotted against the percentage acceptance on logarithmic probability graph paper, the points lie on a slightly curved line, and intermediate percentage points can be read off (see Figure).

8. For a given scheme (i.e. given D and n), the graph of the proportion of the output accepted against the fraction defective p, assumed constant, may be termed the *operating characteristic* of the scheme, by analogy with sampling schemes for bulks of a definite size (see for example Ref. 2). It should be noted, however, that there is some difference in kind between the two concepts. With a deferred sentencing scheme the operating characteristic shows the *proportion* of the output that will be accepted in the long run if its quality is constant, or the *chance* that any particular lot will be accepted, if the quality remains constant for several lots on either side. On the other hand, if the product is divided into bulks and a sampling scheme applied to each separately, the operating characteristic gives the *chance* that any single bulk will be accepted (irrespectively of the quality of neighbouring bulks), or, if all the bulks should have the same quality, the *proportion* of the output accepted in the long run.

9. We consider now how to choose n, the size of defect cluster in the sentencing rule. If the quality of the output remained constant, or only changed very slowly, the properties of the sentencing scheme would be summed up by the operating characteristic. With any given quality of output, a certain proportion of the output would be rejected, according to the operating characteristic, and this independently of the rate of testing, i.e. of the number of articles tested per 1,000 produced. It is obvious from the table above that the larger the value of n the sharper is the distinction given by the scheme between good (acceptable) and bad (rejectable) quality; for the ratio of the values of p for which there are any two proportions of the output accepted, e.g. 99 per cent. and 1 per cent., approaches 1 as n increases. Thus the scheme will be most sensitive, and therefore most useful, when n is large.

Operating characteristics of simple deferred sentencing schemes



But in fact we are not much concerned with constant quality of output, excepting constant good quality, but rather with possible sudden deteriorations in quality, of which we require that the sentencing scheme shall give early warning. A sudden deterioration that is sufficiently serious will be detected sooner with a small value of n than with a large one. So we have two opposing principles, sensitivity and flexibility, requiring respectively a large and a small value of n. Of course, the flexibility of the scheme (its power to detect a change in quality quickly) can be increased by increasing the rate of testing; but when that has been done as far as circumstances permit the choice of n will still be a matter of judgment.

10. When deferred sentencing schemes were first proposed, the intention was that the value of D in the sentencing rule should not be based on a fixed standard of good quality, but on the recent average quality of the process. The latter was to be estimated from a run of test results, and D would be chosen so that if the quality stayed at its previous level the product would have a stated chance, such as 99 per cent., of being accepted. It was argued that the quality was expected to stay constant, and that the prime object of the inspection was to detect any change in quality. If a change were detected the reason for it should immediately be sought, even if the quality were still acceptable; and meanwhile, if the product were not rejected at once, sentence should be suspended until it was clear what the new quality level was. If the quality became stable at a new level, and this was acceptable, a new value of D would be used appropriate to the new level. This reasoning is in line with the theory of ordinary statistical quality control (see for example Ref. 4).

But against this practice there were two difficulties. If a significant cluster of defectives in the test led to outright rejection of the relevant part of the output, or at least to suspension of sentence pending investigation, and if the consumer was applying such a method of inspection to the output of two factories making the same article, the better of the two factories was more severely treated than the worse, in that relatively poor product from the better factory might be rejected, while equally bad product from the worse might be accepted. A good deal of explanation was then necessary. And also it was found that the inspectors who operated the sentencing rule were unwilling to recalculate the value of D from time to time to allow for small changes in the

general level of quality. It became clear that the ideal of constant quality, "the best the factory could do", must be replaced by a stated standard of acceptable quality, arrived at by considering the quality of available supplies and the consumer's needs, which should apply equally to any producer; and that the method of sentencing should not be made a means of bringing pressure to bear on the producer to improve his quality beyond this standard.

It should be noted that even if standards of quality are laid down the inspection scheme is not likely to be applied entirely without regard to the quality actually achieved. If the average quality of a factory is poor, relative to the standard of good quality, a certain proportion of the output will be rejected, and either the standard must be relaxed or the factory will stop work. It is possible to insert a clause into the contract to take explicit account of the average quality of the output, to the effect that, in addition to the control afforded by a deferred sentencing scheme with fixed constants (D, n, etc.), the average fraction defective of the output shall be calculated from time to time by pooling the test results of the last so-many-hundred lots; this should not exceed a stated figure, and if it does so sentence may be suspended on further output while the question of relaxing standards is being considered.

The problem of deciding on standards of good and bad quality arises in all sampling inspection. For a discussion see Ref. 1.

OTHER DEFERRED SENTENCING SCHEMES

11. We turn now to possible developments of the simple deferred sentencing scheme so far considered.

(i) *Retest.*—When a number of consecutive lots have been rejected, they may be treated as a single batch and given a further sampling inspection on a more generous scale. A sequential scheme can be used, for instance. The operating characteristic of the resulting procedure can be very easily deduced, since the chance that any lot shall finally be rejected is the chance that it will be "rejected" by the simple deferred sentencing scheme multiplied by the chance that it will be rejected in the subsequent retest.

The arrangement suffers from what may be a serious practical disadvantage, namely, that after the delay involved in the "rejection" under the preliminary deferred sentencing scheme there is further delay while the retest is being arranged and carried out. It may be preferable to augment the rate of testing temporarily whenever a defective is encountered in the initial inspection, in a routine manner, without waiting to see whether a cluster of defectives is developing. Thus we have the following scheme.

(ii) *Two rates of testing.*—In the absence of defectives, one item is tested per lot. Immediately after a defective the rate of testing is increased to one item per one-kth part of a lot (i.e. one per lot of one-kth the previous size). After C consecutive clear items have been found, the lower rate of testing is resumed. Whenever n consecutive defectives occur within D or fewer items tested, all lots or part-lots consecutively from that giving the first to that giving the nth defective are rejected.

The operating characteristic of this scheme is given in Part II. We have the following numerical results for $n = 5$, $C = \frac{1}{4} D$.

Percentage Points of Dp

n	$\frac{C}{D}$	k	Values of Dp for which a given proportion of the output will be accepted: 99 per cent.	90 per cent.	50 per cent.	10 per cent.	1 per cent.
5	0	—	·97	1·84	3·54	6·1	9·0
,,	$\frac{1}{4}$	2	1·06	2·00	3·82	6·5	9·5
,,	$\frac{1}{4}$	3	1·11	2·09	3·99	6·8	9·8

It will be seen that the introduction of the higher rate of testing has slightly sharpened the operating characteristic, i.e. the ratio of the 99 per cent. and 1 per cent. values of Dp is nearer 1. If such a scheme is compared with a simple scheme with only one rate of testing, such that the average rate of testing of the two schemes is the same when quality is at the 99 per cent. acceptance level, the main difference in effect between the schemes is that the first scheme will lead to a rejection sooner than the second after a deterioration in quality has occurred, and that then the

average rate of testing of the first scheme will be greater. The fact that most of the output which is rejected will have been tested at the higher rate may help to render the scheme acceptable to the producer.

(iii) *Rejecting on either side of the cluster.*—A modification of the simple scheme of §5 that might be considered is as follows. One item is tested per lot. D, n and G being given integers, whenever n defective items are encountered in D or fewer consecutive items tested, all the lots consecutively from that giving the first to that giving the nth defective, together with the G lots next on either side of these lots, will be rejected. Thus one cluster of n defectives may involve the rejection of as many as $D + 2G$ lots.

We should in practice take G to be considerably smaller than D. One might, for example, choose $G = \frac{D}{2(n-1)}$. The properties of such a scheme have not been investigated, but it seems intuitively likely that it would prove useful.

(iv) *Two simple schemes together.*—Another modification, designed to satisfy the opposing requirements of sensitivity and flexibility mentioned in §9, is to make use of two rejection rules like that of §5, with constants D_1, n_1, and D_2, n_2, applied simultaneously to the same single series of test results. We might, for example, have $n_1 = 5$ and $n_2 = 10$. The operating characteristic of such a scheme seems to be difficult to obtain, and no attempt at investigation has been made. The two rejection rules do not, of course, act with statistical independence (output rejected by one rule will tend to be rejected by the other also), but an upper limit to the proportion of the output rejected is got by adding the proportions rejected by the schemes separately, and either of these proportions alone is a lower limit. This remark would probably suffice for most practical purposes.

(v) *Several items tested per lot.*—A modification of the preceding schemes that will usually be made in practice arises from the inconvenience or impossibility of storing the product in small identifiable lots relating to single test items, pending sentence. If the rate of test is 5 per 1,000, according to the above rules the product, as it comes off the line, should be aggregated into lots of size 200, and each lot kept distinct until sentence is passed. Such a small lot size may be inconvenient to the manufacturer, if there is any considerable delay in the testing (as there might well be if the test were one of endurance), as then he will always have a large number of such small lots in his store, each required to be kept distinct. It may be more convenient to label his output in lots of size 1,000 or 2,000, and not accept or reject in smaller amounts. And in some cases the lot size is determined by the process, as when the product consists of a powder or fluid used as a filling for some article. The unit of production here is usually the mix, and if that is of sufficient size to make 1,000 fillings say, five fillings would be selected at random from the mix, and there would be no question of subdividing the mix into five parts of sufficient size for 200 fillings.

Deferred sentencing schemes in which several items are tested per lot have not been considered in general, but Plackett and Godwin have investigated two particular schemes of this kind (see Part II).

12. The schemes so far considered have been symmetrical, in that the same amount of production would have been accepted if the observations had occurred in the reverse order. This is even true of scheme (ii), which appears unsymmetrical at first glance. Actually, of course, we encounter the test results in one direction only, and there is no obvious efficiency in symmetry. An unsymmetrical scheme that has been used (not, I think, for any very good reason) is like that of §5 except that exactly D lots are rejected when a significant cluster occurs, either the D lots up to and including that giving the last defective in the cluster, or the D lots starting with that giving the first defective in the cluster. Another possible non-symmetrical device is the following.

(vi) *Quick release.*—A quick release clause can be inserted in the sentencing rule of §5, to the effect that when a defective occurs after a run of clear items for which the corresponding lots have been accepted, if the next A items following the defective are clear (A an integer less than D), the lots corresponding to these items and the defective one should be accepted forthwith. A can be chosen so that the chance that the $D-A-1$ items following the A clear ones shall contain $n-1$ or more defectives is very small, e.g. ·01, if the quality of production remains constant. The insertion of the quick release clause will then have almost no effect on the operating characteristic of the scheme, but may speed up the sentencing appreciably when quality is good.

13. All these deferred sentencing schemes are based on the idea of picking out clusters of a certain number of defectives which occur within a given space. This sort of criterion has the simplicity which was required in such applications of the method as have been made. But the reader may well feel that a more comprehensive and less arbitrary rule, in which the decision to accept or reject any lot was based on the closeness of defectives on either side, taking into account a large number of defectives and attaching varying weights to them according to their remoteness, would be more powerful. A first step in this direction is shown in scheme (iv) above, but this is still quite arbitrary in its choice of n_1 and n_2.

The practical problem may be crystallized as follows. After a run of uniform acceptable quality, the output deteriorates, the quality, let us say, undergoing a sudden change to a new unacceptable level, where it remains. We require a sentencing scheme under which the run of acceptable quality will almost all be accepted, and after the deterioration there will be a rejection (*a*) with the least possible delay and (*b*) covering as much as possible of the output after the deterioration. One may surmise that no scheme will be uniformly optimum for all possible levels of unacceptable quality; one scheme will be more efficient in detecting a small change in quality and another in detecting a large change. The solution of this problem has not been attempted, nor the investigation of any of the obvious criteria that suggest themselves. If the problem of detecting a change in frequency in a series of random events should occur in other connexions than routine inspection of a factory's output by non-scientific staff, it may be worth somebody's while to go into the matter further.

COMPARISON WITH SEQUENTIAL SAMPLING

14. The considerable diversity that is possible with deferred sentencing schemes has been indicated, and a certain amount of numerical information has been given, so that some of them could be put into immediate operation. In Part II methods are described for obtaining similar information about other schemes.

It remains to consider what are the peculiar virtues of "deferred sentencing". As remarked in §3, an alternative way to arrange the inspection is to divide the product into batches of a suitable size and test each individually by a sequential method. We must see whether this is ever a preferable procedure.

(The individual inspection of batches is the only alternative that I am aware of to deferred sentencing. Two sampling inspection schemes for continuous production that have appeared, the Dodge and Wald-Wolfowitz (Refs. 3 and 8), refer to non-destructive inspection and are not relevant.)

15. Suppose that the product, as it comes off the line, is divided into batches of a certain size, and a sampling inspection scheme is applied to each batch. The operating characteristic of the scheme indicates what is the chance of acceptance of a batch with any given average fraction defective. (If the sampling from the batch is random or suitably stratified the inspection will be a test of the average quality of the batch, and the batch will, of course, be sentenced as a whole, however uneven its quality may be.)

Let us suppose to begin with that the quality of the output is constant. Then corresponding to any deferred sentencing scheme we can find a sequential scheme having almost the same operating characteristic. For example, if p_0 is the fraction defective with 99 per cent. chance of acceptance, a simple deferred sentencing scheme with

$$n = 5,\ D = \frac{\cdot 97}{p_0}$$

and a closed sequential scheme with (in the notation of Ref. 1)

$$b + 1 = \frac{\cdot 322}{p_0},\ H_1 = 2(b+1),\ H_2 = 3(b+1),\ \text{closed at } 4(b+1),$$

have nearly the same operating characteristic: 99 per cent. acceptance at $p = p_0$, 90 per cent. at $p = 1{\cdot}9\,p_0$, 50 per cent. at $p = 3{\cdot}7\,p_0$, 10 per cent. at $p = 6{\cdot}3\,p_0$, 1 per cent. at $p = 9{\cdot}3\,p_0$. (The sequential scheme is as follows. Take successive samples of size $\frac{\cdot 322}{p_0}$. The batch is rejected as

soon as 4 defectives are found, and it is accepted if there are no defectives in the first two samples, or 1 only in the first three, or 2 or 3 in four samples.) The two schemes have just about the same effect, at almost any rate of testing, as long as the quality of output remains constant.

In order to compare the merits of the schemes we consider what happens when there is a change of quality. Suppose that the size of batch is so adjusted that the average rate of testing with the sequential scheme when $p = p_0$ is equal to the rate of sampling with the deferred sentencing scheme. Since the average rate of sampling in the first case is $2{\cdot}70\ (b + 1) = \frac{{\cdot}87}{p_0}$ per "batch", and the rate in the second case is 1 per "lot", the batch will be chosen to be equal to $\frac{{\cdot}87}{p_0}$ lots.

The comparison of the schemes is not very simple, since it will vary with the magnitude of the change in quality and its duration. Moreover, whereas the properties of the sequential scheme are known fully and have been tabulated (Ref. 1), the response of a deferred sentencing scheme to changes in quality has not been investigated exactly. Some rough calculations have yielded the following result. If the changes in quality are not related to the batching, i.e. if there is no tendency for batches to be of uniform quality within themselves and for changes in quality to occur at a change of batch, the two methods of inspection are about equally effective. But if the quality goes with the batches, the sequential scheme is considerably more efficient. Since this possibility was specifically excluded in the conditions of §1, we may conclude that the decision between the two types of inspection scheme will be based mainly on considerations other than economy of testing. The same conclusion would probably hold good if the simple §5 scheme were replaced by any other kind of deferred sentencing scheme.

16. There is a certain amount of difference in operation between the two types of inspection. In the deferred sentencing method, test items are selected at regular intervals from the output and can be despatched for test at once. After the test sentence may be delayed for a certain number of lots. In the sequential method, test items are only selected when a whole batch has been completed. The number of items that will be needed in the test is not certain, and in some cases it will be economical to select and despatch to the place of test the maximum number of items that are likely to be needed, to avoid possible delays in sending for more—test items not used being returned. (The same point arises with scheme (ii) above.) The test itself automatically decides the sentence on the batch, and there should be no question of referring decisions back to "headquarters".

Thus when the test is cumbersome or time-consuming, either in itself or in its preparation, the two inspection methods may involve a different delay between production and sentence. The delay, and the cost of sampling, transport, etc., will have to be assessed for each method and compared. There should be no appreciable difference in the ease of operation of the methods —one of them is not more likely to lead to confusion than the other. The manufacturer may, if he wishes, construct quality control charts from the test results of either, though he will probably feel that the regular sampling rate used with deferred sentencing makes charting a little easier.

17. But there is one consideration that, whenever deferred sentencing methods have been used, has far outweighed any of the above. In order to use a deferred sentencing scheme it is not necessary to have decided on the standards of good and bad quality before carrying out the test, and the standards may be altered at any time subsequent to the test without affecting the appropriateness of the test itself. With sequential inspection this is not so. The only way of testing individual batches that is independent of the quality standards is by "single sampling", in which a fixed number of items are tested per batch, without regard to the number of defectives occurring.* This is in general a considerably less efficient procedure than sequential sampling, and will clearly be less efficient than deferred sentencing, which in its simplest form of §5 is a kind of single sampling with an undetermined and moveable batching.

It has been found that the consumer is often not prepared to make in advance any final statement of his quality requirements, a statement that until further notice lots will irrevocably be sentenced to such and such standards. On the contrary, he will accept the output as long as he

* I am considering the ordinary known methods of sampling inspection: single sampling, double sampling, and Wald sequential inspection. It would, of course, be possible to use some kind of sequential inspection procedure which did not itself involve giving a sentence (as the Wald procedure does), but it seems unlikely that any such scheme would be used in the present instance.

considers the quality to be good, and if a deterioration occurs he will then consider whether his previous requirements should be relaxed, taking into account the available supplies, his needs, etc. The decision to accept or reject may be taken some considerable time after the change in quality has occurred, sentence being meanwhile suspended. Such an attitude of waiting until trouble develops before deciding what to do about it may be quite sound from the consumer's point of view, but blocks any attempt the statistician might make to design a really economical sequential procedure of inspection.

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PART II.—DERIVATION OF OPERATING CHARACTERISTICS

FIRST METHOD

The operating characteristic of the simple deferred sentencing scheme described in §5 above, and also that of the more elaborate scheme (ii) of §11, will now be derived, for the limit when the fraction defective $p \to 0$ and $D \to \infty$.

For the scheme of §5, the problem can be put abstractly as follows. (To begin with, we assume $p > 0$, $D < \infty$.) An array of points (of indefinite number) consists of "defectives" and "non-defectives" in random order, the probability of a defective being p. Integers n and D being given, whenever n consecutive defectives occur in a space of D points or less, all points from the first to the nth defective inclusive are "rejected". Points not rejected are accepted. We require to calculate the proportion of points accepted, on the average.

As regards the simple mathematical properties of such a sequence of defectives and non-defectives, it is easy to verify the following statements.

(i) The chance that the tth defective beyond (i.e. to one side of) any given point should occur at the rth point is

$$ {}^{r-1}C_{t-1}\, p^t q^{r-t}, $$

where $q = 1 - p$.

(ii) Given the position of the tth defective, i.e. given r, all sets of positions of the first $(t-1)$ defectives are equally likely, and have probability

$$ 1/{}^{r-1}C_{t-1}. $$

(iii) It follows, for example, that the probability that the $(t-1)$th defective is at the sth point given that the tth defective is at the rth, is

$$ \frac{{}^{s-1}C_{t-2}}{{}^{r-1}C_{t-1}} \quad (t-1 \leqslant s \leqslant r-1) $$

(iv) The chance that any given point should be a non-defective and lie in a run of precisely r consecutive non-defectives is

$$ rp^2q^r \quad (r \geqslant 1) $$

To find the required operating characteristic, we first find the chance that any point A is a non-defective and is accepted. We have to consider the positions of the $(n-1)$ defectives on either

side of A. Suppose the first defective on either side occurs at the points B and C, and let the next $(n-2)$ defectives beyond B occur at distances $r_1, r_2, \ldots r_{n-2}$ beyond B, and similarly the next $(n-2)$ defectives beyond C occur at distances $s_1, s_2, \ldots s_{n-2}$ beyond C. The notation is shown in the diagram, where a heavy dot indicates a defective.

C A B

● . . . ● . . . ● . . . . . . ● . ● . . . . ● . . . ● . . . ● . . . . ● . . ●

s_{n-2} s_{n-3} s_2 s_1 r_1 r_2 r_{n-3} r_{n-2}

distances from C distances from B

Then if the number of points from B to C inclusive is $D - a$ (a an integer, $n - 2 \leqslant a \leqslant D - 3$), the point A will be accepted if simultaneously

$$\left.\begin{array}{lll} & s_{n-2} & > a \\ r_1 & + s_{n-3} & > a \\ r_2 & + s_{n-4} & > a \\ \cdots & \cdots & \cdots \\ r_{n-3} & + s_1 & > a \\ r_{n-2} & & > a \end{array}\right\}$$

For given values of a, r_{n-2}, and s_{n-2} (both the latter being greater than a), we can find the chance that the remaining inequalities hold, by induction, using (ii) and (iii) above. We then apply (i) to find the chance that the points between B and C will be accepted, whatever the values of r_{n-2} and s_{n-2}, a being still given. Finally we apply (iv) to reach the required chance that A is a non-defective and accepted. The summation involved in this last step is very tedious.

A slight modification of the preceding working yields the chance that any point A is a defective and not rejected. Adding the two results together we have the operating characteristic of the scheme.

In practice we are usually interested in small values of p and large values of D, and the calculations become far less tedious if we set $Dp = u$ and let $D \to \infty$, $p \to 0$, at the outset. We can then reframe the problem as follows.

> A line (of indefinite extent) contains certain points, known as "defectives", distributed in a uniform probability distribution, at the rate of u defectives on the average per unit length of line. An integer n being given, whenever n consecutive defectives occur within an interval of unit length, the part of the line from the first to the nth defective is "rejected". Any portion of the line not rejected by this rule is accepted. We require to find the proportion of the whole line accepted.

Our previous statements (i) to (iv) now read:

(i) The chance that the tth defective beyond any given point O should occur at distance x_t is

$$\frac{1}{(t-1)!}(ux_t)^{t-1}e^{-ux_t}\,u\,dx_t \qquad (0 \leqslant x_t < \infty)$$

(ii) Given the position x_t of the tth defective, the joint distribution of the distances beyond O of the first $(t-1)$ defectives is

$$\frac{(t-1)!}{x_t^{t-1}}\,dx_1 \,.\, dx_2 \,.\, \ldots \; dx_{t-1} \qquad (0 \leqslant x_1 \leqslant x_2 \leqslant \ldots \leqslant x_t)$$

(iii) The distribution of x_{t-1}, given x_t, is

$$(t-1)\,\frac{x_{t-1}^{\,t-2}}{x_t^{\,t-1}}\,dx_{t-1} \qquad (0 \leqslant x_{t-1} \leqslant x_t).$$

(iv) The chance that any given point lies between two consecutive defectives spaced x apart is

$$u^2\,x\,e^{-ux}\,dx \qquad (0 \leqslant x < \infty).$$

We can derive the chance that any given point A is accepted by the method already outlined. We need only consider the case when A is a non-defective point, as the chance that it is a defective is zero. Suppose A lies between two defectives B and C distant $1 - \alpha$ apart $(0 < \alpha < 1)$. Let the distances of the next $n - 2$ defectives beyond B be $x_1, x_2, \ldots x_{n-3}, x_{n-2} = X$, and those beyond C be $y_1, y_2, \ldots y_{n-3}, y_{n-2} = Y$, say. Then for given X and $Y (> \alpha)$, it is easy to prove by induction that the chance that the following inequalities hold simultaneously

$$\left.\begin{array}{l} x_1 + y_{n-3} > \alpha \\ x_2 + y_{n-4} > \alpha \\ \cdots\cdots\cdots \\ x_{n-3} + y_1 > \alpha \end{array}\right\}$$

i.e.

$$1 - \frac{n-3}{n-2} \cdot \frac{(X + Y - \alpha)^{n-4}\,\alpha^{n-2}}{X^{n-3}\quad Y^{n-3}} \qquad (n \geqslant 4).$$

The probability that the stretch BC of the line will be accepted is the expected value of this quantity as X and Y vary, in the distribution given at (i) above, i.e. it is

$$p(\alpha, u) = \int_\alpha^\infty \int_\alpha^\infty \left[1 - \frac{n-3}{n-2} \frac{(X + Y - \alpha)^{n-4}\,\alpha^{n-2}}{(XY)^{n-3}}\right] \frac{(uX)^{n-3}\,(uY)^{n-3}}{[(n-3)!]^2} e^{-u(X+Y)}\, u^2 dX dY$$

$$= \left[\int_{\alpha u}^\infty \frac{x^{n-3}}{(n-3)!} e^{-x} dx\right]^2 - \frac{(\alpha u)^{n-2}}{(n-2)!\,(n-4)!} \int_{\alpha u}^\infty \int_{\alpha u}^\infty (x + y - \alpha u)^{n-4} e^{-(x+y)}\, dx\, dy$$

$$= e^{-2\alpha u}\left[1 + \alpha u + \frac{(\alpha u)^2}{2!} + \ldots + \frac{(\alpha u)^{n-3}}{(n-3)!}\right]^2 - \frac{(\alpha u)^{n-2} e^{-2\alpha u}}{(n-2)!\,(n-4)!} \int_0^\infty t(t + \alpha u)^{n-4} e^{-t}\, dt$$

$$= e^{-2\alpha u}\left[\left(1 + \alpha u + \frac{(\alpha u)^2}{2!} + \ldots + \frac{(\alpha u)^{n-3}}{(n-3)!}\right)^2 - \frac{(\alpha u)^{n-2}}{(n-2)!}\left((n-3) + (n-4).\alpha u \right.\right.$$

$$\left.\left. + (n-5) \cdot \frac{(\alpha u)^2}{2!} + \ldots + \frac{(\alpha u)^{n-4}}{(n-4)!}\right)\right]$$

$$= e^{-2\alpha u}\left[1 + 2(\alpha u) + 2^2 \frac{(\alpha u)^2}{2!} + \ldots + 2^{n-3} \frac{(\alpha u)^{n-3}}{(n-3)!}\right.$$

$$\left. + \sum_{t=1}^{n-3} \left\{ {}^{n-3+t}C_t + {}^{n-3+t}C_{t+1} + \ldots + {}^{n-3+t}C_{n-3} - (n-2-t).{}^{n-3+t}C_{t-1} \right\} \frac{(\alpha u)^{n-3+t}}{(n-3+t)!}\right]$$

This is for $n \geqslant 4$. For $n = 3$ we have simply

$$p(\alpha, u) = e^{-2\alpha u},$$

and for $n = 2$, $p(\alpha, u) = 0$.

If the points B and C are more than unit distance apart, clearly the stretch BC will be accepted. The chance that the point A will be accepted is therefore, from (iv) above,

$$\int_1^\infty u^2\, x\, e^{-ux}\, dx + \int_0^1 u^2\, x\, e^{-ux}\, p(1 - x, u)\, dx$$

$$= (1 + u)e^{-u} + e^{-u} \int_0^u (u - t)e^t\, p\left(\frac{t}{u}, u\right) dt \qquad . \quad . \quad . \quad . \quad (1)$$

Now

$$u\int_0^u (u - t)e^{-t} \frac{t^r}{r!} dt = \left\{-(r + 1) + u\right\}e^{-u} + \left\{(r + 1) + ru + (r - 1)\frac{u^2}{2!} + \ldots + \frac{u^r}{r!}\right\}e^{-2u}.$$

Thus the desired operating characteristic (i.e. chance that any point is accepted, or proportion of the whole line accepted) is, when $n > 4$,

$$P_n(u) = \left\{(-c_0 + 1) + (c'_1 + 1)u\right\}e^{-u} + \left\{c_0 + c_1 u + c_2 \frac{u^2}{2!} + \ldots + c_{2n-6}\frac{u^{2n-6}}{(2n-6)!}\right\}e^{-2u}. \quad . \quad . \quad . \quad (2)$$

where the coefficients $c'_1, c_0, c_1, c_2, \ldots$ are found by writing down the coefficients occurring in $p(\alpha, u)$, namely

$$1, 2, 2^2, \ldots 2^{n-3}, \text{ and } (n-3) \text{ further quantities } (1 \leq t \leq n-3)$$

$${}^{n-3+t}C_t + {}^{n-3+t}C_{t+1} + \ldots + {}^{n-3+t}C_{n-3} - (n-2-t)\,{}^{n-3+t}C_{t-1}, \quad . \quad . \quad (2')$$

and adding them to get c'_1, multiplying them by $(1, 2, 3, \ldots 2n-5)$ and adding to get c_0, multiplying by $(0, 1, 2, \ldots 2n-6)$ and adding to get c_1, multiplying by $(0, 0, 1, 2, \ldots 2n-7)$ for c_2, etc. This form of the answer is convenient for numerical purposes. If we express the powers of 2 in the set of coefficients (2′) as binomial expansions of the same powers of $(1+1)$, and make repeated use of Lemma 2 in Godwin's solution below we can deduce his simpler-looking expressions.

For $n = 2$ or 3, the evaluation of (1) is immediate. We can thus tabulate some operating characteristics, as follows:

$n = 2$: $(1 + u)e^{-u}$

$n = 3$: $2ue^{-u} + e^{-2u}$

$n = 4$: $(-7 + 5u)e^{-u} + (8 + 4u + \frac{1}{2}u^2)e^{-2u}$

$n = 5$: $(-42 + 14u)e^{-u} + (43 + 30u + 9u^2 + \frac{4}{3}u^3 + \frac{1}{12}u^4)e^{-2u}$

$n = 6$: $(-198 + 42u)e^{-u} + (199 + 158u + 59u^2 + \frac{40}{3}u^3 + \frac{23}{12}u^4 + \frac{1}{6}u^5 + \frac{1}{144}u^6)e^{-2u}$

$n = 7$: $(-858 + 132u)e^{-u} + (859 + 728u + 299u^2$

$$+ \frac{235}{3}u^3 + \frac{173}{12}u^4 + \frac{23}{12}u^5 + \frac{13}{72}u^6 + \frac{1}{90}u^7 + \frac{1}{2880}u^8)e^{-2u}.$$

The operating characteristic of scheme (iii) of §11 may be found similarly, but now the chance that a point A is accepted, given the positions B and C of the defectives next on either side, no longer depends simply on the distance BC, but on both distances AB, AC separately; and the step corresponding to equation (1) involves a double integral.

To turn now to the scheme (ii) of §11, we can give an abstract formulation for the limiting case $p \to 0$, as follows. We have set $Dp = u$, $\frac{C}{D} = c$.

> A line (the "testing" line) contains points known as defectives, distributed in a uniform probability distribution of average rate one defective per unit length of line. To each point on the line corresponds a point on a second line, the "production" line. In the absence of defectives, an interval on the testing line corresponds to an equal interval on the production line, but when a defective occurs in the testing line the rate of correspondence is changed to $1 : \frac{1}{k}$, for intervals following (i.e. to the right-hand side of) the defective. After an interval in the testing line of length c clear of defectives, the rate of correspondence returns to $1 : 1$, until the next defective is encountered. Whenever n defectives occur within a unit interval, the part of the production line corresponding to the stretch from the first to the nth defective in the testing line is rejected. It is required to know what proportion of the whole production line is rejected.

Consider any point A on the testing line. If it lies between two consecutive defectives spaced x apart, the corresponding interval on the production line is of length

$$\frac{x}{k} \quad \text{if } 0 \leqslant x \leqslant c,$$

$$\frac{c}{k} + (x - c) \text{ if } x \geqslant c.$$

The average rate of correspondence between the production and testing lines (ratio of corresponding intervals) is therefore, from (iv),

$$\left\{\int_0^c \frac{1}{k} + \int_c^\infty \left[1 - \frac{c}{x}\left(1 - \frac{1}{k}\right)\right]\right\} u^2 x e^{-ux} dx$$

$$= \frac{1}{k} + \left(1 - \frac{1}{k}\right) e^{-uc}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

while the average acceptance rate (expected acceptance on the production line per unit length of testing line) is, from (1), if $c \leqslant 1$,

$$\left\{\int_0^c \frac{1}{k} + \int_c^1 \left[1 - \frac{c}{x}\left(1 - \frac{1}{k}\right)\right]\right\} u^2 x e^{-ux} p(1 - x, u) dx + \int_1^\infty \left[1 - \frac{c}{x}\left(1 - \frac{1}{k}\right)\right] u^2 x e^{-ux} dx.$$

This is easily reduced to the form

$$\frac{1}{k} P_n(u) + \left(1 - \frac{1}{k}\right) e^{-uc} P_n\big(u(1 - c)\big), \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where $P_n(u)$ is as defined in equation (2). Dividing (4) by (3), we have the operating characteristic of the scheme. If $c > 1$, the factor $P_n(u(1 - c))$ in (4) is replaced by unity. The average rate of sampling, as a multiple of the minimum rate for no defectives ($u = 0$), is the reciprocal of (3).

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SECOND METHOD

An alternative method of finding the operating characteristic of the scheme of §5 will now be described.

To determine the probability of acceptance of any lot we need only consider the $(2D - 1)$ lots of which it is the middle one and, in detail, only the positions of the $(n - 1)$ defects nearest to it on either side. The probability of acceptance with k of these lots defective is $p^k(1 - p)^{2D-1-k}$ multiplied by the number of arrangements of the defectives which permit of acceptance. This number is

$${}^{2D-1}C_k \qquad \text{for } 0 \leqslant k \leqslant n - 1. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$2 \sum_{r=0}^{k-n} \sum_{m=-1}^{r-1} \frac{n-r}{n} \, {}^nC_{m+1} \, {}^{r-1}C_m \, {}^{D+m}C_k$$

$$+ \sum_{r} \sum_{m=0}^{r-1} \left({}^{k-r+1}C_{m+1} \, {}^{r-1}C_m - {}^{k-n+1}C_{m+1} \, {}^{n-1}C_m\right) {}^{D+m}C_k$$

$$+ \sum_{m=-1}^{r-2} \left({}^{k-r}C_{m+1} \, {}^{r-1}C_{m+1} - {}^{k-n}C_{m+1} \, {}^{n-1}C_{m+1}\right) {}^{D+m}C_{k-1} \text{ for } n \leqslant k \leqslant 2n - 3, \quad . \quad (2)$$

where for odd k the second term is summed twice for $k - n + 1 \leqslant r \leqslant \frac{k-1}{2}$, and in the third term we have $r = \frac{k+1}{2}$ while for even k, the second term is summed for $k - n + 1 \leqslant r \leqslant \frac{k}{2} - 1$

and again for $k - n + 1 \leqslant r \leqslant \frac{k}{2}$, and in the third term we have $r = \frac{k}{2}$. Finally

$$\sum_{m=-1}^{n-2} \left\{ 2\,{}^{n}C_{m+1}\,{}^{n-2}C_{n-3-m} - 2\,{}^{n-1}C_m\,{}^{n-1}C_{n-2-m} + (k-2n+3)\frac{{}^{n}C_{n-m-1}\,{}^{n-2}C_m)}{n} \right\} {}^{D+m}C_k$$

$$\text{for } 2n - 3 < k \qquad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In these expressions the binomial coefficient ${}^aC_b$ is defined for all positive, negative or zero integers a, b, as the coefficient of X^b in the expansion in ascending or descending powers of X of $(1 + X)^a$.

We now sum the probability expressions over the admissible values of k. If $p \to 0$, $pD \to u$, then the limiting values of the probabilities are

$$e^{-u}\left[\frac{{}^{2n-2}C_{n-1}}{n} \cdot u - \frac{n-3}{n+1} \cdot {}^{2n-1}C_n\right] + e^{-2u} \sum_{k=0}^{2n-6} \frac{\phi(k, n)u^k}{k!},$$

where

$$\phi(k, n) = 2^k - \frac{k}{n}\,{}^{2n-2}C_{n-1} + \frac{n-3}{n+1}\,{}^{2n-1}C_n \qquad \text{for } 0 \leqslant k \leqslant n - 1,$$

and

$$\phi(k, n) = 4.{}^kC_n - 2.{}^{k+1}C_{n+1} + \sum_{r=k-n+1}^{n-1} {}^kC_r - (2n - 1 - k).{}^kC_n$$
$$- \frac{k}{n}.{}^{2n-2}C_{n-1} + \frac{n-3}{n+1}.{}^{2n-1}C_n \qquad \text{for } n \leqslant k \leqslant 2n - 6.$$

The method of obtaining these results is indicated below : we first establish four lemmas dealing with the transformation and summation of expressions involving binomial coefficients.

Lemma 1.

$$\sum_{i=b-a}^{c-d} {}^aC_{b-i}\,{}^cC_{d+i} = {}^{a+c}C_{b+d}.$$

The left-hand side is Σ (coefficient of $X^{b-i+d+i}$ in $(1 + x)^a(1 + x)^c$).

Lemma 2.

$$\sum_{i=b-a}^{c-d} {}^{a+i}C_b\,{}^{c-i}C_d = {}^{a+c+1}C_{b+d+1}.$$

The left-hand side is Σ (coefficient of $X^{a+i-b+c-i-d}$ in $(1 - x)^{-(b+1)}\,(1 - x)^{-(d+1)}$).
Putting b or d zero leads to

$$\sum_{j=e}^{f} {}^{a+j}C_b = {}^{a+f+1}C_{b+1} - {}^{a+e}C_{b+1}, \qquad \sum_{j=e}^{f} {}^{c-j}C_d = {}^{c+1-e}C_{d+1} - {}^{c-f}C_{d+1}.$$

Lemma 3.

$$\sum_{i=0}^{k} {}^kC_i\,{}^{l-m}C_{i-m}\,{}^{a+i}C_{k+l-m} = {}^aC_{k-m}\,{}^{a+m}C_l.$$

Using lemma 1 write the left-hand side as

$$\sum_i \sum_j {}^kC_i\,{}^{l-m}C_{i-m}\,{}^aC_{l+j}\,{}^iC_{k-j-m} = \sum_i \sum_j {}^{l-m}C_{i-m}\,{}^aC_{l+j}\,{}^kC_{j+m}\,{}^{j+m}C_{k-i}$$

$$= \sum_j {}^{l+j}C_{k-m}\,{}^aC_{l+j}\,{}^kC_{j+m} = \sum_j \sum_u {}^{l+j}C_{k-m}\,{}^aC_{l+j}\,{}^{k-m}C_{k-j-m-u}\,{}^mC_u$$

$$= \Sigma\,(\text{coefficient of } y^u\, y^{k-j-m-u} x^{a-l-j}\, (xy)^{l+j-k+m} \text{ in } (1 + y)^m(1 + x + y + xy)^a).$$

Lemma 4.

Consider the sum $\sum_{\rho_1=1}^{n+1}\sum_{\rho_2=1}^{n+2-\rho_1}\ldots\sum_{\rho_r=1}^{n+r-\rho_1-\ldots-\rho_{r-1}}\binom{n+r+s-\rho_1-\ldots-\rho_r}{i}$,

where $0 \leqslant s \leqslant i+1$. (Here and elsewhere below the bracket notation for binomial coefficients is used for greater legibility.)

Instead of summing over all the ρ's we make j of them zero and only sum over the remaining ones. The sum of the ${}^rC_j$ expressions so obtained by choosing the j in every possible way we denote by $f(n, r, j, s, i)$ and show that it is equal to

$$ {}^rC_j\ {}^{n+r+s}C_{i+r-j} - {}^{r+s}C_{s+j-i-1}\ {}^{n+r}C_{r-j-1}. $$

We may group the ${}^rC_j$ expressions according to the number of zeros at the beginning of the sequence $\rho_1, \rho_2, \ldots$. If there are $j-l$ of these, and we sum over ρ_{j-l+1}, then the sum of the expressions in this group is

$$ \sum_{\rho=1}^{n+j-l+1} f(n+j-l+1-\rho,\ r-j+l-1,\ l, s, i). $$

Hence

$$ f(n, r, j, s, i) = \sum_{l=0}^{j}\sum_{\rho=1}^{n+j-l+1} f(n+j-l+1-\rho,\ r-j+l-1, l, s, i). $$

Now $f(n, j, j, s, i) = {}^{n+j+s}C_i$, and the result follows for $r = j+1, j+2, \ldots$ by induction.

The various arrangements of defectives permitting of acceptance may be grouped as follows:

(*a*) $k < n$.

(*b*) $k > 2n-3$, r defects in lots $1, 2, \ldots D-1$, $r < n-1$,

(*c*) ,, ,, $r \geqslant n-1$,

(*d*) $n \leqslant k \leqslant 2n-3$,, $r < k+1-n$,

(*e*) ,, r defects in lots $1, 2, \ldots D$, $r \geqslant k+1-n$,

(*f*) ,, $(r-1)$ defects in lots $1, 2, \ldots (D-1)$, one defect in lot D, $r \geqslant k+2-n$.

Assume $2r \leqslant k$ in (*a*) to (*e*), $2r \leqslant k+1$ in (*f*).

(*a*) All arrangements are acceptable and their number is ${}^{2D-1}C_k$.

(*b*) If $r = 0$ the number of arrangements is ${}^{D-1}C_k$.

If $r \neq 0$, let the r defects in the first $(D-1)$ lots be in lots $a_1, a_2, \ldots a_r$. Let there be ρ_i defects in lots $a_{i-1}+D$ to a_i+D-1 inclusive. Then we must have $0 \leqslant \rho_1+\rho_2+\ldots+\rho_i \leqslant n-2+i-r$. Hence the number of arrangements is

$$ \sum_{a_1=1}^{D-k+n-1}\ \sum_{a_2=a_1+1}^{D-k+n}\cdots\sum_{a_r=a_{r-1}+1}^{D-k+n+r-2}\ \sum_{\rho_1=0}^{n-1-r}\cdots $$

$$ \sum_{\rho_r=0}^{n-2-\rho_1-\ldots-\rho_{r-1}}\binom{a_1-1}{\rho_1}\binom{a_2-a_1}{\rho_2}\cdots\binom{D-a_r}{k-r-\rho_1-\ldots-\rho_r} $$

The limits of summation for the ρ's follow from the conditions above: those for the a's are the widest compatible with acceptance of the configuration. We first sum over the a's, using lemma 2. In order to avoid getting two terms at each summation we require all the ρ's except ρ_1 to have

least value 1. Hence we treat separately the zero values of the ρ's by means of lemma 4. If j ρ's are zero we have

$$ {}^{D+r-j-1}C_k \sum_{\rho_1=0}^{n-1-r} f(n-r-1-\rho_1,\ r-1, j, 0, 0) $$

$$ = {}^{D+r-j-1}C_k \frac{n-r}{n}\ {}^{n}C_{r-j}\ {}^{r-1}C_j $$

again using lemma 2. We now sum this over j from 0 to $(r-1)$.

(c) Since only the arrangement of the $(n-1)$ defects nearest to the middle lot on either side affects the acceptability of the configuration, we have for $r = n-1+l$ an extra term ${}^{a_1-1}C_l$ in the summation. Also we replace r by $n-1+l$ and have as limits of summation $\rho_1 = 0$, $0 \leqslant \rho_2 + \rho_3 + \ldots + \rho_i \leqslant i-1$ $(2 \leqslant i \leqslant n-1)$, $l+1 \leqslant a_1 \leqslant D-k+n-1$, etc. The value of the sum is ${}^{D+n-j-2}C_k\, f(0, n-2, j, 0, 0) = {}^{D+n-j-2}C_k \dfrac{{}^{n}C_{j+1}\,{}^{n-2}C_j}{n}$. This is summed over j from 0 to $n-2$.

(d) When $k \leqslant 2n-3$ we need not perform more than $k-n+1$ summations, since if there are r defects in the first D lots, the positions of the $n-r-1$ after the Dth lot are immaterial and only the positions of the $k-n+1$ after that need be considered. Hence the number of summations is the lesser of r, $k-n+1$. For $r < k-n+1$ we proceed as in (b) and the same answer arises.

(e) If $r > k-n+1$ a further term ${}^{D-a_{k-n+1}}C_{r+n-k-1}$ appears, being the number of arrangements of the remaining $r-(k-n+1)$ defects before lot $(D+1)$. We transform

$$ \binom{D-a_{k-n+1}}{r-k+n-1} \binom{D-a_{k-n+1}}{k-r-\rho_1-\ldots-\rho_{k-n+1}} \quad \text{into} $$

$$ \sum_{i=0}^{r+n-k-1} \binom{r+n-k-2}{i} \binom{k+r-\rho_1-\ldots-\rho_{k-n+1}}{i} \binom{D-a_{k-n+1}+i}{n-1-\rho_1-\ldots-\rho_{k-n+1}} $$

by lemma 3. We then proceed as before, getting

$$ \sum_{i=0}^{r+n-k-1} {}^{r+n-k-1}C_i \sum_{j=0}^{k-n} {}^{D+k-n-j+i}C_k \sum_{\rho=0}^{n-r-1} f(n-r-1-\rho,\ k-n, j, 1, i) $$

$$ = \sum_{i=0}^{r+n-k-1} \sum_{j=0}^{k-n} {}^{r+n-k-1}C_i\ {}^{D+k-n-j+i}C_k $$
$$ [{}^{k-n}C_{k-n-j}\ {}^{k-r+1}C_{k-n+i-j+1} - {}^{k-n+1}C_{j-i}\ {}^{k-r}C_{k-n-j}] $$

(f) If we fix definitely that the Dth lot is defective and that there are $(r-1)$ defects before it we arrive at the sum

$$ \sum_{a_1=1}^{D+n-k-1} \cdots \sum_{a_{k-n+1}=a_{k-n}+1}^{D-1} \sum_{\rho_1=0}^{n-r-1} \cdots \sum_{\rho_{k-n+1}=0}^{k-r-1-\rho_1-\ldots-\rho_{k-n}} $$

$$ \binom{D-1-a_{k-n+1}}{r+n-k-2} \binom{a_1-1}{\rho_1} \cdots \binom{D-a_{k-n+1}}{k-r-\rho_1-\ldots-\rho_{k-n+1}} $$

which gives

$$ \sum_{i=0}^{r+n-k-1} \sum_{j=0}^{k-n} {}^{r+n-k-1}C_i\ {}^{D+k-n-1+i-j}C_{k-1} [{}^{k-n}C_j\ {}^{k-r}C_{k-n-j+i} - {}^{k-n}C_{j-i}\ {}^{k-r}C_{k-n-j}]. $$

We now group these expressions according to the value of m in ${}^{D+m}C_k$ or ${}^{D+m}C_{k-1}$, simplifying by summation in some cases. By subtracting case (f) from case (e) we obtain the number of arrangements in these groups when the Dth lot is non-defective.

We now, for given k, add all the expressions for the various values of r. Since we have assumed in the proofs (a) to (e) that $2r \leqslant k$, and in (f) that $2r \leqslant k+1$, we must use the formulae only

for this range; from the symmetry of the conditions the number of arrangements for r defectives in lots 1 to $D-1$ is the same as for $k-r$.

The results for the limiting case follow since

$$Lt \quad p^k (1-p)^{2D-1-k} \; . \; {}^{D+m}C_k = \frac{u^k}{k!} \; . \; e^{-2u} \text{ when } m = o(D).$$

and

$$Lt \quad p^k (1-p)^{2D-1-k} \; . \; {}^{D+m}C_{k-1} = 0.$$

Further summation and simplification follow on using lemma 1.

H. J. G.

TWO PARTICULAR SCHEMES

In order to show how an operating characteristic can be determined when the above theory does not apply, two particular schemes will be considered. The method of analysis of Scheme 2 is due to Godwin. The computations have been done by Godwin, myself, and the computing section of the Mathematics Division, National Physical Laboratory.

Regarding Scheme 1, the reader will note that it differs in three respects from those so far discussed in Part II :

(i) the number of batches covered by a rejectable cluster of defectives is small, and the fractions defective are not treated as though infinitesimal;

(ii) more than one item is tested per batch ; and

(iii) the rejection rule, when it operates, causes the rejection of a fixed number of batches forward from the batch corresponding to the first member of the rejectable cluster of defectives, instead of only from the first to the last member of the cluster (a possible feature of the rejection rule that was mentioned in §12 of Part I).

It would have been interesting to have tested the effect of each of these modifications separately. Unfortunately it has not been possible to carry through a suitable programme of computations. J. P. Burman has worked on schemes in which modifications (i) and (ii) but not (iii) are made, and has used a method of enumerating acceptable defect-combinations (instead of rejectable combinations). His first mode of classifying the combinations is by the number of consecutive batches, including the one under consideration, that showed no defectives in the test.

Scheme 1

Test a sample of 40 from each batch, and set out the results as in the table below.

	Batch number.																	
	101.	102.	103.	104.	105.	106.	107.	108.	109.	110.	111.	112.	113.	114.	115.	116.	117.	118
Number of defectives in the sample	0	0	1	0	0	1	0	2	0	1	1	1	1	0	0	1	0	0
Number of defectives in previous five samples, including the present one	2	1	2	1	1	2	2	3	3	4	4	5	4	4	3	3	2	1

As long as less than 5 defectives appear in every set of five samples, as shown by the number in the bottom row of the table being less than 5, accept the batch four in arrear of the last batch sampled, i.e. after sampling batch 105, batch 101 may be accepted. If the number in the bottom row at any time is 5 or more, reject all batches back to the one in which the first of the five or more defectives was found, accepting any others that may remain unaccounted for. After rejecting a group of batches, continue sampling as before, rejecting if a number in the bottom row of 5 or more again appears, and otherwise suspending judgment until five further batches have been sampled. Thus in the example given, when batch 111 has been sampled, batch 107 may be

accepted. But when batch 112 has been sampled, batches 108, 109, 110, 111 and 112 must be rejected. Numbers of 5 or more do not appear again in the bottom row, so that as batches 113, 114, 115 and 116 are sampled, judgment is suspended. When 117 has been sampled, 113 may be accepted, and so on.

To analyse this scheme, suppose the number of defectives in the ith batch is n_i.

$$r_i = n_i + n_{i-1} + n_{i-2} + n_{i-3} + n_{i-4}$$

is the number of defectives in the five batches preceding and including the ith. Then the ith batch is rejected if and only if

(1) $r_i \geqslant 5$

(2) $r_i < 5$ (*a*) $r_{i+1} \geqslant 5$; $n_i + n_{i-1} + n_{i-2} + n_{i-3} > 0$

(*b*) $r_{i+1} < 5$, $r_{i+2} \geqslant 5$; $n_i + n_{i-1} + n_{i-2} > 0$

(*c*) $r_{i+1} < 5$, $r_{i+2} < 5$, $r_{i+3} \geqslant 5$; $n_i + n_{i-1} > 0$

(*d*) $r_{i+1} < 5$, $r_{i+2} < 5$, $r_{i+3} < 5$, $r_{i+4} \geqslant 5$; $n_i > 0$

and is otherwise accepted.

We now enumerate the various possibilities specified in (2), find the probability of each, and of (1), and sum. A systematic method of doing this is to consider the group of batches which include the ith and which together account for 1, 2, 3 or 4 defectives; the possibilities for the remaining batches, considered singly, can be detailed in sections corresponding to these numbers. The layout is best seen by enumerating the cases under 2 (*b*).

Defectives

Batch number

$(i-4)$, <	$(i-3)$.	$(i-2)$ and $(i-1)$ and i.	$(i+1)$.	
3	0		3	
3	0		2	2
3	0		1	3
3	0		0	4
2	1		2	2
2	1		1	3
2	1		0	4
1	2		1	3
1	2		0	4
0	3		0	4
2	0	2	2	1
2	0	2	1	2
2	0	2	0	3
1	1	2	1	2
1	1	2	0	3
0	2	2	0	3
1	0	3	1	1
1	0	3	0	2
0	1	3	0	2
0	0	4	0	1

The table below indicates the operating characteristic of this scheme, and also that of a similar scheme in which rejection occurs when 4 or more defectives, instead of 5 or more, occur in 5 consecutive samples.

Proportion defective.	Probability of acceptance (criterion of 5 rejects).	Probability of acceptance (criterion of 4 rejects).
½%	99·00%	95·51%
1%	87·85%	72·35%
4%	01·82%	00·65%
5%	00·28%	00·09%

Scheme 2

This is illustrated in the table below.

	Batch number.														
	101.	102.	103.	104.	105.	106.	107.	108.	109.	110.	111.	112.	113.	114.	115.
Number of defectives in sample of 20	0	0	1	0	0	0	1	0	1	0	1	0	0	1	0
Number of defectives in sample of 30	—	—	1	0	0	—	0	1	1	0	0	0	0	0	0
Number of defectives in previous five samples, including the present one	0	0	2	2	2	2	3	2	4	4	5	4	3	2	2

Sample 20 from each batch until such a time as one or more defectives are found in the sample; then test 30 more of that batch and of all succeeding batches until two consecutive samples of 50 yield no defectives, after which continue inspecting samples of 20. As long as less than 4 defectives appear in every set of five samples, as shown by the number in the bottom row of figures being less than 4, accept the batch four in arrear of the last batch sampled; after sampling batch 105, batch 101 may be accepted. If the number in the bottom row at any time reaches 4 or more, reject all batches back to the one in which the first of the 4 or more defectives was found, and accept any batches that may remain unaccounted for. After rejecting a group of batches continue sampling as before, rejecting if a number in the bottom row of 4 or more again appears, and otherwise suspending judgment until five further batches have been sampled. Thus, in the example given, when batch 108 has been sampled, batch 104 may be accepted. But when batch 109 has been sampled, batches 107, 108 and 109 are rejected, while batches 105 and 106 are accepted because they remain unaccounted for. Batches 110, 111 and 112 are rejected as they are sampled, and onwards from batch 113 judgment is suspended for five batches if the number in the bottom row does not again rise to 4 or more.

The conditions under which a batch is rejected are specified as in the analysis of Scheme 1, with the alteration that the number of rejects 5 is everywhere replaced by 4. It remains as before to find the probabilities of the various cases and it is here that the difficulty arises, namely, that the sample size varies from batch to batch. The first problem is therefore to determine the probabilities of various sample size configurations. Before sampling from any batch, one of the following conditions must hold:

(*a*) We shall take 20 from the batch and only take more if there is a defective in the 20.

(*b*) We shall take 50 from the batch and, if there are no defectives in the 50, take 20 from the following batch.

(*c*) We shall take 50 from the batch, and also from the next batch.

Let the probabilities of these conditions holding before the *i*th batch is sampled be $p_{1,i}$, $p_{2,i}$, $p_{3,i}$ respectively. Let the probability of 0 defectives in a sample of 20 be h, and k the probability of 0 defectives in a sample of 50.

Note now that condition (*a*) holds when the previous sample was 20 and contained no defectives; or when the two previous samples were 50 and contained no defectives, and the one before that contained defectives.

Hence $p_{1,i} = h.p_{1,i-1} + k^2[p_{1,i-3}(1-h) + (1-p_{1,i-3})(1-k)]$ . . . . . (1)

Condition (*b*) holds if the previous sample was a 50 with no defectives, and the one before that contained defectives.

Hence $p_{2,i} = k[p_{1,i-2}(1-h) + (1-p_{1,i-2})(1-k)]$ . . . . (2)

Condition (*c*) holds if the previous sample contained defectives.

Hence $p_{3,i} = p_{1,i-1}(1-h) + (1-p_{1,i-1})(1-k)$ . . . . (3)

Consider first equation (1). In this put $p_{1,i} = u_i + m$.

Then $u_i - hu_{i-1} + k^2(h-k)u_{i-3} + m[(1-h) + k^2(h-k)] = k^2(1-k)$.

Put $m = k^2/[k^2 + (1+k)(1-h)]$, then

$$u_i - hu_{i-1} + k^2(h-k)u_{i-3} = 0.$$

But since the roots of the equation $x^3 - hx^2 + k^2(h - k) = 0$ are all less than 1 in modulus (since $1 > h > k > 0$) it follows $u_i \to 0$ as $i \to \infty$.

Hence, as $i \to \infty$, $p_{1,i} \to p_1 = k^2/[k^2 + (1 + k)(1 - h)]$

$$p_{2,i} \to p_2 = k(1 - h)/[k^2 + (1 + k)(1 - h)]$$

$$p_{3,i} \to p_3 = (1 - h)/[k^2 + (1 + k)(1 - h)]$$

All the configurations of defect numbers necessary for acceptance in the nine consecutive batches of which the given one is the middle one are now enumerated. By multiplying the probability of accepting after a given initial state by the probability of such a state, and summing, the required probability of acceptance is found.

The following results have been obtained:

Proportion defective.	Probability of acceptance.
½%	95·25%
1%	70·63%
4%	00·68%
5%	00·12%

R. L. P.

REGRESSION LINES AND THE LINEAR FUNCTIONAL RELATIONSHIP

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(Communication from the National Physical Laboratory)

Abstract

THE first half of this paper solves the following problem: if there is a linear multiple regression between n variates, under what conditions will the regression continue to be linear when the variates are influenced by error? The new regression coefficients are obtained in terms of the original coefficients.

This leads in the second half to a discussion of the use of regression lines and functional relationships in statistical methodology. In the case of normal distributions the problem of estimation is discussed at some length. Since the classical work has used least squares methods a section relating to this work is included and some criticisms offered.

1. *The Effect of Errors on Regression Lines*

1.1. A random variable ξ_{n+1}, called the dependent variable, has a linear regression on a number of other random variables $\xi_1, \xi_2, \ldots \xi_n$, the independent variables, when, for any fixed set of values of the independent variables, the dependent variable is distributed about a mean which is a linear function of the values of the independent variables. This I write as

$$\bar{\xi}_{n+1} = \sum_{i=1}^{n} \alpha_i \xi_i + \beta$$

where α_i and β are constants. By suitable choice of the origin of coordinates β can be made to vanish and I am left with

$$\bar{\xi}_{n+1} = \sum_{i=1}^{n} \alpha_i \xi_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

It is well known that the presence of errors in the measurement of ξ_{n+1}, provided they are unbiased, will not affect the determination of the regression line in so far as the estimates of the coefficients will continue to be valid; only their standard errors will be increased. However, if similar errors are made in the measurement of ξ_i $(i = 1, 2, \ldots n)$, unbiased or not, the estimates will cease to be valid and we shall not get a true picture of the regression of ξ_{n+1} on the remaining ξ_i's. The effect of errors in the dependent variable can be removed by increasing the size of the sample: the effect of errors in the independent variable can only be removed by using a different method of approach. It is the investigation of this latter effect which occupies the greater part of this paper.

1.2. I formulate the state of affairs as follows:

$\xi_1, \xi_2, \ldots \xi_{n+1}$ are $n+1$ random variables with the joint probability density function $\pi(\xi_1, \xi_2, \ldots \xi_{n+1})$ such that (1) is true.

$\Delta\xi_1, \Delta\xi_2, \ldots \Delta\xi_{n+1}$ are $n+1$ *independent* random variables with probability density functions $g_i(\Delta\xi_i)$ with zero means: furthermore they are independent of the ξ_i's.

I consider the random variables

$$x_i = \xi_i + \Delta\xi_i \;\; (i = 1, 2, \ldots n+1) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and denote their joint probability density function by $p(x_1, x_2, \ldots x_{n+1})$.

Two questions immediately arise: is the regression of x_{n+1} on the remaining x_i's linear, i.e. will

$$\bar{x}_{n+1} = \sum_{i=1}^{n} a_i x_i + b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and if this relationship holds what will be the value of the coefficients a_i and b?

1.3. Assuming that (3) does hold it is not difficult to find these coefficients. I suppose for simplicity that n is equal to 1 so that (1) becomes

$$\bar{\xi}_2 = \alpha\xi_1.$$

This may be written

$$\int\xi_2\pi(\xi_1, \xi_2)\, d\xi_1\, d\xi_2 = \alpha\xi_1\int\pi(\xi_1, \xi_2)\, d\xi_1\, d\xi_2,$$

and on multiplication by ξ_1 and integration over ξ_1 this gives

$$\mu_{11} = \alpha\mu_{20} \qquad (4)$$

where μ_{ij} are the moments about the mean of the joint distribution of ξ_1, ξ_2 (there is obviously no loss in generality in assuming the means of ξ_1, ξ_2 zero—a point I shall return to later, § 1.5). If μ'_{ij} are similarly the moments of x_1, x_2 and μ_i the moments of $\Delta\xi_2$ I have

$$\left.\begin{aligned}\mu'_{20} &= \mu_{20} + \mu_2\\ \mu'_{11} &= \mu_{11}\end{aligned}\right\} \qquad (5)$$

by the independence of ξ_i and $\Delta\xi_i$. Accordingly if $\bar{x}_2 = ax_1$, just as I obtained (4) I obtain

$$\mu'_{11} = a\mu'_{20} \qquad (6)$$

Substituting (5) in (6)

$$\mu_{11} = a(\mu_{20} + \mu_2)$$

and eliminating μ_{11} between this and (4) I have

$$\alpha\mu_{20} = a(\mu_{20} + \mu_2) \qquad (7)$$

This, then, is the connection between α and a and clearly they are only equal if $\mu_2 = 0$, that is when no errors are present. In general if $\alpha > 0$, $\alpha > a > 0$ so that the slope of the regression line tends to be decreased.

However, in deriving (7) I have assumed that the regression of x_2 on x_1 is really linear. This assumption is not trivial, in fact it is not immediately obvious that it would ever be possible for a given distribution of ξ_i's to arrange suitable distributions for the $\Delta\xi_i$'s so that the assumption held. To see what sort of conditions would have to be imposed in order that $\bar{x}_2 = ax_1$ I generalize equation (4). Just as (4) was obtained by multiplying the previous equation by ξ_1 and integrating over ξ_1; by multiplying by $\xi_1{}^r (r > 1)$ and integrating over ξ_1 I obtain

$$\int\int \xi_1{}^r\, \xi_2\, \pi(\xi_1, \xi_2)\, d\xi_1\, d\xi_2 = \alpha \int\int \xi_1{}^{r+1}\, \pi(\xi_1\, \xi_2)\, d\xi_1\, d\xi_2$$

i.e.

$$\mu_{r,\,1} = \alpha\mu_{r\,+\,1,\,0} \qquad (8)$$

This may be put in a more convenient form by the use of cumulants rather than moments. The bivariate cumulants are defined by

$$\exp\left(\sum_{ij}\kappa_{ij}\, \frac{t_1{}^i t_2{}^j}{i!\,j!}\right) = \sum_{ij}\mu_{ij}\, \frac{t_1{}^i t_2{}^j}{i!\,j!}$$

differentiating with respect to t_2 and putting $t_2 = 0$,

$$\left(\sum_i\kappa_{i1}\, \frac{t_1{}^i}{i!}\right) \exp\left(\sum_i\kappa_{i0}\, \frac{t_1{}^i}{i!}\right) = \sum_i\mu_{i1}\, \frac{t_1{}^i}{i!} \qquad (9)$$

and differentiating the original expression with respect to t_1 and again putting $t_2 = 0$,

$$\left(\sum_i\kappa_{i0}\, \frac{t_1{}^{i-1}}{\underline{|i-1}}\right) \exp\left(\sum_i\kappa_{i0}\, \frac{t_1{}^i}{i!}\right) = \sum_i\mu_{i0}\, \frac{t_1{}^{i-1}}{\underline{|i-1}} \qquad (10)$$

Now from (8) it is clear that the right-hand side of (9) is α times the right-hand side of (10), so that from the left-hand sides of (9) and (10) I have

$$\sum_i\kappa_{i1}\, \frac{t_1{}^i}{i!} = \alpha \sum_i\kappa_{i0}\, \frac{t_1{}^{i-1}}{\underline{|i-1}}$$

giving

$$\kappa_{r,\,1} = \alpha\, \kappa_{r+1,\,0} \qquad (r > 1) \qquad (11)$$

These conditions are thus necessary for the regression of ξ_2 on ξ_1 to be linear. The generalization of (5) is (see Theorem II below)

$$\left.\begin{aligned} \varkappa'_{r+1,\,0} &= \varkappa_{r+1,\,0} + \varkappa_{r+1} \\ \varkappa'_{r,\,1} &= \varkappa_{r,\,1} \end{aligned}\right\} \qquad (12)$$

in an obvious notation: (11) applied to the regression of x_2 on x_1 gives

$$\varkappa'_{r,\,1} = a\varkappa'_{r+1,\,0} \qquad (13)$$

as a necessary condition for $\bar{x}_2 = ax_1$. Combining (11) and (13) with (12)

$$\varkappa_{r,1} = a(\varkappa_{r+1,\,0} + \varkappa_{r+1}) = \alpha\varkappa_{r+1,\,0}$$

whence

$$a\varkappa_{r+1} = (\alpha - a)\varkappa_{r+1,\,0} \qquad (14)$$

(14) thus expresses a necessary condition for the regression of x_2 on x_1 to be linear whenever the regression of ξ_2 on ξ_1 is linear. Under certain conditions a reversal of the argument will show it to be sufficient. One point that might be noticed is that (14) does not involve the cumulants of $\Delta\xi_2$, so that the opening remark that unbiased errors in the dependent variable will not affect the regression is verified.

1.4. The above method of analysis has required the existence of all the cumulants of all orders, and the proof of sufficiency mentioned will require a distribution to be uniquely determined by its moments. Such requirements do not give very great generality, and it is natural to search for a more widely applicable analysis: this is provided by utilizing the ideas of characteristic functions (c.f.) and cumulative functions (cm.f.). I use the notation

$$\varphi_\xi(t_1, t_2 \ldots t_n) = \int\int \ldots \int \exp\,[i\sum_{j=1}^{n} t_j\xi_j]\,\pi(\xi_1, \xi_2, \ldots \xi_n)d\xi_1 d\xi_2 \ldots d\xi_n \qquad (15)$$

for the characteristic function of $\xi_1, \xi_2 \ldots \xi_n$, where $\pi(\xi_1, \xi_2, \ldots \xi_n)$ is the marginal distribution of $\xi_1, \xi_2, \ldots \xi_n$, i.e.

$$\pi(\xi_1, \xi_2, \ldots \xi_n) = \int \pi(\xi_1, \xi_2, \ldots \xi_n, \xi_{n+1})d\xi_{n+1}.$$

The cumulative function is then defined by

$$\exp \psi_\xi(t_1, t_2, \ldots t_n)] = \varphi_\xi(t_1, t_2, \ldots t_n).$$

ψ_x, φ_x are defined similarly for $p(x_1, x_2, \ldots x_n)$.

Using these ideas a necessary and sufficient condition for the regression of x_{n+1} to be linear whenever that of ξ_{n+1} is obtained. The proof only requires the functions to be suitably respectable so that operations of reversal of order of integration and differentiation under the sign of integration are valid. Such requirements are more easily satisfied in statistics than those needing the existence of cumulants.

1.5. One point might be mentioned before giving the proof. Just as the regression of ξ_{n+1} was written in the homogeneous form (1), so the regression of x_{n+1}, (3), can be written

$$\bar{x}_{n+1} = \sum_{i=1}^{n} a_i x_i \qquad (16)$$

without any loss of generality. For suppose

$$\int \xi_i \pi(\xi_1, \xi_2, \ldots \xi_{n+1})d\xi_i = 0 \qquad (i = 1, 2, \ldots n + 1),$$

that is the grand mean of ξ_i vanishes: this can be secured by change of coordinates. Then $\beta = 0$ and I have (1). But from (2) it follows that the grand mean of x_i will also vanish so that $b = 0$ and I have (16). Accordingly I suppose both the regressions are in the homogeneous form.

2. *Solution of the General Problem of the Continued Existence of the Regression Line*

2.1. For simplicity in notation I give the proof for $n = 2$; the reader can easily convince himself that no essentially new analysis is involved in the more general case.

Suppose then I have the state of affairs given in § 1.2 above. It immediately follows that

$$p(x_1, x_2, x_3) = \int\int\int g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)g_3(x_3 - \xi_3)\pi(\xi_1, \xi_2, \xi_3)d\xi_1 d\xi_2 d\xi_3,$$

whence

$$\begin{aligned}&\int x_3 p(x_1, x_2, x_3)dx_3\\ &= \int\int\int \xi_3 g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2, \xi_3)d\xi_1 d\xi_2 d\xi_3\\ &= \int\int \xi_3 g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2\\ &= \int\int (\alpha_1\xi_1 + \alpha_2\xi_2)g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2.\end{aligned}$$

Similarly .

$$\begin{aligned}&\int p(x_1, x_2, x_3)dx_3\\ &= \int\int g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2.\end{aligned}$$

Now if the regression of x_3 is linear

$$\frac{\int x_3 p(x_1, x_2, x_3)dx_3}{\int p(x_1, x_2, x_3)dx_3} = \bar{x}_3 = a_1x_1 + a_2x_2$$

and so I have as the condition

$$\begin{aligned}&\int\int (\alpha_1\xi_1 + \alpha_2\xi_2)g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2\\ &= \int\int (a_1x_1 + a_2x_2)g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2 \quad . \quad . \quad . \quad (17)\end{aligned}$$

To put this in a more convenient form I multiply by exp $(it_1x_1 + it_2x_2)$ and integrate with respect to x_1 and x_2. I denote by $G_j(t_j, \xi_j)$ the c.f. of x_j for a fixed ξ_j $(j = 1, 2)$ so that

$$G_j(t_j, \xi_j) = \int exp(it_jx_j)g_j(x_j - \xi_j)dx_j$$

and hence

$$\frac{\delta}{\delta t_j} G_j(t_j, \xi_j) = i\int exp(it_jx_j)x_jg_j(x_j - \xi_j)dx_j \qquad (j = 1, 2)$$

whence (17) becomes

$$\begin{aligned}&i\int\int (\alpha_1\xi_1 + \alpha_2\xi_2)G_1(t_1, \xi_1)G_2(t_2, \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2\\ &= a_1\int\int \frac{\delta}{\delta t_1} G_1(t_1, \xi_1) \,.\, G_2(t_2, \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2\\ &+ a_2\int\int G_1(t_1, \xi_1) \frac{\delta}{\delta t_2} G_2(t_2, \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2 \quad . \quad . \quad . \quad . \quad (18)\end{aligned}$$

By taking expectations of both sides of exp (it_jx_j) = exp $(it_j\xi_j + it_j\Delta\xi_j)$ which is obtained from (2), it follows that

$$G_j(t_j, \xi_j) = \exp\,(it_j\xi_j + K_j(t_j)) \quad (j = 1, 2) \quad . \quad . \quad . \quad . \quad (19)$$

and hence

$$\frac{\delta}{\delta t_j} G_j(t_j, \xi_j) = \exp\,(it_j\xi_j + K_j(t_j)).\left(i\xi_j + \frac{dK_j}{dt_j}\right)$$

where $K_j(t_j)$ is the cm.f. of $\Delta\xi_j$. Using these in (18) and dividing throughout by exp $(K_1(t_1) + K_2(t_2))$ I have

$$\begin{aligned}&i\int\int (\alpha_1\xi_1 + \alpha_2\xi_2)e^{it_1\xi_1 + it_2\xi_2}\pi(\xi_1, \xi_2)d\xi_1 d\xi_2\\ &= \left(a_1\frac{dK_1}{dt_1} + a_2\frac{dK_2}{dt_2}\right)\int\int e^{it_1\xi_1 + it_2\xi_2}\pi(\xi_1, \xi_2)d\xi_1 d\xi_2\\ &+ i\int\int (a_1\xi_1 + a_2\xi_2)\, e^{it_1\xi_1 + it_2\xi_2}\,\pi(\xi_1, \xi_2)d\xi_1 d\xi_2.\end{aligned}$$

Now recalling the definition of the c.f. of ξ_1, ξ_2, (15), this can be simply written

$$(\alpha_1 - a_1)\frac{\delta\varphi_\xi}{\delta t_1} + (\alpha_2 - a_2)\frac{\delta\varphi_\xi}{\delta t_2} = \left(a_1\frac{dK_1}{dt_1} + a_2\frac{dC_2}{dt_2}\right)\varphi_\xi,$$

or more conveniently in terms of the cm.f.

$$(\alpha_1 - a_1)\frac{\delta\psi_\xi}{\delta t_1} + (\alpha_2 - a_2)\frac{\delta\psi_\xi}{\delta t_2} = a_1\frac{dK_1}{dt_1} + a_2\frac{dK_2}{dt_2}.$$

Obviously the extension to $n + 1$ variables is

$$\sum_{i=1}^{n}(\alpha_i - a_i)\frac{\delta\psi_\xi}{\delta t_i} = \sum_{i=1}^{n} a_i\frac{dK_i}{dt_i} \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

I have thus established that (20) is the necessary condition for the regression (16) to hold whenever (1) holds. A reversal of the argument will show that it is sufficient.

I thus have the following:

THEOREM I.—*Under the conditions of* § 1.2, *the necessary and sufficient condition that the regression of x_{n+1} on the remaining x_i's be linear is given by* (20).

2.2. In order to put (20) in a more symmetrical form it is useful to prove a theorem that has been used before (12) and a particular case of which has been used in Theorem I. It is well known in the univariate case, but it is perhaps worth giving here in the general case in terms of cumulative functions. I have, recalling the definitions in § 1.4.

THEOREM II.

$$\psi_x = \psi_\xi + \sum_{i=1}^{n} K_i(t_i) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

For
$$\begin{aligned}\varphi_x &= \int\int \ldots \int \exp[i\sum_{j=1} t_j x_j] p(x_1, x_2, \ldots x_n) \prod_{j=1} dx_j \\ &= \int\int \ldots \int \exp[i\sum_{j=1}^{n} t_j x_j] \int\int \ldots \int \prod_{j=1}^{n} g_j(x_j - \xi_j) \,.\, \pi(\xi_1, \xi_2, \ldots \xi_n) \prod_{j=1}^{n} dx_j d\xi_j \\ &= \int\int \ldots \int \prod_{j=1}^{n} G_j(t_j, \xi_j)\, \pi(\xi_1, \xi_2, \ldots \xi_n) \prod_{j=1}^{n} d\xi_j, \qquad \text{so from (19)} \\ &= \exp(\sum_{j=1}^{n} K_j) \int\int \ldots \int \exp[i\sum_{j=1}^{n} t_j \xi_j]\, \pi(\xi_1, \xi_2, \ldots \xi_n) \prod_{j=1}^{n} d\xi_j \\ &= \exp(\sum_{j=1}^{n} K_j) \,.\, \varphi_\xi.\end{aligned}$$

Taking logarithms the theorem is proved. (12) is now obvious.

Eliminating $\frac{dK_i}{dt_i}$ between (20) and (21) differentiated I have

$$\sum_{i=1}^{n} \alpha_i \frac{\delta\psi_\xi}{\delta t_i} = \sum_{i=1}^{n} a_i \frac{\delta\psi_x}{\delta t_i} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

which shows that the left-hand side of the equation is an invariant. This form might have been obtained directly rather than by obtaining (20) first, but I preferred to obtain a form indicating the nature of the distribution of error, which (20) does, rather than a relation between the x_i and ξ_i distributions.

Using (21) the condition may be written

$$\sum_{i=1}^{n}(\alpha_i - a_i)\frac{\delta\psi_x}{\delta t_i} = \sum_{i=1}^{n} \alpha_i \frac{dK_i}{dt_i} \,. \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

2.3. The most obvious feature of (20) is that it is a relation between the independent variables only. Accordingly ψ_ξ is quite arbitrary apart from having to satisfy (20), for, it having been chosen, ξ_{n+1} can then be selected as any variable distributed anyhow provided only that (1) holds. Thus in discussing solutions of (20) we need not take account of the regression restriction.

Second, if it was known that the regression of x_{n+1} was linear, then it follows that it would be a necessary condition for the linearity of regression of ξ_{n+1} that the relationship between the cumulative functions should hold. Under fairly general conditions this can be shown to be sufficient. The mean value of x_3 (I take $n = 2$, again) is as before

$$\bar{x}_3 = a_1x_1 + a_2x_2 = \frac{\iint \bar{\xi}_3 g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2}{\iint g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2} \quad . \quad . \quad . \quad . \quad (24)$$

If (20), $n = 2$, holds so does (17), and from this and (24) I have

$$\iint \left\{ \bar{\xi}_3 - (\alpha_1\xi_1 + \alpha_2\xi_2) \right\} g_1(x_1 - \xi_1)g_2(x_2 - \xi_2)\pi(\xi_1, \xi_2)d\xi_1 d\xi_2 = 0$$

for all x_1, x_2: under fairly general conditions this implies

$$\bar{\xi}_3 = \alpha_1\xi_1 + \alpha_2\xi_2$$

as required. Thus whenever one of the regressions exists and (20) holds the other regression will also exist. Furthermore this reciprocal relationship shows that an analogous theorem can be stated when one of the regressions exists and the errors are independent of x and not of ξ as here.

3. *The Case of a Single Independent Variable*

3.1. Before discussing the general case it will be helpful to consider the simple regression on a single random variable which has already been mentioned in § 1.3. (1) becomes $\xi_2 = \alpha\xi_1$ and (20) becomes

$$(\alpha - a)\frac{d\psi_\xi}{dt} = a\frac{dK}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

where ψ_ξ is the cm.f. of ξ_1, only, and K the cm.f. of $\Delta\xi_1$, only. This can be integrated immediately giving

$$(\alpha - a)\psi_\xi = aK \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

the constant of integration being zero from the general properties of cumulative functions. Thus I have

> COROLLARY I.—*In the case of a single independent variable the necessary and sufficient condition that the regression continue to be linear when the independent variable is subject to error is that the cm.f. of the independent variable is a multiple of the cm.f. of the error.*

Just as (20) was transformed to give (22) and (23), (26) may be put in either of the forms

$$\alpha\psi_\xi = a\psi_x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

$$(\alpha - a)\psi_x = \alpha K \, . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

Equation (7) may be obtained by using the relationship got from (27), provided the differentials exist,

$$\alpha\frac{d^2\psi_\xi}{dt^2} = a\frac{d^2\psi_x}{dt^2}$$

with $t = 0$, I have

$$\alpha\mu_{20} = a(\mu_{20} + \mu_2)$$

as required: (remembering $\kappa_{20} = \mu_{20}$).

Furthermore it is apparent that (26) is merely another way of writing (14), but the converse is not true since the cumulative function always exists, though the cumulants themselves may not since the function may not be expandable in a power series. An example is provided by the Cauchy distribution where $\psi(t) = -t$. Thus (26) is more general than (14).

3.2. Suppose that the distribution of ξ_1 is normal (this does not necessarily imply that $\pi(\xi_1, \xi_2)$ is a normal bivariate distribution), then

$$\psi_\xi = -\tfrac{1}{2}\sigma^2 t^2$$

where σ^2 is the variance of ξ_1. Hence

$$K = -\tfrac{1}{2}\left(\frac{\alpha}{a} - 1\right)\sigma^2 t^2$$

and is thus the cm.f. of a normal distribution so that $\Delta\xi_1$ is normal, with variance $(^\alpha/_a - 1)\sigma^2$. Thus for normal distributions only a normal distribution of error will preserve the linearity of regression. This result has been obtained by Seares[20] using a result of Eddington's.[13]

Instead of enquiring what is the value of a corresponding to a given error one might equally enquire what sort of error would have to be made to get a given value of a. In the normal case it is clear that all values of $(^\alpha/_a - 1)$ which are positive (and no others) can be obtained by suitable choice of the error variance. That is (if $\alpha > 0$), any a, $\alpha > a > 0$, can be found by taking var. $(\Delta\xi_1) = (^\alpha/_a - 1)\sigma^2$. But suppose ψ_ξ is a general distribution then $K = (^\alpha/_a - 1)\psi_\xi$ and the question immediately arises, is $(^\alpha/_a - 1)\psi_\xi$ a cm.f.? For if an error is to give $a = a_0$, then this must have cm.f. $(^\alpha/_{a_0} - 1)\psi_\xi$. Since the sum of N independent drawings from $\pi(\xi_1)$ has cm f. $N\psi_\xi$, it follows that $(^\alpha/_{a_0} - 1)\psi_\xi$ is a cm.f. if $^\alpha/_{a_0} - 1 = N$, that is $a_0 = \alpha/(N + 1)$ where N is an integer. Thus it is always possible to make errors so that the regression is linear with $a = \alpha/(N + 1)$. As far as I know the general problem of when a positive multiple (not necessarily an integer) of a cm.f. is a cm.f. has not been solved. In particular taking this multiple to be the reciprocal of an integer, N, the problem becomes, can the distribution with cm.f. ψ_ξ be regarded as the distribution of the sum of samples of N from another population with cm.f. ψ_ξ/N? The problem is thus of interest in other connections besides regression.

It is not difficult to see that the (positive) power of a c.f. (corresponding to a multiple of a cm.f.) obeys the obvious necessary conditions for a function to be a c.f. (Kendall,[18] p. 99), so no c.f.'s are excluded on this account. The problem of sufficient conditions presents difficulties which have not been resolved.

There are other distributions besides the normal which satisfy (26): for instance x_1 may be distributed as $\chi^2 - \nu_1$ (ν_1 degrees of freedom) and ξ_1 as $\chi^2 - \nu_2$ (ν_2 degrees of freedom) with $\nu_2 < \nu_1$ so that $\Delta\xi_1$ is distributed as $\chi^2 - (\nu_1 - \nu_2)$ with $(\nu_1 - \nu_2)$ degrees of freedom). (26) is then satisfied with $a = \alpha\nu_2/\nu_1$.

3.3. The speculations in the last paragraph are of theoretical rather than of practical importance. The practical requirement is to know what happens in any given case and this has been met. I shall speak of ξ_i as the true values and x_i as the observed values. Thus far I have considered the regression of ξ_2 on ξ_1, true on true, and of x_2 on x_1, observed on observed. Similarly I can consider the regression of ξ_2 on x_1, true on observed, and x_2 on ξ_1, observed on true. They exist because for fixed ξ_1 or x_1, $\bar{x}_2 = \bar{\xi}_2$ and accordingly there are only two *distinct* regression lines which I shall speak of as the true (regression on ξ_1) and the observed (regression on x_1) lines. Combining this result with (7) I have

COROLLARY II.—*The four regression lines, which coincide in pairs, are given by*

$$\left.\begin{aligned} \xi_2 &= \alpha\xi_1 = \bar{x}_2 \\ \xi_2 &= a x_1 = \bar{x}_2 \end{aligned}\right\} \qquad (29)$$

with

$$a s_1^2 = \alpha\sigma_1^2 \qquad (7')$$

(s_1^2 is the variance of x_1; σ_1^2 of ξ_1). The "bar" in the first of equations (29) means the average value for fixed ξ_1, in the second for fixed x_1: this should not lead to confusion.

As a particular case of (1) it may happen that the variance of ξ_{n+1} about $\Sigma\alpha_i\xi_i$ is zero so that ξ_{n+1} is uniquely determined by a set of values of $\xi_i (i = 1, 2, \ldots n)$ and the regression can be written

$$\xi_{n+1} = \sum_{i=1}^{n} \alpha_i\xi_i \qquad (30)$$

without the "bar." I shall speak of this as a *functional relationship;* some writers use the term structural relationship.

In the case of $n = 1$ which is being considered here this gives $\xi_2 = \alpha\xi_1$, and thus equally the regression of ξ_1 on ξ_2 (reversing the role of dependent and independent variables) exists as a functional relationship $\xi_1 = \frac{1}{\alpha}\xi_2$: this case is of considerable importance. The variables x_1 and x_2 are, of course, not functionally related, but the regressions of x_1 on x_2, x_2 on x_1 can be obtained from (7′) and its analogous equation in terms of ξ_2 and x_2.

Corollary III.—*If ξ_1, ξ_2 are functionally related: $\xi_2 = \alpha\xi_1$ then x_2, x_1 are not functionally related but the regression lines of x_2 on x_1 and x_1 on x_2 (if they exist) are given by*

$$\bar{x}_2 = ax_1,\ \bar{x}_1 = a'x_2$$

with

$$as_1^2 = \alpha\sigma_1^2,\ a's_2^2 = \frac{1}{\alpha}\sigma_2^2 = \alpha\sigma_1^2 \quad . \quad . \quad . \quad . \quad . \quad (31)$$

In connection with the functional relationship, though true in the more general case, the following result will be found useful:

Corollary IV.—*If true and observed regressions exist the expected value of ξ_1 from which a single observed x_1 arose is given by*

$$\hat{\xi}_1 = x_1\sigma_1^2/s_1^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

The distribution of ξ_1 for a given value of x_1 is

$$\frac{\pi(\xi_1)g_1(x_1 - \xi_1)}{\int\pi(\xi_1)g_1(x_1 - \xi_1)d\xi_1}$$

so the expected value of ξ_1 is

$$\hat{\xi}_1 = \frac{\int\xi_1\pi(\xi_1)g_1(x_1 - \xi_1)d\xi_1}{\int\pi(\xi_1)g_1(x_1 - \xi_1)d\xi_1} = a_1x_1/\alpha_1 \qquad \text{from (17)}$$

$$= x_1\sigma_1^2/s_1^2 \qquad \text{from (7′)}$$

This result may be compared with one due to Eddington[13] which Seares[20] uses to obtain his results. Some remarks on the meaning and use of this result will be found in Eddington's paper.

3.4. There is a result due to Allen[9] which I prove as

Corollary V.—*If*

$$\left.\begin{aligned} x_1 &= l\xi + \Delta\xi_1 \\ x_2 &= m\xi + \Delta\xi_2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

where l and m are constants: the necessary and sufficient condition that the regression of x_2 on x_1 be linear whatever may be l and m and provided the variance of ξ is finite is that ξ and $\Delta\xi_1$ should be normally distributed. (This is not exactly the result as stated in Allen's paper because that requires that all the moments of ξ and $\Delta\xi_1$ should be finite which is unnecessary.)

Before proving the corollary I need a

Lemma.—*If $\varphi(x)$, $f(x)$ are continuous, differentiable functions satisfying*

$$\varphi(xy) = f(x)\varphi(y) \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

for all x, y then

$$\varphi(y) = Ay^r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

where A, r are constants.

Differentiate (34) partially with respect to x

$$\frac{\delta\varphi(xy)}{\delta x} = \frac{df}{dx}\varphi(y)$$

or

$$\frac{\delta\varphi(xy)}{\delta y}\cdot\frac{y}{x} = \frac{df}{dx}\varphi(y)$$

so

$$\frac{\delta\varphi(xy)}{\delta y} = \frac{x}{f(x)}\cdot\frac{df}{dx}\cdot\frac{\varphi(xy)}{y} \qquad \text{using (34)}$$

$$= K(x)\frac{\varphi(xy)}{y}, \text{ say,}$$

whence on integration $\log\varphi(xy) = K(x)\log y + C(x)$

therefore $\varphi(xy) = A(x)y^{K(x)}$

putting $x = 1$, $\varphi(y) = Ay^r$ with $A = A(1)$, $r = K(1)$

giving the result required.

Turning now to the corollary I take the particular case of theorem I when there is a functional relationship between ξ_1 and ξ_2. Now Allen has assumed such a relationship ($\xi_1 = l\xi$, $\xi_2 = m\xi$, so $\xi_1 = \frac{l}{m}\xi_2$) and requires the regression to be linear whatever be l and m: clearly this is the same as requiring the regression to be linear for a change of scale in both ξ_1 and ξ_2 axes. The latter is irrelevant but the former requires that (26) be replaced by

$$(\text{cm.f. of } l\xi) = \text{constant times (cm.f. of } \Delta\xi_1)$$

for all l. If $\psi(t)$ is cm.f. of ξ, $\psi(lt)$ is the cm.f. of $l\xi$, whence it follows that on eliminating the cm.f. of $\Delta\xi_1$

$$\psi(lt) = f(l)\psi(t)$$

with $f(l)$ some function of l: so that, applying the lemma,

$$\psi(t) = At^r$$

Differentiating this twice and putting $t = 0$ gives a multiple of the variance of ξ which is not zero except for Dirac's δ-distribution, so that $r = 2$ and the distribution of ξ and hence $\Delta\xi_1$ is normal. This proves the corollary.

The essential distinction between Allen's theorem and my Theorem I is the requirement of linearity for *all* l and m. The example given in her paper satisfies the requirements of Theorem I for particular values of l and m and then the regression is linear, whereas it does not satisfy her theorem and is not linear for all l and m.

4. *The General Case of Multiple Regression*

4.1. I first obtain the relationship between the α's and the a's. Differentiate (22) with respect to t_j giving

$$\sum_{i=1}^{n}\alpha_i\frac{\delta^2\psi_\xi}{\delta t_i\delta t_j} = \sum_{i=1}^{n}a_i\frac{\delta^2\psi_x}{\delta t_i\delta t_j}$$

which on putting $t_i = 0$ $(i = 1, 2 \ldots n)$ yields

$$\sum_i\alpha_i\rho_{ij}\sigma_i\sigma_j = \sum_i a_i r_{ij}s_i s_j \qquad (j = 1, 2 \ldots n) \quad . \quad . \quad (36)$$

where σ_i^2, s_i^2 are the variances of ξ_i, x_i respectively, ρ_{ij} is the correlation between ξ_i and ξ_j and

r_{ij} is the correlation between x_i and x_j ($\rho_{ii} = r_{ii} = 1$). These equations may most conveniently be written in matrix notation: introduce the row vectors

$$\alpha = (\alpha_1, \alpha_2, \ldots \alpha_n)$$
$$a = (a_1, a_2, \ldots a_n)$$

and let Σ and S be respectively the variance-covariance matrices of the ξ's and x's. (36) is then simply

$$\alpha\Sigma = aS \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

It follows from Theorem II that

$$S = \Sigma + \Lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

where Λ is the variance-covariance matrix of the $\Delta\xi_i$'s, but since they are independent it reduces to a diagonal matrix with elements δ_i^2, the variances of $\Delta\xi_i$.

(37) may thus be written

$$\alpha(S - \Lambda) = aS$$

i.e.

$$(\alpha - a)S = \alpha\Lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

Now S is a positive definite matrix so that (39) has the unique solution

$$(\alpha - a) = \alpha\Lambda S^{-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

I postpone discussion of this solution until later (§ 4.6): the points to notice at the moment are that the solution (40) is unique and that it always exists.

4.2. I showed above (§ 3.2) that for simple regressions whatever be ψ_ξ there would exist K such that the equation of condition was satisfied at any rate for some values of a: this was done by taking $K = N\psi_\xi$ giving $a = \alpha/(N + 1)$. It is easy to see that this ceases to hold with multiple regression because ψ_ξ has to satisfy certain conditions *independent of the* K_i. For taking the condition in the form of (20) and differentiating with respect to t_k and t_l with $k \neq l$, I have

$$\sum_{i=1}^{n} (\alpha_i - a_i) \frac{\delta^3 \psi_\xi}{\delta t_i \delta t_k \delta t_l} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (41)$$

This condition is independent of the K_i and accordingly must be satisfied by any ψ_ξ before the nature of the K_i is considered. (41) yields all the equations

$$\sum_{i=1}^{n} (\alpha_i - a_i) \frac{\delta^{p+1}\psi_\xi}{\delta t_1^{p_1} \ldots \delta t_{i-1}^{p_{i-1}} \delta t_i^{p_i+1} \delta t_{i+1}^{p_{i+1}} \ldots \delta t_n^{p_n}} = 0$$

when $p = \Sigma p_i$ and at least two of the p_i are different from zero: provided, of course, the differentials exist. In terms of cumulants this may be written

$$\sum_{i=1} (\alpha_i - a_i)\varkappa(p_1, p_2 \ldots p_{i-1}, \overline{p_i + 1}, p_{i+1} \ldots p_n) = 0 \quad . \quad . \quad . \quad (42)$$

with at least two of the p_i different from zero. Now these equations, either in the form of (41) or (42), together with (40) provide at least $n + 1$ equations in the n unknowns $\alpha_i - a_i$: accordingly solutions will only exist for certain values of the matrix Λ, that is for certain values of the error variances. Now this is not the sort of situation one is interested in: what one wants to be able to say is that when ψ_ξ is given (i.e. is of a given type) then the regression will continue to be linear provided K_i is of some other type irrespective of the variances of $\Delta\xi_i$. The fact that some distributions of special variance may produce linear regression is not usually of such interest. Hence I only consider distributions for which (41) vanish identically, that is for which

$$\frac{\delta^3 \psi_\xi}{\delta t_i \delta t_k \delta t_l} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

for all i, k, l, excluding the cases $i = k = l$.

4.3. The equation of condition can be integrated in either of the forms (20) or (23) when it is an example of Lagrange's linear equation. Taking the form (20), it is integrated by forming the subsidiary equations

$$\frac{dt_1}{\alpha_1 - a_1} = \frac{dt_2}{\alpha_2 - a_2} = \ldots = \frac{dt_n}{\alpha_n - a_n} = \frac{d\psi_\xi}{\sum_i a_i \frac{dK_i}{dt_i}} \quad . \quad . \quad . \quad . \quad . \quad (44)$$

$n - 1$ independent integrals are immediately found from

$$\frac{dt_i}{\alpha_i - a_i} = \frac{dt_{i+1}}{\alpha_{i+1} - a_{i+1}} \qquad (i = 1, 2, \ldots n - 1)$$

in the form $\mu_i = (\alpha_{i+1} - a_{i+1}) t_i - (\alpha_i - a_i) t_{i+1} =$ constant.

Another integral, independent of these, is given by noting that each $dt_i/(\alpha_i - a_i)$ equals the right-hand expression in (44), so multiplying each by $a_i \frac{dK_i}{dt_i} / (\alpha_i - a_i)$ and adding

$$d\psi_\xi = \sum_i \frac{a_i}{\alpha_i - a_i} \frac{dK_i}{dt_i} dt_i$$

which gives

$$\psi_\xi = \sum_i \frac{a_i}{\alpha_i - a_i} K_i(t_i).$$

The general solution is

$$\psi_\xi = \sum_i \frac{a_i}{\alpha_i - a_i} K_i + F(\mu_1, \mu_2 \ldots \mu_{n-1}) \quad . \quad . \quad . \quad . \quad . \quad (45)$$

where F is an arbitrary function. But since I only consider solutions satisfying (43) all the solutions are given by

$$\psi_\xi = \sum_{i=1}^{n} \frac{a_i}{\alpha_i - a_i} K_i + \sum_{i,j=1}^{n-1} c_{ij} \mu_i \mu_j \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46)$$

where the c_{ij} are constants with $c_{ij} = c_{ji}$.

Then for any ψ_ξ the $n(n-1)/2$ coefficients c_{ij} can be determined from the $n(n-1)/2$ coefficients of $t_i t_j$, $(i \neq j)$ in ψ_ξ and then there are n $[n(n+1)/2 = n(n-1)/2 + n]$ equations left to determine for any δ_i^2 (the variance of $\Delta\xi_i$) the values of the a_i. The remaining cumulants are then uniquely determined.

The reader may object that it has been assumed that $a_i \neq \alpha_i$ for any i. If $a_i = \alpha_i$ for some i then I shall still assume (43) to hold for the same reason as I did in the other case. The solution then follows by continuity from (46) as the reader may verify by evaluating the c_{ij} as mentioned above and hence the a_i's when he will obtain (37) which avoids the singularities.

Thus the only solutions which are of general interest are those valid for all values of the variances of $\Delta\xi_i$; the other singular solutions may, in fact will, exist, but only suitable combinations of the errors will provide linear regression. It may happen that in the singular solutions other values of the errors do not produce much deviation from linearity and thus for practical purposes will provide solutions; this remains to be studied.

I have then the following

> THEOREM III.—*Under the conditions of* § 1.2 *the necessary and sufficient condition that the regression of* x_{n+1} *on the remaining* x_i's *be linear for all values of the error variances is given by* (46)

Corollary I to Theorem I is a particular case of this theorem.

When the a_i's have been determined the remaining cumulants of the $\Delta\xi_i$ are given immediately from (46): whether or not these will form admissible cm.f. is unknown.

4.4. The case where the ξ_i are independent is simple and may be directly compared to the simple regression of § 3. The c. f. can be expressed as a product

$$\varphi_\xi = \prod_{i=1}^{n} \varphi_i^{(\xi)}(t_i)$$

where $\varphi_i^{(\xi)}$ is the c.f. of ξ_i, and hence the cm. f. is expressible as a sum

$$\psi_\xi = \sum_{i=1}^{n} \psi_i^{(\xi)}(t_i).$$

(20) then splits up into the n ordinary differential equations

$$(\alpha_i - a_i)\frac{d\psi_i^{(\xi)}}{dt_i} = a_i\frac{dK_i}{dt_i} \qquad (i = 1, 2, \ldots n)$$

which have been met before, (25), and integrate to give

$$(\alpha_i - a_i)\,\psi_i^{(\xi)} = a_i K_i \quad . \quad . \quad . \quad . \quad . \quad . \quad (26')$$

which is exactly analogous to the state of affairs in §3. The following results can be obtained as before:

(i) $$\alpha_i\psi_i^{(\xi)} = a_i\psi_i^{(x)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27')$$

(ii) $$(\alpha_i - a_i)\,\psi_i^{(x)} = \alpha_i K_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28')$$

(iii) $$\alpha_i\sigma_i^2 = a_i s_i^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29')$$

so that if $\alpha_i > 0$, $\alpha_i > a_i > 0$.

(iv) A solution of (26′) which is a cm. f. always exists by taking $K_i = N\psi_i^{(\xi)}$.

(v) $$\bar{\xi}_i = x_i\sigma_i^2/s_i^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32')$$

(vi) Allen's theorem may be extended to the case $\xi_{n+1} = \sum_{i=1}^{n} \alpha_i\xi_i$, where it is required that the regression should be linear for all α_i. The ξ_i, and hence the $\Delta\xi_i$, must be normally distributed.

4.5. The normal distribution will obviously satisfy (43) and so is in the admissible class of distributions. ψ_ξ is then a quadratic in t_i, and from (46) it is then obvious that K_i is a quadratic and hence that $\Delta\xi_i$ must be normally distributed. Conversely if $\Delta\xi_i$ $(i = 1, 2, \ldots n)$ are normally distributed then the ξ_i must be jointly normally distributed.

A general type of distribution which satisfies (43) is given by

$$\xi_i = \zeta_i + \eta_i \qquad (i = 1, 2, \ldots n)$$

where the ζ_i have a multivariate normal distribution and the η_i are independent of the ζ_i and of each other. The distribution of error then satisfies

$$\Delta\xi_i = \Delta\zeta_i + \Delta\eta_i \qquad (i = 1, 2, \ldots n)$$

where the $\Delta\zeta_i$ are normally distributed and the $\Delta\eta_i$ have distributions whose cm. f. are multiples of the cm. f. of η_i. This follows from the additive properties of the cm. f.

4.6. The equations for the a_i have been obtained above, (37), and the method has been indicated whereby they result from the solution (46). If the ξ_i are independent the matrices Σ and S are diagonal and the solution (29′) follows immediately. The general case is more complicated: it ceases to be true (e.g.) that the slope of the "line" is decreased, it may be unaltered or increased, depending on the matrices Σ and Λ An inequality can be obtained, however, in the special case where all the $\Delta\xi_i$ have the same variance λ: Λ is then equal to λI, where I is the unit matrix of order n. (37) is then

$$\alpha\Sigma = a(\Sigma + \lambda I)$$

so

$$\alpha = a(I + \lambda\Sigma^{-1})$$

i.e.

$$\alpha\alpha' = a(I + \lambda\Sigma^{-1})^2 a'$$

or

$$\alpha\alpha' - aa' = a(2\lambda\Sigma^{-1} + \lambda^2\Sigma^{-2})a' \quad . \quad . \quad . \quad (47)$$

(a' is the transpose of a). Now as Σ is positive definite so are Σ^{-1} and Σ^{-2}, and thus the right-hand side of this equation is always positive, consequently

$$\alpha\alpha' > aa',$$

or reverting to the longer notation,

$$\Sigma\alpha_i^2 > \Sigma a_i^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

At one time I thought this relation held generally, but unequal errors can be arranged so that (48) no longer holds: for generally the right-hand side of (47) is

$$a(\Lambda\Sigma^{-1} + \Sigma^{-1}\Lambda + \Lambda\Sigma^{-2}\Lambda)a'$$

and the matrix of this is not necessarily positive definite. An example may prove instructive.

If (4) is generalized to the case of $n + 1$ variables it follows that combining this with (36) one has

$$\sum_{i=1}^{n} a_i r_{ij} s_i s_j = \rho_{j,n+1}\sigma_j\sigma_{n+1} = \sum_{i=1}^{n} \alpha_i \rho_{ij}\sigma_i\sigma_j \quad . \quad . \quad . \quad . \quad . \quad (49)$$

Take $n = 2$ and let the variance-covariance matrix of ξ_1, ξ_2, ξ_3 be

$$\begin{pmatrix} 3 & -1 & 1 \\ -1 & 2 & -1 \\ 1 & -1 & 4 \end{pmatrix}$$

so that Σ is the minor from the first two rows and columns

$$\begin{pmatrix} 3 & -1 \\ -1 & 2 \end{pmatrix}$$

Let Λ be

$$\begin{pmatrix} 1 & 0 \\ 0 & 0 \end{pmatrix}$$

so that S is

$$\begin{pmatrix} 4 & -1 \\ -1 & 2 \end{pmatrix}$$

The right-hand of the pair of equations (49) gives

$$\left.\begin{array}{r} 1 = 3\alpha_1 - \alpha_2 \\ -1 = -\alpha_1 + 2\alpha_2 \end{array}\right\}$$

thus

$$\alpha_1 = \tfrac{1}{5}, \alpha_2 = -\tfrac{2}{5};\ \alpha_1^2 + \alpha_2^2 = \tfrac{1}{5}$$

the left-hand gives

$$\left.\begin{array}{r} 1 = 4a_1 - a_2 \\ -1 = -a_1 + 2a_2 \end{array}\right\}$$

thus

$$a_1 = \tfrac{1}{7}, a_2 = -\tfrac{3}{7};\ a_1^2 + a_2^2 = \tfrac{10}{49}$$

so that

$$\alpha_1^2 + \alpha_2^2 < a_1^2 + a_2^2$$

contradicting (48).

This is rather an extreme case since $\delta_2 = 0$ so that ξ_2 is free from error, but any sufficiently small value of δ_2 will, by continuity, suffice to contradict (48).

The condition for $\alpha_i = a_i$ is easy to obtain from (39): it is that the determinant of the matrix formed from S by replacing the i^{th} column of S by the column vector $\Lambda\alpha'$ be zero. For $n = 2$ the condition for $a_1 = \alpha_1$ is

$$\begin{vmatrix} \alpha_1\delta_1^2 & r_{12}s_1s_2 \\ \alpha_2\delta_2^2 & s_2^2 \end{vmatrix} = 0$$

i.e.

$$\alpha_1\delta_1^2 s_2^2 = \alpha_2\delta_2^2 r_{12}s_1s_2$$

which is possible if α_1 and $\alpha_2 r_{12}$ have the same sign. Thus the slope may be unaltered despite the errors. It is easy to see that it is not possible for all the slopes to remain unaltered, for then α would equal a, and from (39) this would imply $\alpha = 0$, which is impossible.

4.7. Simpler expressions for the a_i in terms of the α_i may be obtained by means of the higher order cumulants. Using the notation of (42) and letting $\varkappa_i(p)$ be the p^{th} cumulant of $\Delta\xi_i$, (46) gives for $p_i > 2$

$$\varkappa(0, \ldots p_i, \ldots 0) = \frac{a_i}{\alpha_i - a_i} \varkappa_i(p_i) \quad . \quad . \quad . \quad . \quad . \quad (50)$$

If $k(p_1, p_2, \ldots p_n)$ are the cumulants of x_i $(i = 1, 2, \ldots n)$ then from Theorem II

$$\varkappa(0, \ldots, p_i, \ldots 0) + \varkappa_i(p_i) = k(0, \ldots p_i, \ldots 0)$$

so in a symmetrical form I have with $p_i > 2$

$$\alpha_i \varkappa(0, \ldots p_i, \ldots 0) = a_i k(0, \ldots p_i \ldots 0) \quad . \quad . \quad (51)$$

which also would follow from (22) using (43).

5. *The Interpretation of the Regression Lines and the Functional Relationship.*

5.1. In the preceding analysis two regression lines have been mentioned:

$$\bar{\xi}_{n+1} = \sum_{i=1}^{n} \alpha_i \xi_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$\bar{x}_{n+1} = \sum_{i=1}^{n} a_i x_i \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

The conditions under which the latter exists when the former does and the relationships between the two have been fully discussed. I now consider what interpretation is to be put upon them, what use they are in statistical methodology, and relate this to the previous work on the subject. The two lines I call respectively the true and observed regression lines but, for reasons which will appear later (§ 5.2), the most important case is where the true line is a line of functional relationship, so that

$$\xi_{n+1} = \sum_{i=1}^{n} \alpha_i \xi_i \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

As I have already said (§ 3.3), this is merely a particular case of the above analysis, and all the general results given above remain true when (30) holds instead of the more general (1). I suppose further that (30) is the only functional relationship which exists between the ξ_i. $(i = 1, 2, \ldots n + 1)$: if there were another

$$\xi_{n+1} = \sum_{i=1}^{n} \beta_i \xi_i$$

this would imply

$$\sum_{i=1}^{n} (\beta_i - \alpha_i)\xi_i = 0$$

i.e. there is a functional relationship between the $\xi_i (i = 1, 2, \ldots n)$. I suppose this not to be so: an idea of the difficulties which are present in the contrary case is to be found in a recent paper by Haavelmo[17]. This paper contains several remarks relevant to the subject matter of this section.

5.2. I consider the case of simple regression $(n = 1)$, and start from equations (29). The second of these equations says that the mean value of either the true or observed value of the dependent variable is linearly related to the *observed* value of the independent variable, x_1, and the slope of the line is the usual regression coefficient of x_2 on x_1. Here then is a use for the observed regression line: given a value of x_1 (*not* ξ_1) the most likely or expected value of x_2 *or* ξ_2 is found by multiplying x_1 by a. Similar remarks apply to the first of equations (29): the mean

value of either the true or observed value of the dependent variable is linearly related to the *true* value of the independent variable, ξ_1, and the slope of the line is the usual regression coefficient of ξ_2 on ξ_1.

Now x_1 is the measurement of a value ξ_1 and this is subject to an error which I called $\Delta\xi_1$. From the interpretations of equations (29) it is clear that the value of either x_2 or ξ_2 can be estimated from either x_1 or ξ_1 (by estimate I mean its mean value can be found): from the former by using $\bar{x}_2 = \xi_2 = ax_1$, from the latter by using $\bar{x}_2 = \xi_2 = \alpha\xi_1$. Since, however, it is only x_1 and not ξ_1 that is known, one would naturally estimate the dependent variable from the observed value x_1 despite the fact that this quantity involves an error. One, of course, would need the error to obey the conditions of Theorem I in order that both the lines exist. Thus the use of a regression line of a dependent variable on an independent variable is to determine the mean value of the dependent variable from the independent variable *even if the latter is in error.* The essential distinction between the two lines is that one refers to observed values of the independent variable, the other to true values.

It would thus appear that the true regression line, relating as it does to ξ_1, is of little use since the mean value of the dependent variable can be found by means of the observed line. When the particular case of the functional relationship is considered it is evident that a knowledge of α is of importance, because most of the laws of the empirical sciences are stated in this form: Ohm's law, for example, and the law of density which I shall consider below. It is for this reason that I suppose (1) replaced by the less general (30) and when I speak of the regression line I refer to the observed line, unless otherwise stated. It is perhaps most helpful if I consider a simple, illustrative example.

5.3. Suppose an experimenter has several pieces of metal of different shapes and sizes and he determines the mass and volume of each piece. Then it is reasonable to suppose that each observation is subject to an error, so that the observations of mass and volume may be considered the x_2 and x_1 respectively of Theorem I, and the true masses and volumes (which are ideals and can never be measured) are the ξ_2 and ξ_1. The first problem I consider is how to estimate the density of the metal. The hypothesis of the existence of density is that $\xi_2 = \alpha\xi_1$ where α is the density: that is the true mass and true volume are functionally related. Hence the question reduces to how to estimate the functional relationship.

Leaving that for a moment I now pose another problem. Suppose the experimenter does not wish, for reasons of convenience, to carry out weighings, he would much sooner measure the volumes, and from these by means of the knowledge of the existence of a functional relationship predict the masses of the pieces. Then the problem is how, from *observed* volumes, to predict true masses. Now Corollary III shows that observed quantities are not functionally related, but they may be distributed so that the regression of one on another is linear. If they are so distributed the second of equations (29) shows the answer: there is not a unique true value corresponding to a given x_1 but the mean of the true values is known to be linearly related to the observed value x_1 and the slope of this line is the regression coefficient of x_2 on x_1. Hence the question how to predict ξ_2 from x_1 is altered to how to estimate the observed regression. Here then are two problems which I call the problem of the functional relationship (estimation of density) and the problem of prediction, and it is the second of these that is answered by finding the regression line, whether or not x_1 is free from error: if it is free from error, and only then, the answers to the two are the same. They are two distinct problems and should not be confused. In a recent paper by Wald[14] there is a discussion of the use of the functional relationship for the purposes of prediction, and an example shows that even when the former is known accurately the predicted values are biased. How the prediction is to be carried out is not stated: it is hoped that the above considerations show the answer whenever the error obeys the required relationship.

5.4. It is sometimes assumed that linear regression is only valid if the independent variable, x_1, is free from serious error. For example, the following remark is quoted from Charnley[15]: "The use of the regression equation to describe a relation between two variates is legitimate only if one of the variates is free from any appreciable error." He is correct if he refers to the functional relationship, but the regression equation continues to have a meaning if both the variates are in error: it describes the relationship between the mean value of one variate and the other variate, and is thus a legitimate weapon to use (e.g. in the problem of prediction) in any case.

5.5. It should be realized that in order to estimate a regression any values of the independent variable may be chosen: there is no need for the sample to be a random selection from the whole population because, by the linearity, information got from a limited range of values of the independent variable supplies knowledge of the whole. The same is obviously not true of the dependent variable where such selection would produce a biased estimate of the mean. The situation has been discussed in some detail by Eisenhart[12], and I take the following quotation from his paper, in my notation his X, Y are ξ_1, ξ_2, his x, y are x_1, x_2:

"It does not seem to be realized that the fitting should be done in terms of the deviations which actually represent 'error.' Thus when the research worker selects the X-values in advance and holds x to these values without error and then observes the corresponding y-values, the errors are in the y-values, so that even if he is interested in using observed values of Y to estimate X he should nevertheless fit $\bar{Y} = a + bX$ and use the inverse of this relation to estimate X, i.e. $X = (\bar{Y} - a)/b$ with the best available estimate of Y substituted for $\bar{Y}$."

This method is not very satisfactory: if the X-values were a random sample from the population, then in order to estimate X from Y the Y assumes the role of independent variable which, being subject to error, vitiates the use of $\bar{Y} = a + bX$ to estimate X. But if the X-values are not so chosen then the only course to adopt is to use the fit $Y = a + bX$, but it should be borne in mind that it will not give an unbiased answer. Of course the problem is a little artificial in that it is postulating that X can be held to certain values whereas Y cannot, and yet Y is being used to estimate X. The solution of this problem rests rather with the experimenter than the statistician.

5.6. In the analysis of variance the sum of squares can be split up into two parts—that due to the regression and that which is called the residual. Obviously the regression line and not the functional relationship is the correct one to use for the value about which the regression deviations are taken, since it is desired to use the mean value of the dependent variable which is better estimated by the regression line.

5.7. The reason why the practical importance of the distinction between the two lines is slight in many cases is probably explained by the fact that the difference between a and α due to the error in x_1 is quite small. Suppose, for example, that the standard error of an observation is one-tenth the standard error of the population of true values, which is quite a considerable error. Then $\delta_1^2 = \frac{1}{100}\sigma_1^2$ so that $a = \alpha\frac{100}{101}$, a change of less than 1 per cent. due to error. Thus in the measurement of density cited above (§ 5.3) the error would certainly be smaller than in this example and the two slopes are for all practical purposes the same, at any rate in comparison with the sampling errors, though in prediction at points very far distant from the mean the effect might be noticeable. In economic data the errors are probably more material and the distinction may be more important here than in physical data.

5.8. One advantage of the functional relationship over the regression line is that the slope of the former can be entered in standard tables as a constant whereas the slope of the latter would require the ratio σ_1^2/s_1^2 from which it was determined to be tabulated as well. It would accordingly be useful if the problem of prediction could be solved using the functional relationship obtained from the tables. This may be done by using Corollary IV, which provides an estimate of the true values ξ_1 from the observed x_1 provided σ_1^2/s_1^2 is known: for combining (32) and the functional relationship

$$\bar{\xi}_2 = \alpha x_1\sigma_1^2/s_1^2;$$

this, of course, is the same as $\bar{\xi}_2 = ax_1$ from (7′), as is otherwise obvious. The usual method is merely to get α from the tables and then say $\xi_2 = \alpha x_1$, which is false: though, as I have said before, the error is slight in many cases.

5.9. Reverting to the physical example, current practice for the estimation of the density, I feel sure, would be to calculate the quantity, sum of all masses over sum of all volumes. This is a method which has been advocated by Campbell[3], and attacked from the point of view of least squares by Stewart[4]. It is probably only justifiable if the estimate is normally distributed, which is unlikely since most possibly the observed quantities will be, whence the estimate, being the ratio of two normally distributed variables, is not so distributed. The main justification for the physicist lies in its simplicity.

5.10. Little more need be added in connection with multiple regression. It is usually assumed that the quantities are normally distributed, and Section 4 has shown that both the regression lines will exist if the errors are normally distributed. Similar remarks will then apply to the distinctions between regression lines and the functional relationships as have been given for simple regressions. In the general case it may be noticed, putting it rather loosely, that in view of the restriction (43) more opportunities are present for both regression lines to exist when $n = 1$ than in the contrary case.

5.11. I summarize:

(A) The functional relationship is required for the statement of laws in the empirical sciences (e.g. that of density) which would hold if no errors existed.

(B) The regression line is required for prediction of either true or observed values of one variate from observations of the other *whether or not this latter is in error*: it being understood that the x_1 from which x_2 is to be predicted comes from the same population as those x_1's used in the estimation of the regression line.

(C) The functional relationship, when available, in tables can be used for prediction provided *true* values can be estimated as in the method above.

(D) The functional relationship and the regression line are the same if, and only if, the independent variable is not in error.

6. *Estimation of the Functional Relationship*

6.1. The general problem of estimating the functional relationship has been tackled by Geary[16]. His method is as follows: he first observes that

$$\varkappa(p_1, p_2, \ldots p_n) = k(p_1, p_2, \ldots p_n) \quad . \quad . \quad . \quad . \quad . \quad (52)$$

in the notation of § 4.7, where two or more of the p_i differ from zero; this follows from Theorem II. He next observes that if $\sum_{i=1}^{n} \alpha_i \xi_i = 0$ (a functional relationship in the homogeneous form) then

$$\Sigma\alpha_i\varkappa(p_1, p_2 \ldots p_{i-1}, p_i + 1, p_{i+1} \ldots p_n) = 0 \quad . \quad . \quad . \quad . \quad (53)$$

This may be proved by an extension to n variables of the proof of (11). Combining (52) and (53) he has

$$\Sigma\alpha_i k(p_1, p_2 \ldots p_{i-1}, p_i + 1, p_{i+1} \ldots p_n) = 0 \quad . \quad . \quad . \quad . \quad (54)$$

where two or more of the p_i's differ from zero. The k's can be estimated from the data and thus from a suitably chosen selection of equations (54) the α_i may be estimated.

The similarities between equations (53) and (42) are obvious: remembering the slight difference in notation in the way the functional relationship has been written down it is easy to see that if the regression of x_j on the remaining x's is linear then (43) implies

$$\varkappa(p_1 \ldots p_{j-1}, 0, p_{j+1}, \ldots p_n) = 0$$

or

$$k(p_1 \ldots p_{j-1}, 0, p_{j+1}, \ldots p_n) = 0$$

with at least two of the p_i different from zero, so that some of equations (54) fail.

Thus the utility of the method diminishes when the requirements of linearity of regression are satisfied. The only case I shall consider is that of normally distributed errors and true values which is not covered by Geary's treatment in so far as all the α_i are concerned: he does give a method of obtaining a sub-set of them. In the first place I confine my attention to linear regression.

6.2. The remark 5.11 (D) suggests that if the usual method of estimating the regression line when the independent variate is free from error could be extended to cover the general case one would obtain the estimates required. This I proceed to do. Wilks[19] (p. 157) introduces regression theory in the following manner: suppose for any value of x, x_k say, y is normally distributed about $ax_k + b$ with variance σ^2 (independent of x_k) so that x is a fixed variate and y a random

variate, the distribution of y about $ax + b$ being regarded as the error; he then requires to estimate a, b and σ^2 which he does by the method of maximum likelihood giving

$$\left.\begin{aligned}\hat{a} &= \sum_k (x_k - \bar{x})(y_k - \bar{y})/\sum_k (x_k - \bar{x})^2\\ \bar{y} &= \hat{a}\bar{x} + \hat{b}\\ \hat{\sigma}^2 &= \frac{1}{n}\sum_k (y_k - \hat{a}x_k - \hat{b})^2\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (55)$$

where the sample is $(x_1, x_2, \ldots x_n;\ y_1, y_2, \ldots y_n)$ and the circumflex is used to denote maximum likelihood estimates. His method depends essentially on the fact that the x_k are free from error: it can be easily extended to the general case for estimating the functional relationship. It should be noted that the suffix refers to the number in the sample, not, as before, to the variable: ξ_1 is replaced by ξ, ξ_2 by η.

6.3. My assumptions are:

(*a*) The observations consist of n pairs of values

$$(x_1, y_1), (x_2, y_2), \ldots (x_n, y_n).$$

(*b*) x_k is normally distributed about ξ_k with variance δ_x^2/P_k $(k = 1, 2, \ldots n)$. (This includes the general case where the errors of each observation differ but the relative accuracies are known precisely.)

(*c*) y_k is normally distributed about η_k with variance δ_y^2/Q_k, $(k = 1, 2, \ldots n)$, and

(*d*) $\eta_k = \alpha\xi_k + \beta$ . . . . (56)

The problem is to estimate α, β, δ_x^2, δ_y^2 and, since the likelihood is expressed in terms of them and they are unknown, ξ_k, $(k = 1, 2, \ldots n)$ (cf. Dent[7]). The likelihood function for a single pair (x_k, y_k) is

$$\frac{\sqrt{P_k}}{\sqrt{2\pi}\delta_x} e^{-\frac{(x_k - \xi_k)^2 P_k}{2\delta_x^2}} \cdot \frac{\sqrt{Q_k}}{\sqrt{2\pi}\delta_y} e^{-\frac{(y_k - \eta_k)^2 Q_k}{2\delta_y^2}}$$

and thus the logarithm of the likelihood function for all the $2n$ observations is

$$L = \tfrac{1}{2}\Sigma \log P_k Q_k - n \log 2\pi\delta_x\delta_y - \sum \frac{(x_k - \xi_k)^2 P_k}{2\delta_x^2} - \sum \frac{(y_k - \eta_k)^2 Q_k}{2\delta_y^2} \quad . \quad . \quad (57)$$

The maximum likelihood equations are:

$$\frac{\delta L}{\delta\delta_x} = 0: \qquad n\hat{\delta}_x^2 = \Sigma(x_k - \hat{\xi}_k)^2 P_k \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

$$\frac{\delta L}{\delta\delta_y} = 0: \qquad n\hat{\delta}_y^2 = \Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})^2 Q_k \quad . \quad . \quad . \quad . \quad . \quad (59)$$

$$\frac{\delta L}{\delta\xi_k} = 0: \qquad (x_k - \hat{\xi}_k)P_k/\hat{\delta}_x^2 + (y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})\hat{\alpha}Q_k/\hat{\delta}_y^2 = 0 \quad . \quad . \quad . \quad (60)$$

$$\frac{\delta L}{\delta\alpha} = 0: \qquad \Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})\hat{\xi}_k Q_k = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (61)$$

$$\frac{\delta L}{\delta\beta} = 0: \qquad \Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})Q_k = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (62)$$

But all is not well with these equations, for substituting (60) in (58) I have

$$n\hat{\delta}_x^2 = \Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})^2\hat{\alpha}^2 Q_k^2\hat{\delta}_x^4/P_k\hat{\delta}_y^4$$

or

$$n\hat{\delta}_y^4/\hat{\alpha}^2\hat{\delta}_x^2 = \Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})^2 Q_k^2/P_k$$

Now imagine the experiment repeated indefinitely; if the results are to be consistent each term on the right-hand side of this equation should estimate $(\delta_y^2/Q_k)Q_k^2/P_k = \delta_y^2 Q_k/P_k$. Consequently I should then have between true values

$$n\delta_y^4/\alpha^2\delta_x^2 = \delta_y^2 \Sigma Q_k/P_k$$

hence

$$\delta_y^2/\delta_x^2 = \alpha^2 K \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (63)$$

where $K = \frac{1}{n}\Sigma Q_k/P_k$ is independent of α. Whilst this gives the maximum of the likelihood it does not provide a sound division of the error between x and y since (63) will not generally hold between true values. (The example is clearer in the case $P_k = Q_k$ when (63) is got directly.) The equations have been solved by Dent[7] without regard to this point: I will include a further criticism of her method later (§ 8.2).

Thus the maximum likelihood estimates are not entirely satisfactory. The fault does not seem to lie with the method but rather with the fact that efficient statistics do not seem to exist. I shall show later (§ 7.2) that if the distribution of ξ_k is normal they certainly do not exist. I am thus forced to make another assumption; the most convenient one seems to be:

(*e*) The ratio δ_y^2/δ_x^2 is known, equal to λ.

6.4. In place of (58) and (59) I get (omitting the suffix from δ_x^2 as being redundant):

$$2n\hat{\delta}^2\lambda = \lambda\Sigma(x_k - \hat{\xi}_k)^2 P_k + \Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})^2 Q_k \quad . \quad . \quad . \quad . \quad (64)$$

(60) become

$$\lambda(x_k - \hat{\xi}_k)P_k + (y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})\hat{\alpha}Q_k = 0 \quad . \quad . \quad . \quad . \quad . \quad (65)$$

and (61), (62) remain unaltered. (61), (62) and (65) are then $n + 2$ equations for $\hat{\alpha}$, $\hat{\beta}$ and $\hat{\xi}_k$. They cannot be solved explicitly except in the special case where $P_k = \mu Q_k$ but by suitable choice of λ, μ may be taken to be unity. In other cases resort must be had to an approximate method.

In the special case substitution of $\hat{\xi}_k$ from (65) in (61) and (62) gives

$$\begin{aligned}(\lambda + \hat{\alpha}^2)\Sigma Q_k(y_k - \hat{\beta})(\lambda x_k + \hat{\alpha}y_k - \hat{\alpha}\hat{\beta}) &= \Sigma\hat{\alpha}Q_k(\lambda x_k + \hat{\alpha}y_k - \hat{\alpha}\hat{\beta})^2 \\ (\lambda + \hat{\alpha}^2)\Sigma Q_k(y_k - \hat{\beta}) &= \hat{\alpha}\Sigma Q_k(\lambda x_k + \hat{\alpha}y_k - \hat{\alpha}\hat{\beta})\end{aligned} \quad . \quad . \quad (66)$$

the second of which yields

$$\bar{y} - \hat{\alpha}\bar{x} - \hat{\beta} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (67)$$

where $\bar{x}$, $\bar{y}$ are the *weighted* means. Using (67) I transfer the origin so that $\bar{x} = \bar{y} = \hat{\beta} = 0$ and then (66) yields the quadratic equation for $\hat{\alpha}$,

$$\hat{\alpha}(s_{yy} - \lambda s_{xx}) + (\lambda - \hat{\alpha}^2)s_{xy} = 0 \quad . \quad . \quad . \quad . \quad . \quad (68)$$

where

$$\left.\begin{aligned} s_{xx} &= \frac{1}{n}\Sigma Q_k(x_k - \bar{x})^2 \\ s_{xy} &= \frac{1}{n}\Sigma Q_k(x_k - \bar{x})(y_k - \bar{y}) \\ s_{yy} &= \frac{1}{n}\Sigma Q_k(y_k - \bar{y})^2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (69)$$

giving

$$\hat{\alpha} = \Theta + \sqrt{\Theta^2 + \lambda} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (70)$$

with

$$\Theta = (s_{yy} - \lambda s_{xx})/2s_{xy} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (71)$$

(67) and (70) provide the maximum likelihood values of α and β, substitution in (65) will provide the $\hat{\xi}_k$ (if needed) and in (64) will give $\hat{\delta}^2$ (see below, § 6.6). The positive sign was used in (70) because this can be shown to give a maximum to L: the other root provides what is called the line of worst fit: it is the line passing through the point $(\bar{x}, \bar{y})$ with minimum likelihood.

The estimate of α provided by (70) is the same as the estimate of the line of best fit given by many previous writers whose work I shall discuss later (§ 8.1). As a result of the above analysis it can now be seen that it is also the maximum likelihood estimate when the errors are supposed normally and independently distributed. Furthermore I notice that as $\lambda \to \infty$, $\delta^2 \to 0$, $\lambda\delta^2$ remaining finite, the results tend to the usual results of Wilks when $\delta_x^2 = 0$.

6.5. I now show $\hat{\alpha}$ is a consistent estimate of α. For I have the following equations:

$$\left.\begin{aligned} E(s_{xx}) &= s_{\xi\xi} + \delta^2(n-1)/n \\ E(s_{xy}) &= s_{\xi\eta} \\ E(s_{yy}) &= s_{\eta\eta} + \lambda\delta^2(n-1)/n \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (72)$$

where $s_{\xi\xi}$, $s_{\xi\eta}$, $s_{\eta\eta}$ are the same quantities as those defined in equations (69) with ξ, η in place of x, y. I also have

$$s_{\eta\eta} = \alpha s_{\xi\eta} = \alpha^2 s_{\xi\xi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (73)$$

in view of the functional relationship.

Since s_{xx}, s_{xy}, s_{yy} converge in probability (c. i. pr.) to the limit of their expectations provided the fourth moments of certain distributions exist, it follows that Θ, equation (71), c. i. pr. to

$$\begin{aligned} &(s_{\eta\eta} - \lambda s_{\xi\xi})/2s_{\xi\eta} \\ &= (\alpha^2 - \lambda)/2\alpha \end{aligned}$$

whence from (70), $\hat{\alpha}$ c. i. pr. to

$$\frac{\alpha^2 - \lambda}{2\alpha} + \sqrt{\left(\frac{\alpha^2 - \lambda}{2\alpha}\right)^2 + \lambda} = \alpha$$

as required. Then equally $\hat{\beta}$ is a consistent estimate of β.

From this proof of consistency it is clear that it holds whenever (72) holds and s_{xx}, s_{xy}, s_{yy} c. i. pr.: furthermore from solving (72) and (73) for α it can be shown that (70) is the only consistent estimate of α employing second moments except for minor alterations (such as using the factor n instead of $n-1$ in dividing a sum of squares). Thus the estimates of α, β have some justification even when the errors are not assumed to be normally distributed.

6.6. I now turn to the question of consistency of $\hat{\delta}^2$: to find $\hat{\delta}^2$; $\hat{\alpha}$, $\hat{\beta}$, $\hat{\xi}_k$ have to be substituted in (64). First using (65) I have (remembering $P_k = Q_k$)

$$\begin{aligned} 2n\hat{\delta}^2\lambda &= (\hat{\alpha}^2/\lambda + 1)\Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})^2 Q_k \\ &= (\hat{\alpha}^2/\lambda + 1)\Sigma(y_k - \hat{\alpha}\hat{\xi}_k - \hat{\beta})y_k Q_k \end{aligned}$$

in virtue of (61) and (62). Using (65) again

$$2n\hat{\delta}^2\lambda = (\hat{\alpha}^2/\lambda + 1)\Sigma Q_k y_k(y_k - \hat{\beta}) - \frac{\hat{\alpha}^2}{\lambda}\Sigma Q_k y_k(\lambda x_k + \hat{\alpha} y_k - \hat{\alpha}\hat{\beta})$$

$$\begin{aligned} &= \Sigma Q_k y_k(y_k - \hat{\alpha}x_k - \hat{\beta}) \\ &= \Sigma Q_k y_k(y_k - \hat{\alpha}x_k - \bar{y} + \bar{x}\hat{\alpha}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{from (67)} \end{aligned}$$

hence

$$\begin{aligned} 2\hat{\delta}^2\lambda &= (s_{yy} - \hat{\alpha}s_{xy}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (74) \\ &= \tfrac{1}{2}\{s_{yy} + \lambda s_{xx} - [(s_{yy} - \lambda s_{xx})^2 + 4\lambda s_{xy}^2]^{\frac{1}{2}}\} \end{aligned}$$

From (72) it follows that the right-hand side of (74) c. i. pr. to

$$s_{\eta\eta} + \lambda\delta^2 - \alpha s_{\xi\eta} = \lambda\delta^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{from (73)}$$

so that $\hat{\delta}^2$ c. i. pr. to $\delta^2/2$ and is thus not a consistent estimate of δ^2. It would appear that this time it is the method that is at fault. $\hat{\delta}^2$ is obtained by dividing a sum of squares by a factor

$2n$; as is well known, the method of maximum likelihood when applied to the normal distribution in order to estimate the mean and the variance gives the factor n instead of $n - 1$ for the divisor of the sum of squares in estimating the variance: here again the factor is wrong, but this time more seriously since the estimate is not now consistent. The correct factor is, as in the variance estimate, the number of degrees of freedom, not the number of observations: here this is $2n - n - 2 = n - 2$. The factor is that given by least squares.

6.7. In the general case where $P_k \neq Q_k$ the likelihood equations can be solved in any numerical case by an iterative method due to Fisher[6] or, as I shall show later (§ 8.4) may be treated as least squares equations and solved by a routine devised by Deming[11]: essentially these two methods are the same. As a first approximation for the method the usual regression coefficients could be used (Equations (55) with weights Q_k).

6.8. The extension of the above method to more than two variables involves no difficulties that are not purely algebraic. Assumption (*e*) must be replaced by an assumption to the effect that all the errors are known relatively, otherwise the method breaks down. Further the estimate of errors will c. i. pr. to δ^2/m rather than δ^2, where m is the number of variables.

6.9. The advantage of the above method is that no mention is made of the distribution of the true values: any set of values may be chosen without any attempt at randomness; compare the remarks in § 5.5. When, however, one considers the problem of the existence of the regression line of y on x this cannot be done without reference to the distribution of true values. If the errors are normally distributed then I have shown that a normal distribution of ξ is essential for linearity of regression of y on x: then η would be normally distributed and hence the regression of x on y would be linear.

The regression lines are often considered from a slightly different point of view: rather more the view I took before I mentioned functional relationship. This starts from a bivariate population such that the regression is linear and on this basis estimates are obtained of the lines: commonly this population is supposed bivariate normal. The remarkable thing is that the estimate of regression of y on x is the same as that given by the above method when x is free from error, equations (55). But the bivariate normal population may be regarded as compounded of a functional relationship and an error in the dependent variable: thus this latter method is based on the same set of assumptions as the other with the additional information that x, or ξ, they now being the same, is normally distributed. We thus have the position that this additional information is irrelevant to the estimation of the regression line. Since ξ is not subject to error this is the same line as the functional relationship, see § 5.11 (D).

Equally the bivariate population may be regarded as compounded of a functional relationship and errors in *both* the variables: I now go on to show that the estimation of the functional relationship is the same as before, so that the additional information about the distribution of ξ, not x now, is irrelevant to the estimation of the functional relationship. Now, of course, I can talk about the regression of y on x, and to make the discussion fairly general I can consider the case where no functional relationship exists but merely *two* regressions, observed and true.

7. *Estimation of Regression Lines*

7.1. I assume

(*a*) $\pi(\xi, \eta)$ is a normal population with $\bar{\xi} = \bar{\eta} = 0$.

(*b*) $\Delta\xi$, $\Delta\eta$ are normally, independently distributed so that Corollary I is satisfied.

Then $p(x, y)$ will be a normal distribution.

I first make a remark about maximum likelihood estimates in general.

Suppose the likelihood function, L, depends on n unknown parameters $\theta_1, \theta_2, \ldots \theta_n$: to estimate these the equations

$$\frac{\delta L}{\delta \theta_i} = 0 \qquad (i = 1, 2, \ldots n)$$

are solved. If now it is desired to estimate $\varphi_1, \varphi_2, \ldots \varphi_n$, functions of $\theta_1, \theta_2, \ldots \theta_n$ the equations

$$\frac{\delta L}{\delta \varphi_i} = 0 \qquad (i = 1, 2, \ldots n)$$

would have to be solved. But since

$$\frac{\delta L}{\delta \varphi_i} = \sum_k \frac{\delta L}{\delta \theta_k} \frac{\delta \theta_k}{\delta \varphi_i}$$

these two sets of equations are equivalent provided the Jacobian

$$\frac{\delta(\theta_1, \theta_2, \ldots \theta_n)}{\delta(\varphi_1, \varphi_2, \ldots \varphi_n)}$$

does not vanish. Accordingly if $\hat{\theta}_1, \hat{\theta}_2, \ldots \hat{\theta}_n$ are the maximum likelihood estimates for $\theta_1, \theta_2, \ldots \theta_n$ and $\varphi_i = \varphi_i(\theta_1, \theta_2, \ldots \theta_n)$ then the maximum likelihood solutions for $\varphi_1, \varphi_2, \ldots \varphi_n$ are

$$\hat{\varphi}_i = \varphi_i(\hat{\theta}_1, \hat{\theta}_2, \ldots \hat{\theta}_n) \qquad (i = 1, 2, \ldots n) \quad . \quad . \quad . \quad (75)$$

Hence once any functions of $\theta_1, \theta_2, \ldots \theta_n$ have been estimated the θ_i's can be estimated by solving equations of the type of (75). Similar remarks apply to least squares estimates.

7.2. Let

$$\begin{pmatrix} \sigma_x^2 & \rho_{xy}\sigma_x\sigma_y \\ \rho_{xy}\sigma_x\sigma_y & \sigma_y^2 \end{pmatrix}$$

be the variance-covariance matrix of $p(x, y)$. The maximum likelihood estimates of σ_x^2, σ_y^2, ρ_{xy} are known (Kendall[18], p. 338): they are

$$\left.\begin{aligned} \hat{\sigma}_x^2 &= \frac{1}{n}\Sigma(x_k - \bar{x})^2 \\ \hat{\sigma}_y^2 &= \frac{1}{n}\Sigma(y_k - \bar{y})^2 \\ \hat{\rho}_{xy} &= \frac{1}{n}\Sigma(x_k - \bar{x})(y_k - \bar{y})/\hat{\sigma}_x\hat{\sigma}_y \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (76)$$

if the means have also to be estimated. These may be compared with (69) with $Q_k = 1$.

For the bivariate normal population these three statistics (with the means) are sufficient statistics.

From Theorem II

$$\left.\begin{aligned} \sigma_x^2 &= \sigma_\xi^2 + \delta_x^2 \\ \sigma_y^2 &= \sigma_\eta^2 + \delta_y^2 \\ \rho_{xy} &= \rho_{\xi\eta}\sigma_\xi\sigma_\eta/\sigma_x\sigma_y \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (77)$$

and for the maximum likelihood estimates I have the same equations with the circumflex over everything, equations (77′): these latter are of the type of (75). Since there are five unknowns on the right-hand side of (77′) it follows that they cannot all be estimated from the sample, which only provides estimates of the left-hand sides. Accordingly two of the five quantities must be known before the others can be estimated (cf. § 6.3). If δ_x, δ_y are known the maximum likelihood estimates of σ_ξ^2, σ_η^2 and $\rho_{\xi\eta}$ are

$$\begin{aligned} \hat{\sigma}_\xi^2 &= \hat{\sigma}_x^2 - \delta_x^2 \\ \hat{\sigma}_\eta^2 &= \hat{\sigma}_y^2 - \delta_y^2 \\ \hat{\rho}_{\xi\eta} &= \rho_{xy}\hat{\sigma}_x\hat{\sigma}_y/\hat{\sigma}_\xi\hat{\sigma}_\eta \end{aligned}$$

with $\hat{\sigma}_x$, $\hat{\sigma}_y$, $\hat{\rho}_{xy}$ given by (76).

α is given by $\rho_{\xi\eta}\sigma_\eta/\sigma_\xi$ and is thus estimated by

$$\hat{\alpha} = \hat{\rho}_{xy}\frac{\hat{\sigma}_x\hat{\sigma}_y}{\hat{\sigma}_\xi^2} = a\frac{\hat{\sigma}_x^2}{\hat{\sigma}_\xi^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (78)$$

which might also have been obtained from $\alpha\sigma_\xi^2 = a\sigma_x^2$. In fact, (77) are merely another way of writing the equations $\alpha\Sigma = aS$ that I had before (§ 4.1).

Other examples of the use of (77) can be treated similarly. In any case two of the quantities on the right-hand side must be known.

7.3. Suppose $\eta = \alpha\xi$, (77) can be written

$$\left.\begin{aligned}\sigma_x^2 &= \sigma^2 + \delta_x^2\\ \sigma_y^2 &= \alpha^2\sigma^2 + \delta_y^2\\ \rho_{xy} &= \alpha\sigma^2/\sigma_x\sigma_y\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (79)$$

where the suffix has been dropped from σ_ξ^2, $\sigma_\eta^2 = \alpha^2\sigma_\xi^2$ and $\rho_{\xi\eta} = 1$. There are now only four unknowns on the right-hand side: assuming as I had to before that $\delta_y^2 = \lambda\delta_x^2$ (79) become for the estimates

$$\begin{aligned}\hat{\sigma}_x^2 &= \hat{\sigma}^2 + \hat{\delta}^2\\ \hat{\sigma}_y^2 &= \hat{\alpha}^2\hat{\sigma}^2 + \lambda\hat{\delta}^2\\ \hat{\rho}_{xy} &= \hat{\alpha}\hat{\sigma}^2/\hat{\sigma}_x\hat{\sigma}_y\end{aligned}$$

Solving for $\hat{\alpha}$

$$\hat{\sigma}_y^2 = \hat{\alpha}\hat{\rho}_{xy}\hat{\sigma}_x\hat{\sigma}_y + \lambda\hat{\sigma}_x^2 - \lambda\hat{\rho}_{xy}\hat{\sigma}_x\hat{\sigma}_y/\hat{\alpha}$$

giving, in the notation of (69) with $Q_k = 1$,

$$\hat{\alpha}(s_{yy} - \lambda s_{xx}) + (\lambda - \hat{\alpha}^2)s_{xy} = 0$$

which is the same as before, (68). Hence the same estimate of α is used whether the population is assumed normal or whether no assumption is made about it at all. Of course the argument here is algebraically similar to that used for the consistency proofs in Section 6: the extra point here is the fact that s_{xx}, s_{xy}, s_{yy} is a set of sufficient statistics.

As before $\hat{\alpha}$ is a consistent estimate of α: solving for $\hat{\delta}^2$

$$\lambda\hat{\delta}^2 = \hat{\sigma}_y^2 - \hat{\alpha}^2\hat{\sigma}^2 = (s_{yy} - \hat{\alpha}s_{xy})$$

which, on comparison with the expression obtained in section 6, (74) is clearly a consistent estimate of δ^2. σ^2 is estimated by $s_{xy}/\hat{\alpha}$ which did not occur before.

7.4. All this is valid if a weight Q_k is attached to every *pair* of readings (x_k, y_k), for they may be regarded as repetitions of the same pair of observations each of unit weight. Accordingly the estimates cover the same case of $P_k = Q_k$ that I was able to solve before and are as general as those. The case of general P_k, Q_k is not open to this treatment since they cannot be regarded as simple repetitions of a pair of observations, but only as the repetition of individual observations.

7.5. (79) may also be solved under other assumptions besides $\delta_y^2 = \lambda\delta_x^2$, e.g. that δ_y is known (this case has occurred in industrial practice). I then have

$$\left.\begin{aligned}\hat{\sigma}_x^2 &= \hat{\sigma}^2 + \hat{\delta}_x^2\\ \hat{\sigma}_y^2 &= \hat{\alpha}^2\hat{\sigma}^2 + \delta^2\\ \hat{\rho}_{xy} &= \hat{\alpha}\hat{\sigma}^2/\hat{\sigma}_x\hat{\sigma}_y\end{aligned}\right\}$$

giving

$$\hat{\alpha}\hat{\rho}_{xy}\hat{\sigma}_x\hat{\sigma}_y + \delta^2 = \hat{\sigma}_y^2$$

so

$$\hat{\alpha} = (s_{yy} - \delta^2)/s_{xy}$$

also

$$\hat{\delta}_x^2 = (\hat{\alpha}s_{xx} - s_{xy})/\hat{\alpha}$$

All such estimates are obviously consistent.

7.6. It has recently been shown by Wald[14] that if the distribution of ξ satisfies the condition

$$\lim_{n \to \infty} \left| \frac{1}{n} \left\{ (\xi_1 + \xi_2 + \ldots + \xi_m) - (\xi_{m+1} + \ldots + \xi_n) \right\} \right| > 0$$

for $m < n$, then it is possible to find consistent estimates of α, β, δ_x^2 and δ_y^2. The method consists

in dividing the observations into two groups, (x_1, y_1), (x_2, y_2), . . . (x_m, y_m) and (x_{m+1}, y_{m+1}), . . . (x_n, y_n): so that the condition is satisfied. Unfortunately as the inequality is strict it is often not possible to do this: some examples are given by Wald for which it is possible, but they would appear artificial. The normal distribution which I have discussed obviously does not satisfy the condition for any division.

Most of the work that has been done in the past has used the methods of least squares, and in order to show the similarity of the above results with those of least squares as well as to put the subject in its historical background, it is to these methods that I now turn.

8. *Estimation by Least Squares*

8.1. The *method* of least squares requires no assumptions about the probability distributions as it applies to any group of ordered pairs of observations though it is not easily justified except in certain cases. The literature relevant to the problem here considered dates back to 1879, when Kummell[1] treated the general problem of least squares: he solved the problem of "best fit" for a line (and for other curves) under the assumption that $P_k = \lambda Q_k$ and $\delta_x^2 = \delta_y^2$. However, the paper was in an obscure journal and passed unnoticed. K. Pearson[2] tackled the problem of the best fit of lines and planes and obtained, in some ways special cases, in some ways generalizations of Kummell's results of which he was ignorant. In all cases he minimized the sum of squares of perpendicular distances from the line. Gini[5] did some work on the subject and obtained the same estimate of the line as Pearson. The work of Dent[7] has already been referred to (§ 6.3).

8.2. The interesting discussion begins with Roos's[8] paper in which he draws attention to the fact that all the previous methods have depended on the choice of the coordinate system. He obtains the conditions that the function U of the observations to be minimized must obey in order that the fitted line

$$ax + by + c = 0$$

be invariant under homogeneous strain and translation: it must be of the form

$$U = \Sigma Q_k f(ax_k + by_k + c)$$

where f is an arbitrary function and the Q_k are weighting factors. He chooses

$$f(ax_k + by_k + c) = (ax_k + by_k + c)^2$$

and assumes the ratio

$$\text{error of } y_k/\text{error of } x_k$$

to be a constant $k = \sqrt{\lambda}$ in my notation. By this means he fits a straight line of slope

$$\frac{s_{yy} - ks_{xy}}{s_{xy} - ks_{xx}}$$

But this is only a consistent estimate of α if

$$\frac{\alpha(\alpha - \lambda)s_{\xi\xi} + k^2\sigma^2}{(\alpha - \lambda)s_{\xi\xi} - k\sigma^2} = \alpha$$

by use of (72) and (73), which is true only if $\delta^2 \to 0$, $\lambda \to \infty$ or $k = -\alpha$. Roos observed that these three cases provided the usual estimates.

There is, as Roos appreciated, a certain arbitrariness in his solution, and therefore it would seem necessary to choose some additional criterion to select the "best" estimate from all possible invariant estimates. Now Roos does not seem to have noticed that Kummell's solution (which is the same as mine) is invariant under the conditions that Roos considers provided account is taken of the fact that λ varies under a strain, as is clear from my assumptions (§ 6.3). Suppose the strain is $x = lX$, $y = mY$, then $\lambda = \Lambda m^2/l^2$ and the new estimate has

$$\Theta' = (m^2 s_{yy} - \frac{m^2}{l^2}\Lambda l^2 s_{xx})/lms_{xy} = \frac{m}{l}\Theta$$

so that the new slope is $m\hat{\alpha}/l$ as required for invariancy. Hence this solution has the advantages

of invariancy and consistency, and as I have remarked before, is the only consistent estimate employing second moments. Thus I reject Roos's solution.

It is to be noticed that more general invariant criteria are not of interest: in the density problem cited above it is the ratio mass to volume that is of interest and not some general transformation of these quantities.

This criterion of consistency is another reason for rejecting Dent's estimate of the slope, without assuming λ known, i.e.

$$\sqrt{s_{yy}/s_{xx}}.$$

This solution is just as arbitrary as that which assumes $\lambda = 1$ to which Miss Dent objects.

8.3. A recent paper by Seares[20] attempts to estimate the functional relationship and the regression lines in the general case $P_k \neq Q_k$: he claims to do it by least squares, but although the solution for α is consistent, the least squares solution is that indicated below, which differs from his. Furthermore the estimate of the regression line is wrong through a misunderstanding of the technique. What he does is to assume both δ_x^2 and δ_y^2 known, which is redundant as has been shown above (sections 6 and 7); he then obtains two solutions for α which are similar to those in § 7.5 and the corresponding situation with δ_x^2 known. The least squares solution for α is then got, according to him, by taking the weighted geometric mean of these two solutions: this is not a minimum of anything as far as I can see. However, the solution is a consistent estimate and is not too difficult to calculate.

I consider the problem afresh: first, the estimation of the regression line, $\bar{y} = ax$. It is clear from the remarks above concerning the distinction between the functional relationship and the regression line that the regression line which is commonly used makes no mention of the true values ξ_k: it is a regression of y on x and it is x, and x alone, that is of interest. A quantity cannot be said to be in error until you introduce some notion of true values; here one asks simply for the regression on some *observed* values, some perfectly real readings, and the population underlying the theory is the population of the observed values. Assuming then that the regression is linear, at any rate approximately, with unequal weights, the error of x is not relevant to the regression of y on x. The error of y, however, is relevant: for any value of x there are many values of y and there is a distribution of y for this given value of x: I wish to estimate the mean of this distribution and to do this the knowledge of the error of y is used just as it would be in estimating the mean of any population.

I can now formulate the least squares method. This consists in minimizing the weighted sum of squares of deviations: ideally $\bar{y}_k = ax_k + b$; the deviations are accordingly $(y_k - ax_k - b)$, where a is the regression coefficient of y on x. The weights are those of y_k only, so the sum of squares to be minimized is

$$\Sigma Q_k(y_k - ax_k - b)^2$$

yielding the usual solutions

$$\left.\begin{aligned} \tilde{a} &= s_{xy}/s_{xx} \\ \tilde{b} &= \bar{y} - \tilde{a}\bar{x}, \end{aligned}\right\} \qquad (80)$$

where the tilda denotes least squares estimates.

8.4. The true regression lines are not amenable to attack by least squares methods as a knowledge of the error is required beforehand, which error is always estimated from the minimum of the sum of squares. However the estimate of the "best" line or the functional relationship can be found: the argument is the same as that above for the regression line except that now x is in error because the true values have a meaning and thus the weights are no longer Q_k. In fact

$$\begin{aligned} \text{var}\,(y_k - \alpha x_k - \beta) &= \text{var}\,(y_k) + \text{var}\,(x_k)\,.\,\alpha^2 \\ &= \delta_y^2/Q_k + \alpha^2\delta_x^2/P_k. \end{aligned}$$

I have to bring in the same assumption $\delta_y^2 = \lambda\delta_x^2$, whence

$$\text{var}\,(y_k - \alpha x_k - \beta) = \delta_x^2(\lambda/Q_k + \alpha^2/P_k)$$

and the weight of $(y_k - \alpha x_k - \beta)$ is

$$P_kQ_k/(\lambda P_k + \alpha^2 Q_k)$$

so that the expression to be minimized is

$$\sum \frac{P_k Q_k}{\lambda P_k + \alpha^2 Q_k}(y_k - \alpha x_k - \beta)^2 \quad . \quad . \quad . \quad . \quad . \quad . \qquad (81)$$

(Most writers implicitly assume λ known, equal to 1, by assuming P_k, Q_k are known relatively, that is are measured on the same scale.) The main feature of interest in (81) is that the weights contain the unknown α: accordingly they must be included in the differentiation; this does not seem to have been admitted as a possibility before. Seares trips up on this point when he attempts to use such weights in the (wrong) estimation of the regression line: he merely uses weights, R_k say, obtains an answer in terms of them, and then investigates the nature of these weights, saying that the theoretical difficulty that the R_k contain the unknown α is of no consequence. That it is of consequence may be seen by comparing the results.

Again (81) can only be minimized with $P_k = Q_k$: the general result can be obtained by the iterative method mentioned before. Differentiation with respect to β immediately gives the second of equations (80), then transferring the mean so that $\tilde{\beta} = 0$, differentiation with respect to α gives

$$\Sigma Q_k(\lambda + \tilde{\alpha}^2)(y_k - \tilde{\alpha} x_k)x_k + \Sigma Q_k \tilde{\alpha}(y_k - \tilde{\alpha} x_k)^2 = 0 \quad . \quad . \quad . \qquad (82)$$

i.e.

$$\tilde{\alpha}^2 s_{xy} + \tilde{\alpha}[\lambda s_{xx} - s_{yy}] - \lambda s_{xy} = 0$$

which is the same quadratic equation as before, (68). Hence I arrive at the usual estimate of the line of best fit as given by Kummell[1] (quoted by Deming[11]). The estimate of error is provided by the minimum sum of squares divided by the degrees of freedom $n - 2$: this sum is

$$\sum \frac{Q_k}{\lambda + \tilde{\alpha}^2}\left\{y_k - \tilde{\alpha} x_k\right\}^2$$

where the origin is such that $\tilde{\beta} = 0$. (82) can be written

$$\Sigma \lambda Q_k(y_k - \tilde{\alpha} x_k)^2 = \Sigma Q_k(\lambda + \tilde{\alpha}^2)(y_k - \tilde{\alpha} x_k)x_k$$

hence the estimate of δ_x^2 is

$$\tilde{\delta}_x^2 = \frac{1}{n-2}\Sigma Q_k(y_k - \tilde{\alpha} x_k)y_k/\lambda$$

so

$$\lambda \tilde{\delta}_x^2 = \frac{n}{n-2}\left\{s_{yy} - \tilde{\alpha} s_{xy}\right\}$$

which may be compared with (74). This shows least squares technique provides the same estimates of α and β as the maximum likelihood in both the cases considered above and provides a consistent estimate of the error. There is no justification of this latter use of least squares comparable with the very elegant Markoff[10] theorem (more correctly called Gauss's theorum) of which the estimation of the regression line provides an example.

8.5. These results have been obtained by many writers (as mentioned above) by minimizing

$$\Sigma P_k(x_k - \xi_k)^2 + \Sigma Q_k(y_k - \alpha \xi_k - \beta)^2$$

with respect to α, β and ξ_k ($k = 1, 2, \ldots n$): they yield (61), (62) and (65) and hence the same estimates of α, β and ξ_k. The advantage of the above method is that the redundant ξ_k are never mentioned.

8.6. Sections 6, 7 and 8 may be summarized very briefly by saying that α should be estimated by (70), β by (67) and the error by

$$\lambda \delta_x^2 = (s_{yy} - \alpha s_{xy})$$

or with the additional factor $n/(n - 2)$ on the right-hand side. It being assumed that $P_k = Q_k$, $\delta_y^2 = \lambda \delta_x^2$ and either

(*a*) errors are normally distributed (§ 6.4)

or

(*b*) the population $\pi(\xi, \eta)$ is bivariate normal (so that a can be estimated as well) (§ 7.3),

or

(*c*) whenever least squares methods are used (§ 8.4)

a should be estimated by s_{xy}/s_{xx} and b as β with the weights of y only when either

(*a*) $\pi(\xi, \eta)$ is bivariate normal and $P_k = Q_k$ (§ 7.2),

or

(*b*) whenever least squares methods are used and whatever be the weights of x_k (justified by Gauss's theorem) (§ 8.3).

If $\pi(\xi, \eta)$ is bivariate normal and $\delta_y^2 \neq \lambda\delta_x^2$ the equations (79) can be solved under other assumptions to yield estimates of α and the error (§ 7.3), and can be used in the form (77) whenever the assumption of the existence of a functional relationship is not used.

If $P_k \neq Q_k$ in the above methods, except when $\pi(\xi, \eta)$ is assumed normal, iterative methods of solution are available.

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Grouping Corrections for High Autocorrelations

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1. *Introduction*

In applying Sheppard's corrections to bivariate second order moments it is customary to assume that the product moment requires no adjustment. While the assumption is usually true, the conditions for its validity no longer hold in cases of nearly perfect correlation, and if Sheppard's correction is then applied to the variances while leaving the covariance unaltered, the adjusted correlation coefficient is sometimes found to exceed unity. The behaviour of the grouped product moment is essentially determined by the effect of grouping on the residual errors about the regression line, and difficulty arises when the standard error of the residuals is small compared with the group intervai, even though the latter is itself small relative to the standard deviation of the individual variates.

The question of the correct adjustments to make in such cases assumes some importance in work on stationary time series when interest is centred on the high correlations corresponding to short lags. Cunningham and Hynd(1), for example, consider the survival chance of a target subjected to short bursts from a gun whose successive shots are highly autocorrelated in aim. Their experimental data are obtained under rigorous flying conditions and may have to be recorded in rather crude group intervals, making if necessary to adjust the resulting correlograms for the effect of grouping before using them to calculate target survival chances. A similar situation occurs when the autocorrelations are calculated on a relay computer of limited capacity, the original data having to be grouped to lie within the scope of the machine.

It is to be expected that the product moment grouping correction will be sensitive to the form of the distribution of residuals when there is high correlation. We consider in the present note the correction to be made to high autocorrelations when these residuals (or more appropriately, the differences between variate values separated by the lag intervals) are normally distributed, and the effect of a moderate degree of non-normality is discussed. The results should be applicable to long stationary time series, where the distribution of the differences is often found to be nearly normal. Short sample series whose length is of the order of a few "periods" are not likely to satisfy the required condition.

2. *The General Bivariate Case*

Let x_1, x_2 in the bivariate distribution $f(x_1, x_2)\, dx_1 dx_2$ be grouped respectively in intervals of width ω_1, ω_2, the centres of the initial intervals being located at ξ_1, ξ_2. The grouped frequency corresponding to intervals r_1, r_2 is

$$F(r_1, r_2;\ \xi_1, \xi_2) = \int_{-\frac{1}{2}\omega_1}^{\frac{1}{2}\omega_1} dx_1 \int_{-\frac{1}{2}\omega_2}^{\frac{1}{2}\omega_2} dx_2\, f(x_1 + r_1\omega_1 + \xi_1, x_2 + r_2\omega_2 + \xi_2)$$

and the characteristic function for the grouped distribution is—

$$M(t_1, t_2;\ \xi_1, \xi_2) = \sum_{r_1=-\infty}^{\infty} \sum_{r_2=-\infty}^{\infty} e^{it_1(r_1\omega_1 + \xi_1) + it_2(r_2\omega_2 + \xi_2)} F(r_1, r_2;\ \xi_1, \xi_2)$$

which has periods ω_1, ω_2 in ξ_1, ξ_2 respectively. Following Fisher(2), we develop M as a double Fourier series

$$M(t_1, t_2;\ \xi_1, \xi_2) = \sum_{-\infty}^{\infty} \sum_{-\infty}^{\infty} P_{s_1, s_2}\, e^{2\pi i\left(\frac{s_1\xi_1}{\omega_1} + \frac{s_2\xi_2}{\omega_2}\right)}$$

the coefficients being

$$P_{s_1,s_2} = \frac{1}{\omega_1\omega_2}\int_{\frac{1}{2}\omega_1}^{\frac{1}{2}\omega_1} d\xi_1 \int_{-\frac{1}{2}\omega}^{\frac{1}{2}\omega_2} d\xi_2\, M(t_1,t_2;\, \xi_1,\xi_2)\, e^{-2\pi i\left(\frac{s_1\xi_1}{\omega_1}+\frac{s_2\xi_2}{\omega_2}\right)}$$

$$= \frac{1}{\omega_1\omega_2}\int_{-\frac{1}{2}\omega_1}^{\frac{1}{2}\omega_1} dx_1 \int_{-\frac{1}{2}\omega_2}^{\frac{1}{2}\omega_2} dx_2 \sum_{r_1=-\infty}^{\infty}\ \sum_{r_2=-\infty}^{\infty} \int_{-\frac{1}{2}\omega_1}^{\frac{1}{2}\omega_1} d\xi_1 \int_{-\frac{1}{2}\omega_2}^{\frac{1}{2}\omega_2} d\xi_2\, f(x_1 + r_1\omega_1 + \xi_1,\, x_2 + r_2\omega_2 + \xi_2)$$

$$\times\ exp\left[i\left(t_1 - \frac{2\pi s_1}{\omega_1}\right)(r_1\omega_1 + \xi_1) + i\left(t_2 - \frac{2\pi s_2}{\omega_2}\right)(r_2\omega_2 + \xi_2)\right]$$

$$= \frac{1}{\omega_1\omega_2}\int_{-\frac{1}{2}\omega_1}^{\frac{1}{2}\omega_1} dx_1 \int_{-\frac{1}{2}\omega_2}^{\frac{1}{2}\omega_2} dx_2\, e^{-i\left(t_1-\frac{2\pi s_1}{\omega_1}\right)x_1 - i\left(t_2-\frac{2\pi s_2}{\omega_2}\right)x_2} \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} e^{i\left(t_1-\frac{2\pi s_1}{\omega_1}\right)z_1 + i\left(t_2-\frac{2\pi s_2}{\omega_2}\right)z_2} f(z_1, z_2)\, dz_1 dz_2$$

$$= \frac{sin\,(\frac{1}{2}\omega_1 t_1 - \pi s_1)}{(\frac{1}{2}\omega_1 t_1 - \pi s_1)} \cdot \frac{sin\,(\frac{1}{2}\omega_2 t_2 - \pi s_2)}{(\frac{1}{2}\omega_2 t_2 - \pi s_2)}\ \varphi\left(t_1 - \frac{2\pi s_1}{\omega_1},\, t_2 - \frac{2\pi s_2}{\omega_2}\right)$$

where

$$\varphi\,(t_1, t_2) = \int_{-\infty}^{\infty}\int_{-\infty}^{\infty} e^{it_1x_1 + it_2x_2} f(x_1, x_2)\, dx_1,\, dx_2$$

Hence

$$M(t_1, t_2;\, \xi_1, \xi_2) = \sum_{-\infty}^{\infty}\sum_{-\infty}^{\infty} \frac{sin\,(\frac{1}{2}\omega_1 t_1 - \pi s_1)}{(\frac{1}{2}\omega_1 t_1 - \pi s_1)} \cdot \frac{sin\,(\frac{1}{2}\omega_2 t_2 - \pi s_2)}{(\frac{1}{2}\omega_2 t_2 - \pi s_2)}\ \varphi\left(t_1 - \frac{2\pi s_1}{\omega_1},\, t_2 - \frac{2\pi s_2}{\omega_2}\right) e^{2\pi i\left(\frac{s_1\xi_1}{\omega_1}+\frac{s_2\xi_2}{\omega_2}\right)} \tag{2.1}$$

and the moments μ of the ungrouped distribution are given in terms of the grouped moments m, as far as the second order, by

$$\mu_1 = m_1 - \sum_{s=-\infty}^{\infty}{}' (-)^s \frac{i\omega_1}{2\pi s}\ \varphi\left(-\frac{2\pi s}{\omega_1}, 0\right) e^{\frac{2\pi i s\xi_1}{\omega_1}},\quad \mu_2 = \&c.$$

$$\mu_{20} = m_{20} - \frac{\omega_1^2}{12} - \sum_{s=-\infty}^{\infty}{}' (-)^s \frac{\omega_1^2}{2\pi^2 s^2}\ \varphi\left(-\frac{2\pi s}{\omega_1}, 0\right) e^{\frac{2\pi i s\xi_1}{\omega_1}},\quad \mu_{02} = \&c.$$

$$\mu_{11} = m_{11} + \sum_{s_1=-\infty}^{\infty}{}' \sum_{s_2=-\infty}^{\infty}{}' (-)^{s_1+s_2} \frac{\omega_1\omega_2}{4\pi^2 s_1 s_2}\ \varphi\left(-\frac{2\pi s_1}{\omega_1}, -\frac{2\pi s_2}{\omega_2}\right) e^{2\pi i\left(\frac{s_1\xi_1}{\omega_1}+\frac{s_2\xi_2}{\omega_2}\right)}. \tag{2.2}$$

(Σ' denoting summation excluding $s = 0$).

The first two are the ordinary univariate formulae, and subject to the customary conditions under which Sheppard's corrections are valid the remainders may be neglected, since ω is assumed small compared with $\sigma = \sqrt{\mu_{20}}$. As pointed out by Kendall([3]), if the group origins ξ_1 and ξ_2 are considered to be located at random, all the remainders, including that of μ_{11}, average out to zero and the average relations

$$\mu_1 = \overline{m}_1,\qquad \mu_{20} = \overline{m}_{20} - \frac{\omega_1^2}{12},\qquad \mu_{11} = \overline{m}_{11}$$

hold whatever the size of the group intervals or the value of the correlation. But this remark is not relevant to the present problem, since the operations of averaging m_{20}, m_{02} and m_{11} are not

independent and it is not legitimate to calculate a correlation coefficient from the average grouped moments in cases where the remainder for μ_{11} is not negligible.

3. *The Corrections for High Autocorrelations*

Autocorrelation coefficients calculated from stationary time series have the simplifying feature that the group interval ω and group origin ξ are the same for both variates x_1 and x_2. The distribution for each of the correlated variates is the same, though their joint distribution is not necessarily symmetrical in x_1, x_2. (Consider, for example, the time series $x_r = \varepsilon_r + \lambda\varepsilon_{r-1}$, where the distribution for the independent ε_r's is skew.) Nevertheless it is convenient to work in terms of $x_1 + x_2$ and $x_1 - x_2$, whose joint characteristic function is $\psi(u, v) = \varphi\,(u + v, u - v)$. The product moment formula (2.2) may be written in the form

$$\mu_{11} = m_{11} - \sum_{s=1}^{\infty} \frac{\omega_2}{4\pi^2 s^2} \left\{ \psi\left(0, -\frac{2\pi s}{\omega}\right) + \psi\left(0, \frac{2\pi s}{\omega}\right)\right\} \quad . \quad . \quad (3.1)$$

together with a remainder made up of terms involving $\psi\left(-\frac{\pi r}{\omega}, -\frac{(2s+r)\pi}{\omega}\right)$, $r \neq 0$. Since $\psi(t, 0)$ is the characteristic function of the aggregate $x_1 + x_2$ distribution, which has a standard error large compared with ω, and $\left|\psi\left(-\frac{\pi r}{\omega}, -\frac{(2s+r)\pi}{\omega}\right)\right| \leqslant \left|\psi\left(-\frac{\pi r}{\omega}, 0\right)\right|$, it is easy to show that the remainder is negligible whenever Sheppard's corrections are valid for the univariate moments. On the other hand, the terms containing $\psi\left(0, -\frac{2\pi s}{\omega}\right)$ may not be neglected, since $\psi\,(0, t)$ is the characteristic function of the aggregate $x_1 - x_2$ distribution whose standard error is small compared with ω for high enough autocorrelations.

We remark that when the autocorrelation is unity, all the higher moments of $x_1 - x_2$ are zero for the type of distributions usually met with, and

$$\psi\left(0, -\frac{2\pi s}{\omega}\right) = 1 \text{ for all } s. \quad \text{Then } \mu_{11} = m_{11} - \frac{\omega^2}{12}$$

and the correlation between the grouped variates is also unity. This is as it should be, for the observations now lie on the principal diagonal line of the square grid formed by the group intervals, and the operation of grouping merely rearranges the observations along the line. In the more general bivariate case when the correlation is perfect so that the observations lie along a line, grouping in general displaces the observations to either side of the line, making the correlation between the grouped variates less than unity.

4. *The Normal Case*

In many time series the differences $x_1 - x_2$ are found to have a near normal distribution. For short lags, this is not necessarily implied by approximate normality in the observations themselves. Time series generated by linear processes may often be expected to have normally distributed observations through the operation of the central limit theorem, but as the autocorrelation approaches unity the distribution of differences depends increasingly on the form of the distribution of the original random errors from which the time series was evolved.

When the differences can be considered normally distributed, we take $\psi\,(0, t) = e^{-\sigma^2(1-\rho)t^2}$, ρ being the autocorrelation coefficient and σ the standard deviation of the ungrouped observations (whose distribution is itself not necessarily assumed to be normal). The product moment relation becomes.

$$\rho\sigma^2 = m_{11} - \sum_{s=1}^{\infty} \frac{\omega^2}{2\pi s^2} e^{-\frac{4\pi^2\sigma^2}{\omega^2}(1-\rho)s^2} \quad . \quad . \quad . \quad . \quad . \quad (4.1)$$

Writing m_2 for the variance of the aggregate of all the grouped observations, $C = \frac{1}{\omega^2}(m_2 - m_{11})$ and $c = \frac{\sigma^2}{\omega^2}(1 - \rho)$, the corrected correlation is obtained as $\rho = 1 - \frac{\omega^2}{\sigma^2}c$, where c is given in

terms of C in Table I, col. 1, and $\sigma^2 = m_2 - \frac{\omega^2}{12}$ in the usual way. The table was computed from the formula

$$C = c + \frac{1}{12} - \sum_{s=1}^{\infty} \frac{1}{2\pi^2 s^2} e^{-4\pi^2 s^2 c} \qquad (4.2)$$

except for small values of c, when it is better to write (4.2) as

$$C = \int_0^c dc \sum_{s=-\infty}^{\infty} e^{-4\pi^2 s^2 c}$$

which transforms to

$$C = \int_0^c \frac{dc}{2\sqrt{\pi c}} \sum_{r=-\infty}^{\infty} e^{-\frac{r^2}{4c}} = \sqrt{\frac{c}{\pi}} + \int_0^c \frac{dc}{\sqrt{\pi c}} \sum_{r=1}^{\infty} e^{-\frac{r^2}{4c}} \qquad (4.3)$$

using a well known result. The integral is negligible for small c, since it behaves like $4c^{\frac{3}{2}}e^{-\frac{1}{4c}} / \sqrt{\pi}$ and for values of C up to 0.06 the formula

$$c = \pi C^2 \qquad (4.4)$$

is accurate to four places of decimals. (It may be used up to $C = 0{\cdot}14$ with an error in c of within 1 per cent.)

5. *The Effect of Non-normality*

From the form of (3.1) it is at once evident that, provided the remainder is negligible, the odd moments of $x_1 - x_2$ do not enter into the product moment correction, which is therefore unaffected by skewness in the difference distribution. To examine the effect of a moderate degree of kurtosis, let us suppose it to be sufficiently well represented by a Gram-Charlier Type A series with terms up to the fourth order, whose characteristic function is

$$\psi(0, t) = e^{-\sigma^2(1-\rho)t^2}\left[1 + \frac{1}{6}\sigma^4(1-\rho)^2\gamma_2 t^4\right] \qquad (5.1)$$

ignoring the t^3 term for the reason stated. The relation (4.2) between C and c is modified to

$$C = c + \frac{1}{12} - \sum_{s=1}^{\infty} \frac{1}{2\pi s^2} e^{-4\pi^2 s^2 c} - \frac{4}{3}\pi^2 c^2 \gamma_2 \sum_{s=1}^{\infty} s^2 e^{-4\pi^2 s^2 c} \qquad (5.2)$$

which for small c reduces to

$$c = \pi C^2 \Big/ \left(1 - \frac{\gamma_2}{24}\right)^2 \qquad (5.3)$$

Table I, col. 2, gives c in terms of C for $\gamma_2 = 1$. The percentage error in c if normality is assumed when in fact $\gamma_2 = 1$ is also shown. It is seen to increase to the value of 8·9 per cent. calculated from (5.3) as c tends to zero. Errors of this magnitude for $\gamma_2 = 1$ may therefore be expected in $1 - \rho$, the quantity usually of interest in the type of problem requiring the present theory.

Acknowledgment

I am indebted to Mrs. V. Semple for the computation of Table I.

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TABLE I

For small c, $c \sim \pi C^2 \Big/ \left(1 - \frac{\gamma_2}{24}\right)^2$

For large C, $c \sim C - \frac{1}{12}$

C.	c. $\gamma_2 = 0$.	c. $\gamma_2 = 1$.	Per cent. difference.
·01	·00031	·00034	8·9
·02	·00126	·00137	8·9
·03	·00283	·00310	8·9
·04	·00503	·00547	8·9
·05	·00785	·00855	8·9
·06	·01131	·01231	8·9
·07	·01539	·01676	8·9
·08	·02011	·02189	8·9
·09	·02545	·02770	8·8
·10	·03141	·03415	8·7
·11	·03800	·04118	8·4
·12	·04519	·04870	7·8
·13	·05294	·05662	7·0
·14	·06119	·06487	6·0
·15	·06988	·07338	5·0
·16	·07891	·08218	4·1
·17	·08822	·09106	3·2
·18	·09774	·10017	2·5
·19	·10740	·10944	1·9
·20	·11716	·11884	1·4
·21	·12700	·12835	1·1
·22	·13689	·13797	0·8
·23	·14682	·14766	0·6
·24	·15677	·15742	0·4
·25	·16674	·16724	0·3
·26	·17671	·17709	0·2
·27	·18670	·18699	0·1
·28	·19669	·19690	0·1
·29	·20668	·20684	0·1
·30	·21668	·21680	0·0

SOME SEQUENTIAL TESTS OF STUDENT'S HYPOTHESIS

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(Communication from the National Physical Laboratory)

1. *Introduction*

SEQUENTIAL analysis has been introduced in America by Wald (1945) and in this country by Barnard (1946), and these two papers should be consulted for the principles of the subject. Barnard's paper contains a review of Wald's work.

I shall propose here a simple sequential test of Student's hypothesis, based on Wald's binomial test, and compare it with the ordinary Student's t-test, and also with a test proposed by Wald.

It is required to test the hypothesis H_0 that the mean μ of a normal population is a certain value μ_0, the variance σ^2 being unknown. Dantzig (1940) has shown that any such test based on a single sample, whose power function is independent of σ, must have constant power for all values of μ, equal to the size of the critical region or significance level. (Such a test, for example, would be the trivial one which results in the null hypothesis always being accepted. The power is then constant, equal to zero.) This result, however, does not apply to sequential tests, and in fact Stein (1945) has produced a two-sample test for Student's hypothesis having a power function independent of σ. The average sample size of this test depends upon σ, and the test may be compared for economy in sampling with tests for the hypothesis that the mean is μ_0 when the variance is known. While tests of the type discussed by Stein are clearly important, I shall consider here only sequential tests whose power is a function of $(\mu - \mu_0)/\sigma$, and so are directly comparable with the t-test.

It is customary to distinguish two different uses of the t-test as a test of H_0. The one-sided test is sensitive to values of μ on one side only of μ_0 and consists in rejecting H_0 whenever

$$t = \frac{(\bar{x} - \mu_0)\sqrt{N-1}}{s} > t_{2\alpha,\, N-1}, \qquad . \quad . \quad . \quad . \quad . \qquad (1)$$

where $\bar{x}$, s are the mean and standard deviation (divisor $\sqrt{N}$) of a sample of size N, and $t_{2\alpha,\, N-1}$ is taken, for example, from the table of the distribution of t in Fisher and Yates (1943), corresponding to a probability level of 2α and $N - 1$ degrees of freedom. This test has the properties that

$$\left.\begin{array}{ll} \text{(i)} & \text{the probability is } \alpha \text{ of rejecting } H_0, \text{ when it is true,} \\ \text{(ii)} & \quad ,, \qquad ,, \qquad ,, \leqslant \beta \text{ of accepting } H_0, \text{ when in fact } \mu - \mu_0 \geqslant D\sigma,\ (D > 0), \end{array}\right\} \quad (2)$$

where β is a function of N, α and D. Neyman and Pearson (1933) have shown that this defines a uniformly most powerful test against hypotheses that $\mu > \mu_0$. Similarly, the test which rejects H_0 whenever $t > - t_{2\alpha,\, N-1}$ has the properties (2) above, with $\geqslant D\sigma$ replaced by $\leqslant - D\sigma$, and is uniformly most powerful with respect to values of $\mu < \mu_0$.

The two-sided test is sensitive to values of μ on both sides of μ_0, and consists in rejecting H_0 when

$$|\, t \,| > t_{\alpha,\, N-1},$$

where $t_{\alpha,\, N-1}$ corresponds in Fisher and Yates' Tables to a probability level of α and $N - 1$ degrees of freedom. This test has the properties that

$$\left.\begin{array}{ll} \text{(i)} & \text{the probability is } \alpha \text{ of rejecting } H_0, \text{ when it is true,} \\ \text{(ii)} & \quad ,, \qquad ,, \qquad ,, \leqslant \beta \text{ of accepting } H_0, \text{ when in fact } |\, \mu - \mu_0 \,| \geqslant D\sigma, \end{array}\right\} . \quad . \quad (3)$$

β again being a function only of N, α and D. This test is known to possess the optimum property of being unbiased of type B_1 (Neyman, 1935).

2. *One-sided Tests*

2.1. *A Sequential Test.*—A test for Student's hypothesis to detect one-sided alternatives may be derived quite simply from Wald's probability ratio sequential (P.R.S.) test for the probability p in the binomial distribution.

Denote observations greater and less than μ_0 by $+$ and $-$ respectively. On the hypothesis H_0 that $\mu = \mu_0$, the probability of an observation being a $-$ is $\frac{1}{2}$, while on the hypothesis H_+ that $\mu = \mu_0 + D\sigma$, this probability is

$$p_1 = \frac{1}{\sqrt{2\pi}} \int_D^{\infty} e^{-\frac{1}{2}x^2} dx.$$

If, therefore, the observations are taken sequentially and recorded as $+$ or $-$, a test fulfilling the conditions (2) will be the Wald P.R.S. binomial test with a probability of error of the first kind equal to β for $p = p_1$ and a probability of error of the second kind equal to α for $p = \frac{1}{2}$. (The "probability of error of the first kind" in this context is not the probability of rejecting, when true, Student's hypothesis, but the probability of rejecting, when true, the smaller value of p.)

This test is defined *approximately* by the following procedure (see Barnard's paper):

Start a score at a_2 marks.
For each $+$, add on 1 mark,
" " $-$, subtract b marks.
Reject H_0 when the score reaches $a_1 + a_2$,
Accept H_0 " " " zero,

where $a_1 = \dfrac{\log (1 - \beta)/\alpha}{\log 2(1 - p_1)}$, $\quad a_2 = \dfrac{\log (1 - \alpha)/\beta}{\log 2(1 - p_1)}$

and $b = \dfrac{\log 1/2p_1}{\log 2(1 - p_1)}$.

(From the form in which this result is quoted by Barnard, p_2 has been replaced by $\frac{1}{2}$, H and H' by a_1 and a_2 so as to avoid confusion with the notation for hypotheses, and α and β interchanged, since in our notation α is the probability of rejecting, when true, the *higher* value of p.)

Table 1 gives the values of a_1, a_2 and b for $D = 0{\cdot}5\ (0{\cdot}1)\ 3{\cdot}0$ and three different combinations of α and β. (The examples given later in this paper are not restricted to the use of these values of α and β.)

The above test will detect sufficiently large changes of the mean in the positive direction. It is clearly applicable for detecting changes in the negative direction. Thus a test fulfilling condition (2) (ii), but with $\geqslant D\sigma$ replaced by $\leqslant -D\sigma$ will be defined by the following procedure:

Start a score at a_1 marks.
For each $+$, add on b marks,
" " $-$, subtract 1 mark.
Reject H_0 when the score reaches zero,
Accept H_0 " " " $a_1 + a_2$,

a_1, a_2 and b being the same as before. For simplicity, it will be assumed in the Examples and in paragraph 2.2 that in using a one-sided test we wish to detect positive changes of the mean.

Example 1

A test is required for which $D = 1$, $\alpha = 0{\cdot}025$, $\beta = 0{\cdot}1$.

These values of D, α and β give $p_1 = 0{\cdot}1587$, whence $a_2 = 4{\cdot}38$, $b = 2{\cdot}21$, $a_1 + a_2 = 11{\cdot}26$. The application of the test is much facilitated by the use of integers for a_1, a_2 and b. If we add 9 instead of 1 to the score for a $+$, we shall replace a_2, b and $a_1 + a_2$ by $9a_2$, $9b$ and $9(a_1 + a_2)$, which in this case are approximately equal to 39, 20 and 101 respectively. (This makes little difference to the validity of the test, especially as it results in a good approximation to b.)

TABLE 1

	$\alpha = \cdot 01,\ \beta = \cdot 10$		$\alpha = \cdot 025,\ \beta = \cdot 10$		$\alpha = \cdot 05,\ \beta = \cdot 10$		
D.	a_1	a_2	a_1	a_2	a_1	a_2	b.
0·5	13·88	7·07	11·05	7·02	8·92	6·94	1·49
0·6	12·08	6·15	9·62	6·11	7·76	6·04	1·61
0·7	10·81	5·51	8·61	5·47	6·95	5·41	1·74
0·8	9·89	5·04	7·87	5·00	6·35	4·95	1·89
0·9	9·19	4·68	7·32	4·65	5·90	4·60	2·04
1·0	8·65	4·41	6·89	4·38	5·55	4·33	2·21
1·1	8·22	4·19	6·55	4·16	5·28	4·11	2·38
1·2	7·88	4·02	6·28	3·99	5·06	3·94	2·57
1·3	7·61	3·88	6·06	3·85	4·89	3·81	2·78
1·4	7·39	3·77	5·89	3·74	4·75	3·70	2·99
1·5	7·21	3·67	5·74	3·65	4·63	3·61	3·23
1·6	7·07	3·60	5·63	3·58	4·54	3·54	3·47
1·7	6·95	3·54	5·53	3·52	4·46	3·48	3·73
1·8	6·85	3·49	5·46	3·47	4·40	3·43	4·01
1·9	6·78	3·45	5·40	3·43	4·35	3·39	4·30
2·0	6·72	3·42	5·35	3·40	4·31	3·36	4·61
2·1	6·67	3·40	5·31	3·37	4·28	3·34	4·94
2·2	6·63	3·38	5·28	3·35	4·26	3·32	5·28
2·3	6·59	3·36	5·25	3·34	4·24	3·30	5·63
2·4	6·57	3·35	5·23	3·33	4·22	3·29	6·00
2·5	6·55	3·34	5·22	3·32	4·21	3·28	6·39
2·6	6·54	3·33	5·21	3·31	4·20	3·27	6·79
2·7	6·52	3·32	5·20	3·30	4·19	3·26	7·21
2·8	6·52	3·32	5·19	3·30	4·19	3·26	7·64
2·9	6·51	3·32	5·18	3·29	4·18	3·26	8·09
3·0	6·50	3·31	5·18	3·29	4·18	3·25	8·55

TABLE 2*a*

$\mu = 0,\ \sigma = 10$

Reading	+ or −	Score
..	..	39
7·9	+	48
6·7	+	57
−22·3	−	37
−3·1	−	17
0·2	+	26
−6·6	−	6
6·1	+	15
−11·7	−	−5

Accept H_0

TABLE 2*b*

$\mu = 10,\ \sigma = 10$

Reading	+ or −	Score
..	..	39
7·1	+	48
8·8	+	57
16·9	+	66
−5·9	−	46
3·7	+	55
6·5	+	64
17·5	+	73
22·3	+	82
13·0	+	91
5·8	+	100
−8·7	−	80
1·0	+	89
9·9	+	98
1·4	+	107

Reject H_0

Tables 2*a* and 2*b* illustrate the application of the test to observations taken sequentially, at random, from normal populations, first having $\mu = 0$, $\sigma = 10$, and secondly having $\mu = 10$, $\sigma = 10$, the test being based on $\mu_0 = 0$.

Thus, in the first case H_0 was accepted correctly after a sample of 8, and in the second case H_0 was rejected correctly after a sample of 14.

Example 2

An immediate application is to the problem of testing whether the means of two normal populations are equal, the variances being unknown. Student's t-test may be applied and is valid if the variances of the two populations are equal. Otherwise it is fairly safe to use if the sample sizes N_1, N_2 are equal ($= N$) (e.g. Hsu, 1938). In this case, however, an exact t-test may be obtained, for if the observations, x_i, y_i ($i = 1, 2, \ldots, N$), are independent, then $z_i = (x_i - y_i)$ is distributed normally with mean $(\mu_1 - \mu_2)$ and variance $(\sigma_1^2 + \sigma_2^2)$, where μ_1, μ_2, σ_1, σ_2 are respectively the means and the standard deviations of the distributions of x and y. In the sequential test, therefore, we take pairs of observations (x, y) from the two populations and treat the difference $(x - y)$ as the new variable.

H_0 is now the hypothesis that $\mu_1 - \mu_2 = \mu_0$ (in the usual situation where it is required to test whether μ_1 and μ_2 are equal, $\mu_0 = 0$), and the sequential test gives a probability of α of rejecting H_0, when it is true, and a probability $\leq \beta$ of accepting H_0 when in fact

$$(\mu_1 - \mu_2) \geq \mu_0 + D(\sigma_1^2 + \sigma_2^2)^{\frac{1}{2}}.$$

In Tables 3*a* and 3*b* are given random samples taken sequentially from two normal populations, first having $\mu_1 = 0$, $\sigma_1 = 10$, $\mu_2 = 0$, $\sigma_2 = 5$; secondly having $\mu_1 = 11{\cdot}2$, $\sigma_1 = 10$, $\mu_2 = 0$, $\sigma_2 = 5$, where $11{\cdot}2 = \sqrt{(10^2 + 5^2)}$. The test is based on $\mu_0 = 0$, $D = 1$, $\alpha = {\cdot}025$, $\beta = {\cdot}1$, so the same values of a_1, a_2 and b are used as in Example 1.

TABLE 3*a*

$\mu_1 = 0, \sigma_1 = 10; \mu_2 = 0, \sigma_2 = 5$

x	y	$x-y$	+ or −	Score
..	..	..	.	39
2·9	2·0	0·9	+	48
9·0	11·1	−2·1	−	28
4·9	−2·9	7·8	+	37
9·6	−0·5	10·1	+	46
5·2	7·5	−2·3	−	26
−13·3	−6·5	−6·8	−	6
−5·7	8·8	−14·5	−	−14

Accept H_0

TABLE 3*b*

$\mu_1 = 11{\cdot}2, \sigma_1 = 10; \mu_2 = 0, \sigma_2 = 5$

x	y	$x-y$	+ or −	Score
..	..	..	..	39
24·3	4·2	20·1	+	48
7·5	−7·3	14·8	+	57
9·7	7·3	2·4	+	66
30·5	0·7	29·8	+	75
5·9	0·9	5·0	+	84
26·9	−1·2	28·1	+	93
15·7	−3·7	19·4	+	102

Reject H_0

Thus, in the first case H_0 was accepted correctly after a sample of 7, and in the second case H_0 was rejected correctly after a sample of 7.

The power of this method is discussed at the end of paragraph 2.2.

2.2. *Economy of the test as compared with Student's* t-*test*.—Denote by H_+ the hypothesis that $\mu = \mu_0 + D\sigma$. The one-sided t-test defined by (1) has a probability α of rejecting H_0 when true, and a probability β of accepting H_0 when H_+ is true, where

$$1 - \beta = P\{t > t_{2\alpha,\, N-1} \mid H_+\}$$

$$= P\left\{\frac{\sqrt{N-1}\,(\bar{x}/\sigma - \mu_0/\sigma - D) + \sqrt{N-1}\,D}{s/\sigma} > t_{2\alpha,\, N-1} \,\middle|\, H_+\right\}.$$

If H_+ is true, $\sqrt{N}(\bar{x}/\sigma - \mu_0/\sigma - D)$ and $Ns^2/(N-1)\sigma^2$ are distributed respectively as a normal deviate with zero mean and unit variance, and as $\chi^2/(N-1)$ with $(N-1)$ degrees of freedom, and the quantity

$$\frac{\sqrt{N-1}\,(\bar{x}/\sigma - \mu_0/\sigma - D) + \sqrt{N-1}\,D}{s/\sigma}$$

is therefore distributed in the non-central t-distribution (*cf.* Johnson and Welch, 1940).

Using the notation for this distribution adopted in this last-quoted paper we have

$$1-\beta = P\,(N-1,\ \sqrt{N}D,\ t_{2\alpha,\,N-1}),$$

and the value of D for which β takes a certain value is given by

$$\begin{aligned}\sqrt{N}D &= \delta\,(N-1,\ t_{2\alpha,\,N-1},\ 1-\beta)\\ &= -\,\delta\,(N-1,\ -t_{2\alpha,\,N-1},\ \beta) \qquad (3\text{A})\end{aligned}$$

From the tables of $\delta(N-1,\ -t_{2\alpha,\,N-1},\ \beta)$ given by Johnson and Welch, and from some tables by Neyman and Tokarska (1936), the graphs shown in Fig. 1 were drawn. For various

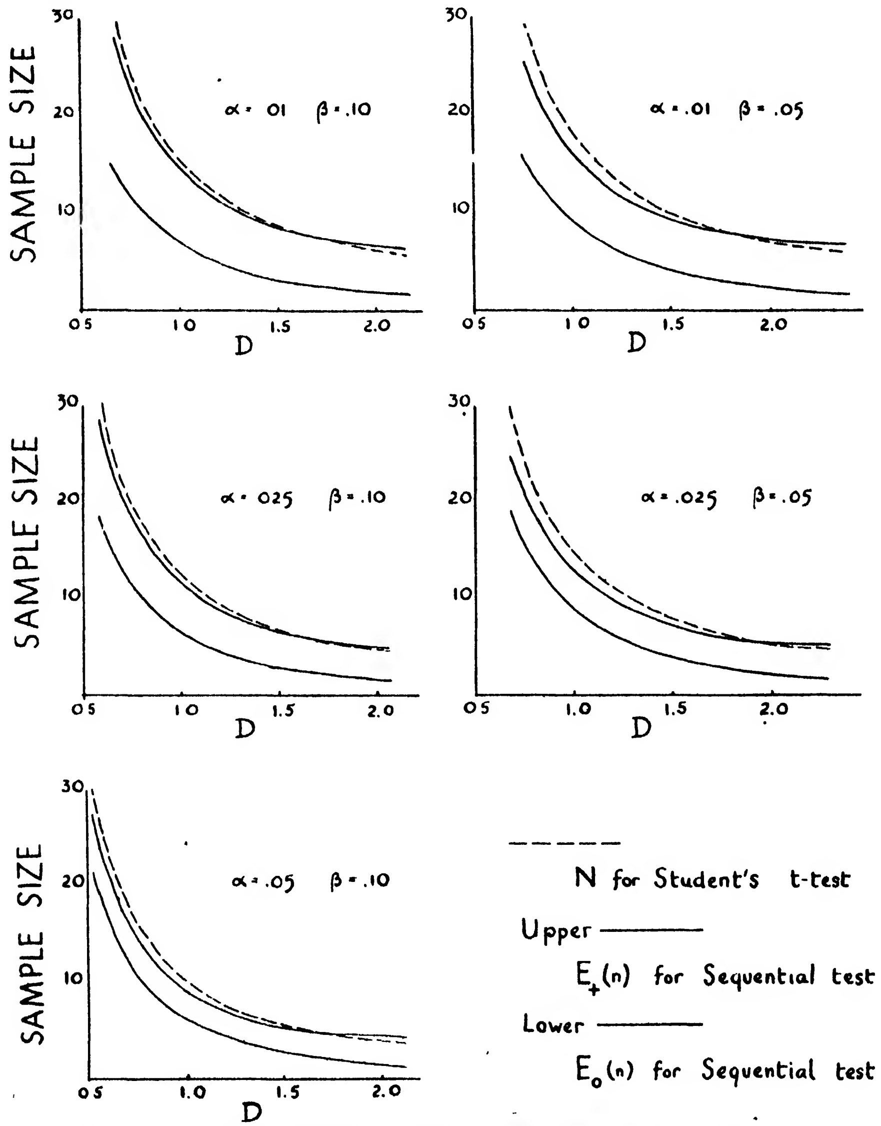

FIG. 1.—Graphs comparing the average sample size required by the sequential test of paragraph 2.1, when H_0 or $H+$ is true, with the sample size of the equivalent Student's t-test.

combinations of α and β likely to be used, the graphs show the relation between D and N. (Strictly, the relation is defined only for integral values of N, but for purposes of comparison we shall use (3A) to define the relation between D and non-integral sample sizes.)

The sequential test suggested in paragraph 2.1 is clearly equivalent to this test in that it has the same probabilities of error of the two kinds, α and β, with respect to the same hypotheses H_0 and H_+. Unlike Student's test, however, the sequential test has a variable sample size, n, and on each of the hypotheses H_0 and H_+, n will be distributed with means $E_0(n)$ and $E_+(n)$ respectively. If we show that, say, $E_0(n) < N$ for some values of α, β and D, we may say that when H_0 is true the sequential test is, in the long run, the more economical.

Now $E_0(n)$ and $E_+(n)$ are given approximately by the following formulae (see Wald) :

$$E_0(n) \simeq \frac{(1-\alpha)\log(1-\alpha)/\beta - \alpha\log(1-\beta)/\alpha}{\frac{1}{2}\log 1/4p_1(1-p_1)} = \frac{(1-\alpha)a_2 - \alpha a_1}{\frac{1}{2}(b-1)}. \quad . \quad (4)$$

$$E_+(n) \simeq \frac{(1-\beta)\log(1-\beta)/\alpha - \beta\log(1-\alpha)/\beta}{(1-p_1)\log 2(1-p_1) - p_1\log 1/2p_1} = \frac{(1-\beta)a_1 - \beta a_2}{1-p_1(b+1)}. \quad . \quad (5)$$

The graphs of $E_0(n)$ and $E_+(n)$ as functions of D are drawn in Fig. 1. It will be seen that for given α, β and D, $E_0(n)$ is always considerably less than N, and that $E_+(n)$ and N are nearly equal, $E_+(n)$ being the smaller for $D < D_0$, where D_0 varies between 1·7 and 2·0.

It must be remembered that if a_1, a_2 and b are defined as in paragraph 2.1 in terms of D, α and β, then a test based on them will not have exactly the required properties, nor will $E_0(n)$ and $E_+(n)$ be given exactly by the above equations, so the graphs of Fig. 1 are not strictly accurate. Investigations based on the exact solutions of Burman (1946) for a Wald P.R.S. binomial test, for integral a_1, a_2 and b, suggest that for given D, α and β, the test will produce an error of the first kind very nearly equal to α, and an error of the second kind less than β, and is consequently equivalent to a Student's test of the same significance level as expected, but with a greater sample size. $E_0(n)$ is given quite accurately by (4), but $E_+(n)$ is greater than (5). The graphs shown in Fig. 1 are probably substantially correct.

In practice μ will usually take values other than μ_0 and $\mu_0 + D\sigma$. The curve of the average sample size $E(n)$ as a function of μ has a maximum between these two values, $E(n)$ being less than $E_0(n)$ for $\mu < \mu_0$ and less than $E_+(n)$ for $\mu > \mu_0 + D\sigma$.

Fig. 1 may also be used to describe the relation between the sequential test used in Example 2, with the variate $z = (x - y)$, and the corresponding fixed sample size test using z, which we shall call the *difference* test. Now the difference test is not the most powerful with respect to values of $\mu_1 - \mu_2$ greater than μ_0. When $\sigma_1 = \sigma_2 = \sigma$, the Student's test based on the criterion

$$\frac{(\bar{x}-\bar{y})\sqrt{N-1}}{\sqrt{s_1^2+s_2^2}},$$

where $\bar{x}$, $\bar{y}$, s_1 and s_2 are respectively the means and standard deviations in the two samples of size N, is uniformly most powerful for these alternatives. We shall call this *Student's* test, although the difference test also uses the t-distribution. Now, the denominator of the difference test is the sample standard deviation of z, which is distributed as a multiple of χ with $N-1$ degrees of freedom, while that of Student's test has $2N-2$ degrees of freedom, and in fact we can say that by equation (3A) the Student's test gives

$$\sqrt{N}\,D_1 = -\,\delta\,(2N-2,\ -t_{2\alpha,\,2N-2},\ \beta),$$

while the difference test gives

$$\sqrt{N}\,D_2 = -\,\delta\,(N-1,\ -t_{2\alpha,\,N-1},\ \beta),$$

where β is the probability of accepting H_0 when $\mu_1 - \mu_2 = \mu_0 + \sqrt{2}D_i\sigma$ ($i = 1, 2$). For large N these two expressions tend to equality, and for fairly small N their difference is small. For example, for $N = 10$, $\alpha = {\cdot}025$, $\beta = {\cdot}1$, we have $D_1 = 1{\cdot}08$ and $D_2 = 1{\cdot}15$. Fig. 1, then, gives a fairly good idea, at any rate for $\sigma_1 = \sigma_2$, of the relation between the sequential test for two populations and the current Student's test.

3. *Two-sided Tests*

3.1. *Extension of the sequential test.*—The test defined in paragraph 2.1 may be extended to the problem of detecting departures of μ from μ_0 in both directions. Let us denote by T_+ the one-sided P.R.S. test for positive changes of μ, as given in paragraph 2.1, but with α replaced by $\alpha/2$, and by T_- the corresponding test for negative changes of μ, again with α replaced by $\alpha/2$. H_0, H_+ and H_- denote the hypothesis that $\mu = \mu_0$, $\mu_0 + D\sigma$ and $\mu_0 - D\sigma$, respectively. T_+ will then accept either H_0 or H_+, and T_- either H_0 or H_-. If observations are drawn sequentially from the population and subjected to both T_+ and T_- until results are obtained from both tests, we can have the following combinations of results :

		Test T_+		Test T_-
(I)	.	Accept H_0	.	Accept H_0
(II)	.	,, H_+	.	,, H_0
(III)	.	,, H_0	.	,, H_-

We now formulate the following test:

For (I), accept H_0
,, (II), ,, H_+
,, (III), ,, H_-

Fig. 2 illustrates the position on the lattice diagram (Barnard's "inspection diagram"). A + is represented by a move of 1 unit parallel to Ox, and a – by 1 unit parallel to Oy.

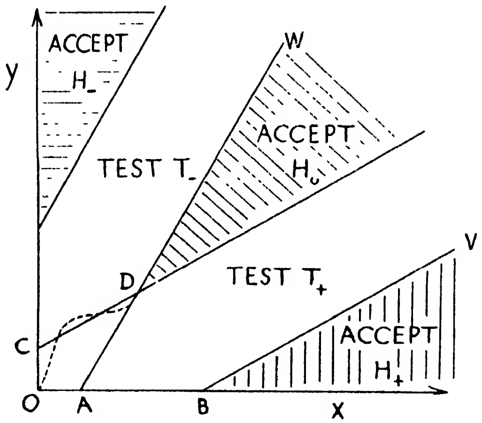

FIG. 2.—Lattice diagram representation of the two-sided sequential test of paragraph 3.1.

The regions are symmetrically placed, $OA = a_2/b$, $OB = a_1$, and the slopes of BV and AW are respectively $1/b$ and b. (The boundaries are strictly not straight lines, but in the form of steps, as may easily be seen from the test procedure of paragraph 2.1, and the exact dimensions are given by Stockman and Armitage (1946).)

The fourth alternative, to accept H_+ under T_+ and H_- under T_-, is clearly impossible if $OA \leqslant OB$, and this follows from the definitions of a_1, a_2 and b, provided that $\alpha \leqslant \beta$.

The final decision is made when a path reaches a shaded region, with the exception that if *both CD* and *AD* are crossed (as shown by the dotted line), we accept H_0 immediately, instead of waiting till the path reaches one of the shaded regions.

When H_0 is true, the probabilities of (II) and (III) are each $\alpha/2$; when H_+ is true, the proba-

bility of (II) is $1 - \beta$; and when H_- is true, the probability of (III) is $1 - \beta$. Consequently, the test has the following properties:

$$\left.\begin{array}{l} \text{(i) the probability is } \alpha \text{ of rejecting } H_0 \text{, when it is true,} \\ \text{(ii) ,, ,, ,, } \leqslant \beta \text{ of rejecting } H_+ \text{, when } \mu - \mu_0 \geqslant D\sigma, \\ \text{(iii) ,, ,, ,, } \leqslant \beta \text{ ,, ,, } H_- \text{, ,, } \mu - \mu_0 \leqslant -D\sigma. \end{array}\right\} \quad . . \quad . \quad (6)$$

3.2. *Comparison with the two-sided Student's test.*—Suppose that, in applying the two-sided Student's test, we accept H_+ when $t > t_{\alpha, N-1}$, and accept H_- when $t < -t_{\alpha, N-1}$; then the test satisfies the conditions (6), and may be directly compared for economy with the sequential test of paragraph 3.1.

It will be noticed that the two sets of conditions (3) and (6) differ slightly in form. In (6), the probability of rejecting H_+, when true, is equal to the probability of accepting H_0 plus the probability of accepting H_-, when H_+ is true, and so the β of (6) is greater than the β of (3). In fact, denoting these two values by β_2 and β_1 respectively, we have (paragraph 2.2):

$$\beta_2 = 1 - P(N-1, \sqrt{N}D, t_{\alpha, N-1})$$
$$\beta_1 = P(N-1, \sqrt{N}D, -t_{\alpha, N-1}) - P(N-1, \sqrt{N}D, t_{\alpha, N-1}),$$

$1 - \beta_1$ being the "power" of the test with respect to H_+. Since $P(N-1, \sqrt{N}D, -t_{\alpha, N-1})$ will in all practical cases be very nearly equal to 1 (i.e. H_- is very unlikely to be accepted when H_+ is true), β_1 and β_2 will differ very slightly, and as the formula for β_1 does not admit of a tabulated inversion analogous to (3A), we shall use $\beta = \beta_2$. The graphs of Fig. 1 are thus applicable for obtaining N in terms of D, α and β if for a significance level of α we read off the graph corresponding to $\alpha/2$.

Now the sample size of the sequential test of paragraph 3.1 is distributed as the maximum of a sample of 2 (non-independent) observations from a population having the same distribution as the sample size in the one-sided case, when H_0 is true; and as the maximum of two (non-independent) observations taken from different populations, when H_+ or H_- is true. (When H_+ is true, however, T_- will almost always accept H_0 quickly, and the sample size will be that required for T_+ to accept or reject H_+, which may be found from Fig. 1; similarly when H_- is true.)

The sequential test with $\alpha = \cdot 05$, $\beta = \cdot 1$, $D = 1$ was repeated 60 times with samples from a normal population, when H_0 was true, and the distribution of sample size had mean 13·88 and variance 56·20. For H_+, the average sample size $E_+(n)$, as given by Fig. 1, is about 11·7. The sample size of the corresponding t-test is about 12·6. As a second example, the test with $\alpha = \cdot 05$, $\beta = \cdot 05$, $D = 2$ was repeated 60 times with samples from a normal population with H_0 true, and the distribution of sample size had mean 5·15 and variance 5·03. For H_+, the average sample size $E_+(n)$, as given by Fig. 1, is about 5·6. The sample size of the corresponding t-test is also about 5·6.

3.3. *Another binomial test.*—As an alternative to the test of paragraph 3.1, Barnard suggests the following:

Observations are taken sequentially, and in pairs. The combinations $+-$ or $-+$ are denoted by y, and $++$ or $--$ by x. Under H_0 the probability of a y is $\frac{1}{2}$, while under H_+ or H_- the probability of a y is

$$p = 1 - p_1^2 - (1 - p_1)^2$$
$$= 2p_1 (1 - p_1),$$

where, as before,

$$p_1 = \frac{1}{\sqrt{2\pi}} \int_D^\infty e^{-\frac{1}{2}x^2} dx.$$

This test does not distinguish between H_+ and H_-, and its properties may be put in the form (3), where $\beta = \beta_1$ (see paragraph 3.2). In practice, of course, if H_0 was rejected it would be quite

easy to see whether this was due to a preponderance of $++$ or $--$, and thus whether H_+ or H_- was true. The sample size is twice that required for a P.R.S. test to distinguish between $2p_1(1-p_1)$ and $\frac{1}{2}$, with errors of the first and second kinds β and α respectively; while in the test of paragraph 3.1, the sample size is the maximum of two (non-independent) sample sizes for the P.R.S. test to distinguish between p_1 and $\frac{1}{2}$, with errors of the first and second kinds β and $\frac{1}{2}\alpha$ respectively. It is not obvious how these two tests compare in general.

For the first example considered in paragraph 3.2, the test suggested by Barnard is very uneconomical, having $E_0(n) = 32{\cdot}6$ and $E_+(n) = 42{\cdot}0$.

3.4. *Wald's non-linear test.*

3.4.1. Wald (1945, pp. 183–186) proposes a P.R.S. test satisfying conditions (3) which is not linear, i.e. does not reduce to a linear scoring procedure.

If the observations in a sample of n are x_{in} $(i = 1, 2, \ldots, n)$, let

$$X = \frac{\frac{1}{2}\int_0^\infty \frac{1}{\sigma^n}\exp\left[-\frac{\Sigma(x_{in}-\mu_0-D\sigma)^2}{2\sigma^2}\right]d\sigma}{\int_0^\infty \frac{1}{\sigma^n}\exp\left[-\frac{\Sigma(x_{in}-\mu_0)^2}{2\sigma^2}\right]d\sigma} + \frac{\frac{1}{2}\int_0^\infty \frac{1}{\sigma^n}\exp\left[-\frac{\Sigma(x_{in}-\mu_0+D\sigma)^2}{2\sigma^2}\right]d\sigma}{\int_0^\infty \frac{1}{\sigma^n}\exp\left[-\frac{\Sigma(x_{in}-\mu_0)^2}{2\sigma^2}\right]d\sigma}, \quad . \quad . \quad . \quad . \quad (7)$$

the summations being over $i = 1, 2, \ldots, n$. (Wald's d_0 is replaced by D, and the mean μ_0 has been introduced instead of zero without any loss of generality.) At the nth stage,

Accept H_0 if $X \leq \beta/(1-\alpha)$.
Reject H_0 if $X \geq (1-\beta)/\alpha$.
Take a further observation if $\beta/(1-\alpha) < X < (1-\beta)/\alpha$.

Wald shows that the power of the test is a monotonically increasing function of $|(\mu-\mu_0)/\sigma|$, and that X is a strictly increasing function of z^2, where $z = \Sigma(x-\mu_0)/\sqrt{\Sigma(x-\mu_0)^2}$ (the suffices having been dropped), without actually evaluating X.

The integrals in (7) are evaluated in paragraph 3.4.2, and expressions for X given in terms of z, n and D, so that if L_n, U_n are solutions for z^2 of the equations

$X = \beta/(1-\alpha)$
and $X = (1-\beta)/\alpha$, respectively,

the procedure at the nth stage becomes:

Accept H_0 if $z^2 \leq L_n$.
Reject H_0 if $z^2 \geq U_n$.
Take a further observation if $L_n < z^2 < U_n$.

Values of L_n and U_n for $n = 2, 3, \ldots, 30$, and for various combinations of α, β and D are given in Table 4.

3.4.2. The first term of (7)

$$= \frac{\frac{1}{2}\int_0^\infty \frac{1}{\sigma^n}\exp\left[-\frac{\{\Sigma(x-\mu_0)^2-2D\sigma\Sigma(x-\mu_0)+nD^2\sigma^2\}}{2\sigma^2}\right]d\sigma}{\int_0^\infty \frac{1}{\sigma^n}\exp\left[-\frac{\Sigma(x-\mu_0)^2}{2\sigma^2}\right]d\sigma} \quad . \quad . \quad . \quad (8)$$

In the numerator of (8), put $y = \sqrt{\Sigma(x-\mu_0)^2}/\sigma - D\,\Sigma(x-\mu_0)/\sqrt{\Sigma(x-\mu_0)^2}$, and in the denominator, put $y = \Sigma(x-\mu_0)^2/2\sigma^2$. Then (8) becomes

$$\exp\left[\frac{D^2}{2}(z^2-n)\right]\int_{-Dz}^{\infty}(y+Dz)^{n-2}e^{-\frac{1}{2}y^2}\,dy$$

Similarly, putting $y = \sqrt{\Sigma(x-\mu_0)^2}/\sigma + D\Sigma(x-\mu_0)/\sqrt{\Sigma(x-\mu_0)^2}$ in the numerator, the second term of (7) becomes

$$\frac{\exp\left[\frac{D^2}{2}(z^2-n)\right]\int_{Dz}^{\infty}(y-Dz)^{n-2}e^{-\frac{1}{2}y^2}\,dy}{2^{\frac{n-1}{2}}\,\Gamma\left(\frac{n-1}{2}\right)}.$$

$$\therefore\; X = \mathrm{I}\,.\,\frac{\exp\left[\frac{D^2}{2}(z^2-n)\right]}{2^{\frac{n-1}{2}}\,\Gamma\left(\frac{n-1}{2}\right)},$$

$$\text{where I} = \int_{-Dz}^{\infty}(y+Dz)^{n-2}e^{-\frac{1}{2}y^2}\,dy + \int_{Dz}^{\infty}(y-Dz)^{n-2}e^{-\frac{1}{2}y^2}\,dy$$

$$= \int_{-\infty}^{Dz}|(y-Dz)^{n-2}|\,e^{-\frac{1}{2}y^2}\,dy + \int_{Dz}^{\infty}|(y-Dz)^{n-2}|\,e^{-\frac{1}{2}y^2}\,dy$$

$$= \int_{-\infty}^{\infty}|(y-Dz)^{n-2}|\,e^{-\frac{1}{2}y^2}\,dy.$$

$I\,/\,\sqrt{2\pi}$ is the $(n-2)$-th absolute moment about the point Dz of the normal distribution with zero mean and unit variance.

For even n,

$$\mathrm{I} = \int_{-\infty}^{\infty}\sum_{j=0}^{n-2}(-1)^j\,{}^{n-2}C_j\,(Dz)^j\,y^{n-j-2}\,e^{-\frac{1}{2}y^2}\,dy$$

$$\sqrt{2\pi}\sum_{j=0}^{\frac{n-2}{2}}(n-3-2j)\,(n-5-2j)\;.\;.\;.\;3.1.\;\;{}^{n-2}C_{2j}\,(Dz)^{2j}.$$

$$\therefore\; X = I\,.\,\frac{\exp\left[\frac{D^2}{2}(z^2-n)\right]}{\sqrt{2\pi}\,(n-3)\,(n-5)\;.\;.\;.\;3.1}$$

$$= \exp\left[\frac{D^2}{2}(z^2-n)\right].\sum^{\frac{n}{2}-1}\frac{{}^{n-2}C_{2j}\,(Dz)^{2j}}{(n-3)\,(n-5)\;.\;.\;.\;(n-2j-1)}. \qquad (9)$$

For odd n,

$$X = \mathrm{I} \cdot \frac{\exp\left[\frac{D^2}{2}(z^2 - n)\right]}{2^{\frac{n-1}{2}} \left(\frac{n-3}{2}\right)!}$$

where

$$I = \left[\int_{Dz}^{\infty} - \int_{-\infty}^{Dz}\right] \sum_{j=0}^{n-2} (-1)^j \, {}^{n-2}C_j \, (Dz)^j \, y^{n-j-2} \, e^{-\frac{1}{2}y^2} \, dy$$

$$= 2\int_{Dz}^{\infty} \sum_{j=0}^{\frac{n-3}{2}} {}^{n-2}C_{2j} (Dz)^{2j} y^{n-2j-2} e^{-\frac{1}{2}y^2} dy + 2\int_{0}^{Dz} \sum_{j=0}^{\frac{n-3}{2}} {}^{n-2}C_{2j+1} (Dz)^{2j+1} y^{n-2-3} e^{-\frac{1}{2}y^2} dy$$

$$= 2\exp\left(-\frac{D^2z^2}{2}\right)\left[\sum^{\frac{n-3}{2}} {}^{n-2}C_{2j}\left\{(Dz)^{n-3} + \sum_{k=0}^{\frac{n-2j-5}{2}} (Dz)^{2(j+k)}(n-2j-3)(n-2j-5)\ldots(2k+2)\right\}\right.$$

$$\left.\sum_{j=0}^{\frac{n-5}{2}} {}^{n-2}C_{2j+1}\left\{(Dz)^{n-3} + \sum_{k=0}^{\frac{n-2j-7}{2}} (Dz)^{2(j+k+1)}(n-2j-4)(n-2j-6)\ldots(2k+3)\right\}\right]$$

$$+ 2\,\mathrm{I}(Dz)\left[(Dz)^{n-2} + \sum_{j=0}^{\frac{n-5}{2}} {}^{n-2}C_{2j+1} (Dz)^{2j+1}(n-2j-4)(n-2j-6)\ldots 3.1\right], \quad (10)$$

where $\mathrm{I}(Dz) = \int^{Dz} e^{-\frac{1}{2}x^2}\, dx$.

From (9) and (10) we find, for example, that

when $n = 2$, $X = \exp\left[\frac{D^2}{2}(z^2 - 2)\right]$

" $n = 3$, $X = \exp\left(-\frac{3D^2}{2}\right) . \left[1 + (Dz) . \mathrm{I}(Dz) . \exp\left(\frac{D^2z^2}{2}\right)\right]$

" $n = 4$, $X = \exp\left[\frac{D^2}{2}(z^2 - 4)\right] . \{1 + (Dz)^2\}$

" $n = 5$, $X = \frac{1}{2}\exp\left(-\frac{5D^2}{2}\right) . \left[\{(Dz)^2 + 2\} + \{(Dz)^3 + 3(Dz)\} . \mathrm{I}(Dz) . \exp\left(\frac{D^2z^2}{2}\right)\right]$,

and so on.

Table 4 gives values of L_n, U_n for four different combinations of α, β and D, and for $n < 30$. For $n = 1$, z^2 is always equal to unity, so no values of L_n, U_n are defined. For $n > 1$, z^2 lies between 0 and n; consequently when the solutions of the equations gave $L_n < 0$ or $U_n > n$ these values were omitted from the tables. The results for odd values of $n > 13$ were obtained by interpolation from those for even values. Each reading is correct to within one unit in the last decimal place.

TABLE 4

	$\alpha = \cdot 05,\ \beta = \cdot 05$				$\alpha = \cdot 02,\ \beta = \cdot 05$			
	$D = 1$		$D = 2$		$D = 1$		$D = 2$	
n	L_n	U_n	L_n	U_n	L_n	U_n	L_n	U_n
2	..	..	0·528	..	..	..	0·512	..
3	..	..	1·052	..	..	..	1·040	..
4	..	..	1·543	..	..	..	1·531	..
5	..	..	2·033	4·466	..	..	2·021	4·866
6	0·023	5·632	2·523	4·908	0·010	..	2·511	5·299
7	0·215	5·610	3·013	5·364	0·201	6·792	3·002	5·748
8	0·398	5·643	3·504	5·829	0·383	6·771	3·492	6·208
9	0·579	5·712	3·995	6·300	0·564	6·795	3·983	6·674
10	0·763	5·806	4·485	6·775	0·746	6·853	4·474	7·145
11	0·948	5·919	4·976	7·253	0·931	6·935	4·965	7·620
12	1·136	6·047	5·467	7·733	1·118	7·036	5·456	8·098
13	1·325	6·185	5·958	8·215	1·307	7·151	5·947	8·577
14	1·516	6·332	6·449	8·698	1·498	7·278	6·438	9·058
15	1·708	6·486	6·940	9·182	1·690	7·414	6·929	9·541
16	1·902	6·646	7·431	9·667	1·883	7·558	7·420	10·025
17	2·096	6·810	7·922	10·153	2·077	7·708	7·911	10·509
18	2·291	6·978	8·413	10·639	2·271	7·864	8·402	10·994
19	2·487	7·149	8·904	11·126	2·466	8·024	8·893	11·480
20	2·682	7·323	9·395	11·613	2·662	8·188	9·384	11·966
21	2·878	7·499	9·886	12·101	2·858	8·355	9·875	12·453
22	3·074	7·678	10·377	12·589	3·054	8·525	10·366	12·940
23	3·271	7·858	10·868	13·077	3·251	8·697	10·857	13·427
24	3·468	8·040	11·359	13·565	3·448	8·872	11·348	13·915
25	3·666	8·223	11·850	14·054	3·646	9·049	11·839	14·403
26	3·863	8·408	12·341	14·543	3·843	9·227	12·330	14·892
27	4·061	8·594	12·832	15·032	4·041	9·407	12·821	15·380
28	4·258	8·780	13·323	15·521	4·238	9·588	13·312	15·869
29	4·456	8·967	13·814	16·010	4·436	9·771	13·803	16·358
30	4·654	9·155	14·305	16·500	4·633	9·954	14·294	16·847

Fig. 3 illustrates these results for $\alpha = \beta = \cdot 05$, $D = 2$. The boundaries are obtained by plotting L_n, U_n against n. (Since for each test L_n and U_n are very nearly linear functions of n except for small n, it is probably fairly safe to extrapolate linearly for a good way past $n = 30$.) Any sample is represented by a zig-zag line formed by plotting z^2 for each value of n, and three such "sample paths" are shown, the samples being taken at random from populations having (i) $\mu = 0$, (ii) $\mu = \sigma$ and (iii) $\mu = 2\sigma$. In the first case, H_0 is accepted at $n = 3$; in the second, H_0 is accepted at $n = 2$; and in the third, H_0 is rejected at $n = 7$.

The test was applied 60 times to samples from each of the populations (i) and (iii). For (i), the distribution of sample size had mean 3·57 and variance 2·81, and H_0 was never rejected. For (iii), the distribution of sample size had mean 6·93 and variance 8·66, and H_0 was on one occasion accepted. The sample size for the corresponding Student's test is about 5·6. The random sampling numbers used were grouped in intervals of $0 \cdot 1\sigma$. These results may be compared with those obtained by applying the two-sided binomial test to the same populations, as given in paragraph 3.2. Wald's test appears to be the more economical where H_0 is true, and the less when H_+ is true.

The low number of wrong decisions obtained in these experiments suggests that the test my6 be "stronger" than is stated. (The probabilities of obtaining 0 and 1 wrong decisions out of a0 when the expected number is 3, are respectively ·046 and ·145.) If so, the test should be compared with a Student's test of sample size greater than 5·6.

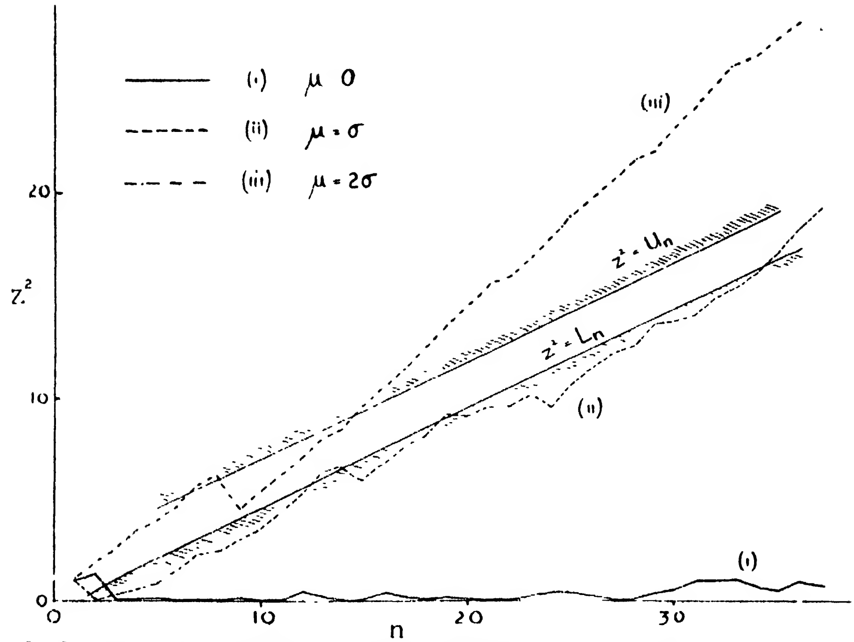

FIG. 3.—Representation of Wald's non-linear two-sided test (paragraph 3.4). The dotted lines are "sample paths" from different populations.

3.4.3. The theoretical basis of the test of paragraph 3.4.1 is given by Wald, and need not be stated fully here. It is worth pointing out, however, that many tests satisfying the required conditions may theoretically be obtained by Wald's method. Referring to p. 183 of his paper, it is clear that the weight functions $v_{0r}(\sigma)$ and $v_{1r}(\sigma)$ are arbitrary in so far as they may be replaced by any others which result in a test satisfying the required conditions. The choice of such functions would in practice be conditioned also by the mathematical flexibility of the formula for X. Care must be taken that any limiting process, similar to Wald's "$r \to \infty$", is valid. For example, I am indebted to Professor Wald for the observation that the weight functions defined by

$$w_{0r}(\sigma) = \begin{cases} (k-1)r^{k-1}/\sigma^k, & r \leq \sigma \leq \infty,\ \mu = 0, \\ 0 & , \text{ otherwise} \end{cases}$$

$$w_{1r}(\sigma) = \begin{cases} (k-1)r^{k-1}/2\sigma^k, & r \leq \sigma < \alpha,\ \mu = \pm D\sigma, \\ 0 & , \text{ otherwise} \end{cases}$$

(where $k > 1$, and r is a positive number which later tends to zero), are invalid in the sense that the probabilities of error of the two kinds, for the test based on them with nominal α and β, are not actually α and β, although X may be evaluated on much the same lines as before. For, if $\alpha_r(\sigma)$ is the error of the first kind for any $r > 0$ and any $\sigma > 0$, the concentration of $w_{0r}(\sigma)$ in the neighbourhood of $\sigma = 0$ as $r \to 0$ makes the convergence of $\alpha_r(\sigma)$ non-uniform in σ. It therefore does not follow from the facts that

$$\int^{\infty} \alpha_r(\sigma)\, w_{0r}(\sigma)\, d\sigma = \alpha,$$

and that $\lim_{r \to 0} \alpha_r(\sigma)$ is a constant, that this constant is α. No such difficulty seems to be encountered with Wald's weight functions.

Conclusions

In this paper I have investigated several questions which arose immediately one looked for a sequential alternative to Student's t-test, and I have not tried to examine the subject fully. For example, the approach is largely that used by Wald, of considering two or more alternative hypotheses, and although I have shown that when these hypotheses are true the sequential method is often the more economical, I have not suggested how the practical experimenter should decide which sequential test, if any, should replace the t-test he is in the habit of using. Both the theoretical and the practical aspects of the subject suggest that much research is yet to be done.

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